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RICHARD FEYNMAN IN BRAZIL: RECOLLECTIONS

by

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RIO DE JANEIRO

1988

It was in 1949 that Richard Feynman came to Rio de Janeiro for the first time. As he went to Princeton to give Seminars he met Jayme Tiomno and Walter Schützer, two graduate students from Brazil who were working with Professor John Wheeler. As Tiomno learnt that Feynman was studying Spanish to go to South America, he suggested that he come instead to Rio where Cesar Lattes and I had just founded the Brazilian Center for Research in Physics (CBPF). To help in his study of Portuguese Tiomno gave Feynman a paper I had written in this language on the theory of nuclear forces in 1948.

Feynman - and also Cecile Morette, later Mrs. De Witt - visited us that year.

In 1951, a new building was inaugurated for the CBPF at the university Campus. I had invited Tiomno, back from Princeton, and Guido Beck, then in Argentina, to join our theoretical group.

Lattes was working to establish a nuclear emulsion laboratory; cooperation with the Chacaltaya Laboratory in Bolivia at 5000 meters altitude was reactivated - after Lattes had exposed there, in coming from Bristol, the plates which detected for the first time pions and pion -muon decay¹ in cosmic radiation in 1947.

In 1951, I invited Feynman to come again to the CBPF. It was the year in which he left Cornell to work at Caltech and he took his sabbatical year 1951 - 1952 to come to Rio, before going to Caltech. He spent ten months with us. He enjoyed the beaches and the whole atmosphere in Rio, he also liked the institute we were trying to build up and was apparently happy to cooperate with us. This is quite clear in the letters he wrote me along the following years which are reproduced in this notice.

In 1951, he was working on meson theory after his well-known success in quantum electrodynamics. He proposed to me that we investigate whether the pseudoscalar meson theory could give some results in the description of the deuteron which could be experimentally checked in spite of the difficulty of the $\frac{1}{r^3}$ - singularity at the origin, of the Yukawa potential.

The results of this research are in a paper published in the Proceedings of a Symposium² which was held in Rio in 1952, reproduced in the Appendix.

To this Symposium there came a number of physicists from the U.S., Europe and Latin America among them Isidor Rabi, Eugene P. Wigner, Herbert Anderson, Sergio de Benedetti, R.G. Herb, Emilio Segrè, Martin Deutsch, D.W. Kerst, John and Leona Marshall, from the U.S., Carl von Weizsäcker, Reinhard Oehme, Gert Molière, Wilhelm Macke, from Europe, Manuel Sandoval Vallarta, Marcos Moshinsky, Fernando de Alba, from Mexico, Ricardo Gans from Argentina.

During his stay in 1951-1952 in Brazil, Feynman had the occasion to discuss with David Bohm on his hidden variables model of quantum mechanics. This contrasted sharply with some of the physicists in that Symposium who were hostile to Bohm when he presented his paper - Bohm's model was regarded by some of his eminent opponents as an attempt to introduce an interpretation of quantum mechanics based on dialectical materialism. Those were the years when maccarthysm was beginning to dominate the political scene in the United States and Bohm left the U.S. to take a position in Brazil at the University of São Paulo. Tiomno and I had met Bohm earlier in Princeton and suggested to him that he come to Brazil. Later, as the U.S. Embassy took his passport away, Lattes

obtained a Brazilian document for him through the President of the CBPF, who had been an Ambassador from Brazil in some countries. Bohm and Feynman interacted also during the meeting of the Brazilian Society for the Progress of Science (SBPC) in the city of Belo Horizonte, in 1952. The horror of Feynman to artificial ceremonies and his human qualities were put in evidence during this meeting. In one of the excursions outside Belo Horizonte Feynman had the occasion to see a number of poor children in slums. His revolt was expressed spontaneously when authorities entered solemnly to begin the Conference dinner. At the sound of music which announced the arrival of these authorities Feynman could not resist a protest by openly leaving the ceremony. We joined him later in a restaurant where he was relaxed and beautifully playing with his hands on the table as if it were a drum.

During this period Feynman gave lectures at the CBPF on meson theory. At the University he gave a series of lectures on electromagnetism. Tiomno, Elisa Frota Pessoa and I discussed with Feynman on the structure of the educational system in Brazil. In particular, we showed him some of the Brazilian books on physics for high school students. Feynman then gave a lecture at the University, at our suggestion, to extensively criticize these books. We were at that time trying to introduce a reform in the physics curriculum at the University; Tiomno and I started to translate and adapt to Portuguese an American book, High School Physics, by O.H. Blackwood, W.B. Herren and W.C. Kelly, which was published later.

In February 1952, Feynman took part in the Carnival festivities in Rio. He had met people of a samba organization (they are called "samba schools") and started to learn with them

how to play certain instruments - this organisation was called "The Fakers of Copacabana" and he actually took part in their exhibition wearing a Carnival costume. A photograph of Feynman disguised as Mephistophilis in a Carnival ball at the Municipal Theatre of Rio de Janeiro appeared in the weekly magazine "O Cruzeiro" dated 15th March 1952, page 49, Rio de Janeiro.

The Symposium on New Research Techniques in Physics was held from July 15 to July 29 in Rio and São Paulo and as Feynman left on June 1952 to go to Japan I presented our paper - that is why my name was written before that of Feynman in the publication.

In 1953, I invited Feynman to come again to the CBPF for the Summer term. This time he came with his wife, Mary Lou, whom he had recently married. He came then to the Laboratory fully dressed with necktie and coat, which allowed us to guess when Mary Lou left before him (via Bolívia and Peru) - from that day on Feynman came to the laboratory without necktie and jacket.

It was during this period that Feynman started formulating his theory of superfluidity in Helium (see his letter to me dated November 5, 1953).

In 1953, UNESCO sent a mission of physicists to the CBPF for a year, composed of Guiseppe Occhialini, Ugo Camerini, Gert Molière and Han Joos. Two students from Buenos Aires who had concluded their undergraduate courses, Daniel Amati and Alberto Sirlin, applied for a fellowship to the CBPF and came to Rio. They were among those who profited the most from the activities at the CBPF and from Feynman's lectures and seminars.

In 1956 Feynman invited me to come to Caltech. That was the

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year when we investigated the muon capture process by hydrogen and light nuclei, by comparing the direct Fermi interaction predictions with those deduced from the coupling via pions. At that time, a precise study of this question was possible in view of the Chew model for the proton. In this paper³, the first calculation on the induced weak pseudoscalar coupling was presented based in perturbation theory, a result taken up immediately afterwards by Lincoln Wolfenstein⁴.

In 1954, a serious political crisis which led to the suicide of President Getulio Vargas in Brazil affected the National Research Council and the atomic energy program adopted by this organisation. In particular at our laboratory there was a divergence of views on this crisis. Feynman's letter to me dated February 15, 1956 is a reply to a letter of mine and shows his great interest in the preservation of our Center, which survived that crisis.

Back from Caltech, as I read Feynman and Gell-Mann's paper on the V-A weak interaction I was led to publish a paper in which I presented the value of the W-boson mass by assuming $e=g$ - this seemed plausible to me in view of the fact that the vector character of photons and weak bosons pointed to a deep relationship between these particles which would reveal itself in the equal strength of their interactions with matter. I also discussed the possibility⁵ of the existence of the neutral vector boson by proposing a study of the elastic electron-neutron scattering (neutrino beams were unthinkable at a time, 1958, when the distinction of the muon-neutrino from the electron-neutrino was not yet established).

In 1959, the library of CBPF was destroyed by fire. I wrote to Feynman and to other colleagues about it. His wonderful reaction was expressed in a circular letter to American physicists dated June 3, 1959 where he calls for donations of books and reviews - he sent us his personal collection of The Physical Review and other journals and books - see his letter of June 3, June 22 and October 19, 1959.

After the creation of the Latin American School of Physics by the initiative of Marcos Moshinsky, of the National Autonomous University of Mexico, Juan José Giambiagi, then at the National University of Buenos Aires (now at CBPF) and myself, I organised the 5th School at the CBPF and thought of inviting Feynman as one of the lecturers. His letter of April 30, 1963 discusses the subjects of his choice - we finally decided on lectures on Solid State Physics which was then being developed in Latin America. This time he came with his wife, Gweneth, and his son, Carl.

In 1964, there was a coup d'état in Brazil and the installation of a military regime. That was a time of political persecutions and commissions of military investigation were established in most universities and research institutes throughout Brazil. As I left the Direction of the CBPF, which I had occupied since 1960, I went to the University of Paris at Orsay, at the invitation of Maurice Lévy. I came back to Rio in 1967 but two years later hundreds of university professors were fired among them Mario Schönberg, a well-known physicist from the University of São Paulo, Jayme Tiomno and Elisa Frota Pessoa and my wife, Maria Laura, a mathematician, and myself from the National University at Rio. In 1969 I left for Carnegie-Mellon University,

in Pittsburgh, at the invitation of Lincoln Wolfestein and Sergio de Benedetti. In 1970, I decided to accept an invitation from the University of Strasbourg to take a chair of Nuclear and High Energy Physics and stayed there until 1986.

In 1972, I met Feynman again, this time in Mexico City where we were invited as lecturers at a Summer School at the National Polytechnic Institute organised by Feliciano Sanchez Sinencio, a former graduate student at CBPF. We discussed physics, he asked me what I was doing and told me he was skeptical of the electroweak model which developed from the Steven Weinberg's paper of 1967. The reason of his scepticism was that the strong interactions did not seem at that time to be describable by field theory and in his view the unification attempts would have to consider the possibility of being extended to the four known interactions. We went together to visit several of the monuments of the beautiful pre-colombian art and culture. In Mexico, Feynman gave an interview to the press where he spoke against the military dictatorship, an unusual sign of friendship for he wanted to be always free from political participation.

The last time I saw Feynman was in 1977 when I invited him to come to Strasbourg. The High Energy Division of the Nuclear Research Center of that city, of which I was then Director, promoted an International Symposium on Multiparticle Dynamics and we wanted to have him to present a review paper on the parton model and its development. He came by car with colleagues from Bielefeld, West Germany and was interested in knowing the beautiful Alsace region. The Symposium was held in the small and charming town of Kaysersberg, birthplace of Albert Schweitzer.

The first news of Feynman's illness I had at the Tokyo International Conference on High Energy Physics in July 1978 where he was supposed to give an invited lecture.

The image I keep of Feynman is one of an intense joy in doing research, in thinking, in learning and finding by himself in his own inimitable fashion, a singular intuition, a rich imagination which has given so many beautiful gifts to physical knowledge.

With the years, he developed a deep epistemological grasp of physics exhibited for instance in his book on quantum mechanics, the third volume of the Feynman's Lectures on Physics, in his lectures contained in The Character of Physical Laws. In his visit to Brazil in the year 1959 he presented a paper to the Brazilian Academy of Sciences on original research he had carried on biology - when the time came for his sabbatical year 1958-1959 instead of leaving Pasadena he preferred to stay at Caltech and take instead a trip to another domain of science, to biology. This Academy had already received him in his preceding sabbatical year in Rio; as a courtesy to visiting foreign scientists the members of the Academy used to make the oral presentation of their papers in English. A fact which Feynman jokingly presents in his book "Surely you're joking, Mr. Feynman" as if his presentation in his Portuguese had incited the Academy to finally adopt this language in their meetings... Would that be true, *mutatis mutandis*, for Japan, which Feynman also visited?

The Chapter of this book on his Brazilian experience is a series of jokes - it can only be understood by its title. His students in Rio were among the best we ever had, among them Amati (now at CERN), Sirlin (now at New York University), the Brazilians Samuel Mac Dowell

(now at Yale University), Herch Moysés Nussensveig (now at the Rio Catholic University-PUC), Fernando Souza Barros (at the Federal University of Rio de Janeiro), Ricardo Palmeira (now at NASA) Erasmo Ferreira (at the PUC) and so on. His letters in the present article prove on the contrary, how he loved that experience and our institute.

He was always happy, energetic, enthusiastic and witty.

On the 29th June 1953, Robert Oppenheimer and his wife arrived by boat to Rio de Janeiro at the invitation of the President of the Brazilian National Research Council to lecture at CBPF. As Feynman and I went to the harbor to greet Oppenheimer, late in the afternoon, there were fire-crackers which filled the sky. It was Saint Peter's day which is celebrated, as in Saint John's day on the 24th June, this way. This led Feynman to tell me: "I only hope that Oppie does not think that these firecrackers are in commemoration of his arrival" and we both laughed loudly. Our great esteem for Oppenheimer allowed us to enjoy this joke which emphasized the importance he had assumed for the world as the Director of the Manhattan Project at Los Alamos.

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1. C.M.G.Lattes, H.Muirhead, G.P.S. Occhialini and C.F.Powell, Processes involving charged mesons Nature 159, 694 (1947);
 2. J.Leite Lopes and R.P.Feynman, On the pseudoscalar meson theory of the deuteron, Symposium on New Research Techniques, Acad. Brasil Ci, 251 (1954)
 3. J.Leite Lopes, Capture of negative mesons by light nuclei, Phys. Rev. 109, 509 (1958)

4. L.Wolfenstein, Virtual pion effect in meson capture,
Nuovo Cimento 8, 882 (1958)
5. J.Leite Lopes, A model of the universal Fermi interaction, Nucl.
Phys. 8, 234 (1958).

On the Pseudoscalar Meson Theory of the Deuteron*

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We report on an attempt to find a mathematically valid consequence of the YUKAWA pseudoscalar meson theory which disagrees with experiment. As is well known, the symmetrical theory with gradient coupling predicts in first order a nuclear potential:

$$V = g^2 \mu (\tau_1, \tau_2) \left\{ \frac{1}{3} (\sigma_1, \sigma_2) + S_{12} \left(\frac{1}{3} + \frac{1}{x} + \frac{1}{x^2} \right) \right\} \frac{e^{-x}}{x} \quad (1)$$

where $x = \mu r$ and μ is the reciprocal COMPTON wave-length of the π -meson; S_{12} is the tensor force. But this is not a true mathematical consequence of the theory because (a) the nucleons are treated non-relativistically, and (b) only the term of first order in g^2 has been included. Nevertheless, at first sight we might expect that a complete mathematical theory would not essentially alter the potential (1) outside some small value of x . The relativistic corrections ought to come at the COMPTON nuclear wave length $x \sim 0.15$ and should be small for x large relative to this. The corrections of order g^4 come from the exchange of two mesons. They represent intermediate states of energy 2μ and produce exponentials like e^{-2x} . Hence they might be expected to have effects at $x \sim 0.5$ and here only of order g^2 or 20% of the first order term (1) (however, see below).

We have tried to see whether the present data on $n - p$ forces (the deuteron properties in particular) could be consistent with potential (1) outside some small $x = x_0$. What the potential is inside of this rough limit x_0 we do not know. The procedure is illustrated by imagining for a moment we were dealing only with a central potential, which is the case of (1) for the singlet state:

$$V_S = -g^2 \mu \frac{e^{-x}}{x} \quad (2)$$

We solve the wave equation for $u = x\psi$ for S -wave scattering for all x in this potential, but do impose the usual boundary condition that u vanishes at the origin. Starting from the outside and proceeding inward this ought to give the correct wave function at least until x_0 — that is over the greater part of the interaction range. At x_0 it should be fitted in slope and value to a completely unknown function coming from inside x_0 . But the solution from the inside is probably energy insensitive until momenta of order x_0^{-1} are involved. Assuming the slope to value unknown but constant at x_0 would permit thereby a determination of the scattering cross-section for much higher energies than the present approximations

*Reprinted from "Symposium on New Research Techniques in Physics", July 15-29, 1952.

of the theory of effective range and scattering length would permit. Instead of choosing an x_0 and giving the slope there, since there is really only one unknown constant, we have for convenience carried the solution all the way to $x = 0$. The behavior of the solution extrapolated in this manner to the origin is a new effective constant to describe the inadequacy of the potential (2) for short distances. One expects such things as the scattering at energies below momenta x_0^{-1} and properties depending on the wave function not too close to the origin to be correctly described by (2) with its new boundary condition. Actually, we have two accurate data on the singlet interaction, the scattering length and the effective range. The first determines the asymptotic behavior $1 - x/a_0$ of the solution as $x \rightarrow \infty$. We used it, $a_0^{-1} = -0.06$, to start the numerical solution of the radial wave equation from the outside. The effective range r_0 depends on an integral, $\int_0^\infty dx [(1 - x/a_0)^2 - u_0^2]$

where u_0 is the zero energy u , but we needed the experimental value $r_0 = 2.76 \times 10^{-13}$ cm. to determine the as yet unknown g^2 . By trial and error we determined $g^2 = 0.18$. The wave function at the origin behaved like a series involving BESSEL functions. We did not go on to calculate phase shifts from potential (2) and boundary conditions for higher energies but turned instead to the triplet state potential. If χ and φ represent the S and D parts of the wave function

$$x\psi = \chi + \frac{\sqrt{2}}{4} S_{12} \varphi$$

the SCHRÖDINGER equation becomes for the deuteron ground state:

$$\ddot{\chi} = A \chi - \sqrt{2} B \varphi; \quad \ddot{\varphi} = \left(A + B + \frac{6}{x^2} \right) \varphi - \sqrt{2} B \chi \quad (3)$$

where
$$A = \epsilon^2 - C \frac{e^{-x}}{x}, \quad \epsilon^2 = -\frac{ME}{\mu^2}, \quad C = \frac{M}{\mu} g^2;$$

$$B = 6 C e^{-x} \left(\frac{1}{x^3} + \frac{1}{x^2} + \frac{1}{3x} \right)$$

using the g^2 value obtained from the singlet state. Of course these equations have a singularity, but although we shall carry the solution to the origin, we do not imagine (3) to be valid below some x_0 . The equations have an explosive exponential solution (of the form $k \exp(\alpha/\sqrt{x})$) near the origin. This exponential rises on decreasing x so soon and so rapidly that for appreciable k it does not seem possible that any modification of the potential for small x could bring the solution within

a reasonable size again. Therefore, we have assumed that our solution has as x tends to zero an asymptotic form with $k = 0$. It varies then as $\cos(\alpha/\sqrt{x} + \lambda)$. This boundary condition has been suggested by VON NEUMANN and PAULI and discussed by CASE (1950). We consider λ as another unknown constant whose value depends on the true potential. We used binding energy of the deuteron as the experimental quantity determining this constant. With the solutions obtained in this way numerically we have three quantities which we can compare to experiment. (Table I).

TABLE I

QUANTITY		EXPERIMENT	THEORY
Quadrupole moment	$\int_0^\infty (x\varphi - 0.353 \varphi^2) x^2 dx$	$2.73 \times 10^{-27} \text{ cm}^2$	$3.53 \times 10^{-27} \text{ cm}^2$
Effective range	$\frac{1}{2} - \int_0^\infty (x^2 + \varphi^2) dx$	$1.65 \times 10^{-13} \text{ cm}$	$2.32 \times 10^{-13} \text{ cm}$
% D state	$\int_0^\infty \varphi^2 dx / \int_0^\infty (\varphi^2 + x^2) dx$	$4 \pm 2 \%$	10.2 %

The second column tells how these quantities depend on the properties of the ground state wave function. The last two show a serious disagreement between theory and experiment. There is too much tensor force. Estimates show that the potential must be wrong by its own order of magnitude at least as far out as $x = 0.7$.

If the argument about the validity of (1) for x large given above were valid, we would have here evidence that the YUKAWA pseudoscalar theory with gradient coupling would not give correct results even if the potential were calculated accurately. Unfortunately the argument about the g^4 corrections is not valid. A direct calculation of the g^4 static correction by TAKETANI, MACHIDA & ONUMA (1951) (and confirmed independently by us) shows that large combinatorial coefficients come in so that the g^4 order corrections are larger than the g^2 potential (1) for $g^2 = 0.2$ even as far out as $x = 1$. Therefore, the potential (1) has neither theoretical nor experimental justification. This conclusion has also been obtained much earlier by TAKETANI, NAKAMURA & SASAKI (1951) in much the same way.

It is unlikely that any calculation by a perturbation expansion (for example, using the diagrams and methods of electrodynamics) has value for nucleon- π -meson problems. At least such a calculation should be accompanied with a good argument establishing its validity.

Incidentally, the g^4 potential was carried out to first order in v/c as well. It shows a spin-orbit force too large but of the right sign for the nuclear shell model of HAXEL, JENSEN and SUESS and of MAYER. It arises from the fact that the non-relativistic nucleon-meson coupling (σ, q) — where q is the meson momentum — must, for Galilean invariance, be replaced by $(\sigma, q - p E/M)$ where E is the meson energy, p the average momentum before and after interaction, and M the proton mass. A velocity v increases q by Ev and p by Mv so the term is now Galilean invariant. Hence, we apparently cannot conclude from the existence of the spin-orbit force that the pi-meson theory of nuclear forces is incorrect.

BIBLIOGRAPHY

CASE (1950), *Phys. Rev.*, **80**, 797.

TAKETANI, MACHIDA & ONUMA (1951), *Progr. Theor. Phys.*, **6**, 63S.

TAKETANI, NAKAMURA & SASAKI (1951), *Progr. Theor. Phys.*, **6**, 581.

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February 15, 1954

Dear Dick:

I am glad to know from your cable that you accept coming to Rio next Summer.

I have asked the Brazilian Research Council funds for our courses and am trying to obtain help from Pan American Union (Latin fellowships), UNESCO and U.S. National Science Foundation. This Foundation has a representative in Rio. He told me that if you apply to the NSF in Washington they will sponsor financially your trip to Rio.

I am therefore sending you the enclosed invitation in case you need it to send to the NSF. The financial aid of the NSF would be a great help to us, since we all would very much like to have you again in Rio. As usual, we have financial difficulties. But we have to fight and try to progress.

If things go well, I would like, at your early convenience, to have the title of your lectures and perhaps a very short program.

All best wishes

Leite

Um abraço do

Carto 2

Chicago, Illinois

January 18, 1952

Dr. R.P. Feynman
 Miramar Palace Hotel
 Copacabana
 Rio de Janeiro, Brazil

Dear Dick:

I was very much interested in your letter of December 19, especially so because we have been thinking here along lines that are somewhat related to yours. We have some experimental results that seem to indicate the importance of the conservation of the resultant isotopic spin in the problem of scattering of pions by hydrogen. I am enclosing some letters to the Physical Review on this subject.

I have consulted my "experts"* on your assumption that the quantities X_1, X_2 , etc. are real. As you may expect, they from on such assumptions, as experts would. I believe, however, that they are right and that the quantities are real only when they are small, but that phase differences develop when the cross section becomes comparable to λ^2 . For this reason I believe that your formulas need some changes at energies where this is the case. An approach to essentially the same discussion that seems to me above suspicion is the following.

Let's assume (a) that the resultant isotopic spin is a good quantum number; (b) that the scattering, at least in a certain energy interval, is due to p-waves only. In the scattering of pions of any charge by nucleons there are involved only states of isotopic $1/2$ and $3/2$. If we restrict ourselves to p-waves only, the true spin can have only the values $1/2$ and $3/2$. There are, therefore, only four combinations of isotopic and true spin

*In the special instance expert = Goldberger.

$$\sigma_7(0)/\lambda^2 = 4\alpha_{33}^2 + \alpha_{31}^2 + 4\alpha_{33}\alpha_{31} = (2\alpha_{33} + \alpha_{31})^2$$

$$\begin{aligned}\sigma(0)/\lambda^2 &= \frac{1}{9} [4\alpha_{33}^2 + \alpha_{31}^2 + 16\alpha_{13}^2 + 4\alpha_{11}^2 + 4\alpha_{33}\alpha_{31} \\ &\quad + 16\alpha_{33}\alpha_{13} + 8\alpha_{33}\alpha_{11} + 8\alpha_{31}\alpha_{13} + 4\alpha_{31}\alpha_{11} + 16\alpha_{13}\alpha_{11}] \\ &= \frac{1}{9} [2\alpha_{33} + \alpha_{31} + 4\alpha_{13} + 2\alpha_{11}]^2\end{aligned}$$

$$\begin{aligned}\sigma(0)/\lambda^2 &= \frac{2}{9} [4\alpha_{33}^2 - 8\alpha_{33}\alpha_{13} + 4\alpha_{13}^2 + \alpha_{31}^2 - 2\alpha_{31}\alpha_{11} + \alpha_{11}^2 \\ &\quad + 4\alpha_{33}\alpha_{11}] \\ &= \frac{2}{9} (2\alpha_{33} - 2\alpha_{13} + \alpha_{31} - \alpha_{11})^2\end{aligned}$$

at 0° zero angle

$$\sqrt{\sigma_7/\lambda^2} = 2\alpha_{33} + \alpha_{31}$$

$$\sqrt{\sigma_7/\lambda^2} = \frac{1}{3}(2\alpha_{33} + \alpha_{31}) + \frac{2}{3}(2\alpha_{13} + \alpha_{11})$$

$$\sqrt{\sigma_0/\lambda^2} = \frac{2}{3}(2\alpha_{33} + \alpha_{31}) - \frac{2}{3}(2\alpha_{13} + \alpha_{11})$$

checking me.

$$\sigma_7(90^\circ)/\lambda^2 = (\alpha_{33} - \alpha_{31})^2$$

$$\sigma_7(90^\circ)/\lambda^2 = \frac{1}{9} (\alpha_{33} - \alpha_{31} + 2(\alpha_{13} - \alpha_{11}))^2$$

$$\sigma_0(90^\circ)/\lambda^2 = \frac{4}{9} (\alpha_{33} - \alpha_{31} - \alpha_{13} + \alpha_{11})^2$$

at 90°

$$\sqrt{\sigma_7/\lambda^2} = \alpha_{33} - \alpha_{31}$$

$$\sqrt{\sigma_7/\lambda^2} = \frac{1}{3}(\alpha_{33} - \alpha_{31}) + \frac{2}{3}(\alpha_{13} - \alpha_{11})$$

$$\sqrt{\sigma_0/\lambda^2} = \frac{2}{3}(\alpha_{33} - \alpha_{31}) - \frac{2}{3}(\alpha_{13} - \alpha_{11})$$

checking again

that count, namely, $3/2-3/2$, $3/2-1/2$, $1/2-3/2$, and $1/2-1/2$. I shall abbreviate the notation by writing as indices the four combinations, 33 , 31 , 13 , 11 .

From the assumptions made it follows that at a given energy all the scattering phenomena are characterized by four real quantities, namely the four phase shifts α_{33} , α_{31} , α_{13} , and α_{11} for these four states. These four real quantities are, of course, functions of the energy. In a scattering measurement, on the other hand, it is possible, in principle, to measure for each energy the three scattering cross sections of π^+ on protons and π^- on protons, without and with exchange of charge. Furthermore, each cross section can be measured at 0° and 90° in the center-of-mass system so that this gives for each energy six measurable quantities that can be expressed in terms of four phase shifts only, whereby two verifiable conditions are left over. The formulas are a little bit complicated and, if I have made no mistake, they are the following. σ_+ , σ_- , σ_0 are the cross sections per steradian of the three processes. One finds

$$\sigma_+(0^\circ) = \lambda^2 \left\{ 4 \sin^2 \alpha_{33} + \sin^2 \alpha_{31} + 4 \sin \alpha_{33} \sin \alpha_{31} \cos(\alpha_{33} - \alpha_{31}) \right\}$$

$$\sigma_+(90^\circ) = \lambda^2 \left\{ \sin^2(\alpha_{33} - \alpha_{31}) \right\}$$

$$\begin{aligned} \sigma_-(0^\circ) = \frac{\lambda^2}{9} \left\{ 4 \sin^2 \alpha_{33} + \sin^2 \alpha_{31} + 16 \sin^2 \alpha_{13} + 4 \sin^2 \alpha_{11} + \right. \\ + 4 \sin \alpha_{33} \sin \alpha_{31} \cos(\alpha_{33} - \alpha_{31}) + 16 \sin \alpha_{33} \sin \alpha_{13} \cos(\alpha_{33} - \alpha_{13}) + \\ + 8 \sin \alpha_{33} \sin \alpha_{11} \cos(\alpha_{33} - \alpha_{11}) + 8 \sin \alpha_{31} \sin \alpha_{13} \cos(\alpha_{31} - \alpha_{13}) + \\ \left. + 4 \sin \alpha_{31} \sin \alpha_{11} \cos(\alpha_{31} - \alpha_{11}) + 16 \sin \alpha_{13} \sin \alpha_{11} \cos(\alpha_{13} - \alpha_{11}) \right\} \end{aligned}$$

$$\begin{aligned} \sigma_-(90^\circ) = \frac{\lambda^2}{9} \left\{ \sin^2(\alpha_{33} - \alpha_{31}) + 4 \sin^2(\alpha_{13} - \alpha_{11}) + \right. \\ \left. + \sin(\alpha_{33} - \alpha_{31}) \sin(\alpha_{13} - \alpha_{11}) \cos(\alpha_{33} + \alpha_{31} - \alpha_{13} - \alpha_{11}) \right\} \end{aligned}$$

$$\sigma_0(0^\circ) = \frac{2\lambda^2}{9} \left\{ 4 \sin^2(\alpha_{33} - \alpha_{13}) + \sin^2(\alpha_{31} - \alpha_{11}) + \right. \\ \left. + 4 \sin(\alpha_{33} - \alpha_{13}) \sin(\alpha_{31} - \alpha_{11}) \cos(\alpha_{33} + \alpha_{13} - \alpha_{31} - \alpha_{11}) \right\}$$

$$\sigma_0(90^\circ) = \frac{2\lambda^2}{9} \left\{ \sin^2(\alpha_{33} - \alpha_{31}) + \sin^2(\alpha_{13} - \alpha_{11}) - \right. \\ \left. - 2 \sin(\alpha_{33} - \alpha_{31}) \sin(\alpha_{13} - \alpha_{11}) \cos(\alpha_{33} + \alpha_{31} - \alpha_{13} - \alpha_{11}) \right\}$$

Concerning the possibility of checking experimentally similar formulas, I am afraid that this may prove very difficult. At low energies, as far as we know, the cross sections of the pions on hydrogen become very small and in addition the influence of the s-scattering, which is small but not negligible, is probably important. One might think, of course, to add to the previously considered phase shifts of four p-waves also the two additional phase shifts of the s-waves. At the same time the experimentally observable quantities would increase from six to nine because when s-waves also are scattered, interference between s- and p-waves would make it meaningful to measure for each event the cross section forward, backward and at 90° . Apart of the difficulties of doing this with usable accuracy, there is one theoretical difficulty that arises from the fact that the isotopic spin at low energy is certainly not too good a constant of motion because of the mass difference between the neutral and the charged pion, and also between the neutron and the proton. I have tried to see how this complication could be corrected for, and I think that it can be done, but I doubt that it is entirely unambiguous.

At higher energy, on the other hand, where the measurement would be more readily feasible, it is probable that higher angular momenta become

important. Unfortunately, the energy range that is accessible to our experimentation is not adequate to decide whether and where the d-waves become important.

Sincerely yours,

EF:cf

Enrico Fermi

P.S. I have had your letter duplicated and sent copies to the following:

Leisskopf
Steinberger
Serber
Wheeler
Oppenheimer
Yang
Bethe
Marshak
Wick
Brueckner
Chew
Christy
McMillan
Lepore

-21-

March 3, 1953.

Professor R.P. Feynman
Department of Physics
California Institute of Technology
Pasadena, Cal.
U.S.A.

Dear Dick:

As you know, you are a Permanent Professor at this Centro, with the special privilege to give your collaboration to a few American institutes each year. But we would like to invite you to come home again and stay a few months with us.

Please let us know whether you will come this Summer (June etc) so that the Conselho can take the necessary steps for your tickets (you and Mrs. Feynman).

Best wishes.

Um grande abraço

J. Leite Lopes

March 30, 1953.

Nº 188

Professor Richard F. Feynman
Norman Bridge Laboratory of Physics
California Institute of Technology
Pasadena, California.
U.S.A.

Dear Dick:

Thanks for your letter of March 13.

We shall pay here you and Mrs. Feynman's tickets for the route Los Angeles - Rio - Los Angeles. In order to go to Japan from here you have to return to your East Coast, so it will be easier and cheaper for you that you take care of the tickets for Tokyo (the dollar is now at almost 50 cruzeiros on the free market).

As soon as we buy the tickets we shall let you know. I assume that you will be here from about June 10 to Sept. 5.

I would appreciate it if you gave a course at the Centro on Quantum mechanics (for about 8 students with a reasonably good preparation for it).

If you agree to it we can also arrange that you give a few popular lectures on atoms or any other topics of your choice.

Um grande abraço

J. Leite Lopes

P.S. We shall try to buy the tickets by Braniff Airline which stops in Lima, Peru. While here you will receive Cr\$ 16.800,00 monthly.

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

NORMAN BRIDGE LABORATORY OF PHYSICS

November 5, 1953

Professor J. Leite Lopes
Centro Brasileiro de Pesquisas Fisicas
Av. Wenceslau Braz 71
Caixa Postal 46
Rio de Janeiro, Brasil

Dear Leite:

The magazine Kagaku says it will be perfectly all right if you publish the article "The Present Situation in Fundamental Theoretical Physics" in any language, anywhere. So you can tell the guys down there to go ahead. I think you have a copy. If not, tell me and I'll send one.

I had a pretty good time in Japan. I'm still working on the Helium. Nothing very new -- just the polishing, etc., needed for the new paper. I can deduce the main hydrodynamic and thermodynamic equations of the two fluid theory. I have as yet no clue as to what causes the critical velocity phenomena (viz. at low velocities there is no flow resistance in capillaries, but at higher speeds there is). Perhaps the film effect is simply van der Waals attraction plus easy flow. Other people have proposed this. Before I subscribe to it I must calculate the energies of the liquid in their films to see if there is anything special which tends to make the film extra thick, etc.

Some new data came out on neutron scattering in helium at 4.2°K from which $S(K)$ can be got, and thereby the energy curve. It comes out with a dip as expected, but the energy gap is equivalent to 19°K. Landau shows that to get agreement with experiment the gap should be 9.6°K, or only half as much. This is discouraging. Possible cause is lousy neutron data, or perhaps the 4.2°K instead of 0°K alters $S(K)$ a lot, or finally the wave function

$$\sum_i e^{i\mathbf{K} \cdot \vec{R}_i} \phi$$

is too simple and rough as a trial function so that, as is characteristic of the variational method, the energy comes out above the true energy. More complicated trial functions like

$$\sum_j \sum_i e^{i\mathbf{K} \cdot (\vec{R}_i + \vec{R}_j)/2} G(\vec{R}_i - \vec{R}_j) \phi$$

should be tried, with G determined for minimum, or just taken as some reasonable function, like gaussian. I haven't even estimated how much improvement I could get this way.

Give my regards to everyone. Eu tenho saudades do Brasil.

Sincerely,

Rick Feynman

R. P. Feynman

RPF:n

May 1955

Dear Leite,

Gosh, its good to hear you are in the U.S. so near, and yet unfortunately so far. Los Angeles is too far from N.Y. I won't be in N.Y. in the near future.

I guess, from your address that you are a delegate from Brazil for Atomic Energy. It is good that scientists, not politicians or admirals are sent this time.

How does everything go in teaching and work at the Center? Lat-tes told me all the details of the exposure up to the time he first left Brazil for the N.Y. Amer. Physical Society meeting. I don't know what happened since.

This year I am going to visit Japan. I'd like to talk physics with the guys there and live in the strange country-and Mary Lou wants to see all the artistic stuff, etc. there.

How's your painting?

My best to your wife and Sergio.

Heekahdow Phainmon

P.S. I got lots of reprints from that article on fundamental problems in theor. phys. you had published for me. There has been quite a demand for them. Thanks for taking care of that. Do you think, if I could write some articles on Bohm's Quantum Interpretation it would be good to publish them in Brazil. Do you guys need articles like that?

R.P. Feynman

Feb 15, 1956

Dear Leite,

I can think of no excuse for not writing sooner.

I was very interested in your detailed letter on the Centro. The only other word I had heard was a complete description from Lattes from his point of view a few days after the admiral was put out. It all saddens me very much. I too worked, a little time at least, to try to make science in Brazil, and in particular the Centro, strong. It is so hard to get started because with so few people personal problems and characteristics of individuals make so much difference. There are not enough people involved, or rather enough separates institutions involved that the thing is stable against statistical fluctuations. You can't go off and get a job at another Centro if the one you are working at is unsatisfactory. In a sense the time I spent in Brazil is a little wasted-but not really. The people we taught physics to still know it and the many things we discussed together are still a profit to both of us. Also no one can destroy the fun we had in the past - I can still play the pandeiro and speak a few words of Portuguese.

Now tell me, is anything new growing up there? Does peace reign again. What about your teaching program in the University? Is that interrupted? Are there any new students?

I'm sorry I was not in Brazil when all the excitement came off. At least we could have talked things a little. I remember so many less important but equally sad and fruitless messes.. Remember the Betatron-Van de Graaff argument in São Paulo? And the worries Bohm had about the administration, till he wrote to Einstein? Then there was the fight between the two German guys at these theoretical institute. But this now is far more serious.

We were very unhappy to hear that Carmita was sick. I hope she is much better now. Was it serious? Sergios Christmas

note shows that he is an artist too - but the message was sad.

You wrote that you want to spend some time away. If you want me to write any letters of recommendation, let me know. Can you ask the American Embassy about Fulbright Scholarship or Grants to the U.S.? From time to time various Universities in U.S. ask me if I know of someone for job. Should I tell them about you?

We enjoyed Japan very much. I worked hard-lots of talks, and discussions at many cities and Universities. I spent most of my time at Yukawa's place at Kyoto. Still I am trying to understand superconductivity. I worked very hard at it in Japan but couldn't solve it. Finally back here I ran out of ideas and ambition. I haven't been able to work on anything very much for the last 4 or 5 months. I'm in the doldrum. I guess it is psychological - perhaps it is the shock of discovering a problem I can't solve. I can't work on anything else because that's admitting failure, and I can't work on it because of the unadmitted failure. I haven't accomplished anything in the last year. I haven't even written those articles for you on the interpretation of quantum mechanic that I wanted to.

Mary Lou joins me in sending our best regards and in hoping Carmita and Sergio and You are all very well and you can write us and tell us some good news. Tenho saudades do Brasil, como nunca! Um abraço, Leite.

Ricardo Feynman

June 3, 1959

To: My Colleagues

From: Richard P. Feynman

As you know, I have always been interested in the development of physics in Brazil. I've been there four times, lecturing at their "Brazilian Center for Physical Research" - once staying 10 months. The "center" is in a small building of about 15 or 20 rooms, with a small library. This is where all the useful advanced work in Brazil is done, the most important physics professors do their research and advanced teaching there and their more elementary teaching at the Federal University.

Now they have had a calamity. I have a letter from my good friend (who spent a year visiting here, by the way) J. Leite Lopes. He writes:

"I am writing you under distress. The library - our nice little library - and the nuclear emulsion laboratory have been totally destroyed by fire.

Any help - reprints, preprints, lecture notes, books, journals - from our friends and colleagues for the reconstruction of the library and laboratory will be immensely appreciated".

I am sending them my collection of the PHYSICAL REVIEW (1940-59), REVIEWS OF MODERN PHYSICS, PROGRESS OF THEORETICAL PHYSICS (a Japanese journal) and THE PROCEEDINGS OF THE NATIONAL ACADEMY. But that isn't very much. I am trying to make up more complete sets of journals. Another big problem is books.

Do you have any items in physics or mathematics that you would like to send to help them? Or do you know of anyone who might have something useful? Please let me know if you do.

Thank you very much.

R. P. Feynman

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

MAN BRIDGE LABORATORY OF PHYSICS

June 22, 1959

Dr. Leite Lopes
Centro Brasileiro de Pesquisas Fisicas
Av. Wenceslau Braz, 71
Caixa Postal 46
Rio de Janeiro, Brasil

Dear Leite:

Everyone here is very anxious to help with your library. Many of my colleagues have contributed journals. Altogether we are prepared to send you the following journals, and a few books:

Physical Reviews, 1929-1959
Physica 1950-1955
American Journal of Physics, Jan. '52-May '52: Jan. '53-Sept. '54
Reviews of Scientific Instruments, Jan. 1946-Dec. 1957
Physics Abstracts, July 1938-Dec. 1951
Reviews of Modern Physics, 1940-1958
Progress of Theoretical Physics, 1946-59

Books

Smythe, STATIC AND DYNAMIC ELECTRICITY, 2nd Edition (2 copies)
Margenau and Murphy, THE MATHEMATICS OF PHYSICS AND CHEMISTRY
Panofsky and Phillips, CLASSICAL ELECTRICITY AND MAGNETISM
Lapp and Andrews, NUCLEAR RADIATION PHYSICS
Thomas, CALCULUS AND ANALYTIC GEOMETRY
Hill, ELECTRONICS IN ENGINEERING
Frank, INTRODUCTION TO ELECTRICITY AND OPTICS, 2nd Edition
Semat, ATOMIC PHYSICS
Huxley, WAVE GUIDES
Born, NATURAL PHILOSOPHY OF CAUSE AND CHANCE
Weisner, INTRODUCTION TO THE THEORY OF EQUATIONS
Kellog, FOUNDATIONS OF POTENTIAL THEORY
Perlis, THEORY OF MATRICES
Courant and Hilbert, METHODS OF MATHEMATICAL PHYSICS, Volume I
Kaplan, ADVANCED CALCULUS
Agnew, DIFFERENTIAL EQUATIONS
Courant, DIFFERENTIAL AND INTEGRAL CALCULUS, Volume II
Heisenberg, COSMIC RADIATION
Thorndike, MESONS
Wigner, GRAPPENTHEORIE UND IHRE ANWENDUNG AUF DIE QUANTENMECHANIK
DER ATMOSPEKTREN
Jackson, THE PHYSICS OF ELEMENTARY PARTICLES
HOAG, ON QUANTUM FIELD THEORIES

- M. Benedict and C. Williams, Engineering developments in the gaseous diffusion process, McGraw-Hill, 1949.
- L. Robin, Tables des fonctions de Legendre associées, Centre National d'Études des Telecommunications, 1952.
- J. C. P. Miller, Tables of Weber parabolic cylinder functions. National Physical Laboratory, 1955.
- N. Arley, Stochastic processes and cosmic radiation, Copenhagen, 1943.
- H. Bremmer, Terrestrial radio waves. Elsevir, 1949.
- L. Ford, Differential equations
- A. R. Forsyth, Differential equations
- F. B. Pidduck, Currents in aerials and high-frequency networks, Oxford University Press, 1946
- G. F. Tricomi, Funzioni ipergeometriche confluenti, Roma, 1954.
- Mathematical Tables and other Aids to Computation (periodical) incomplete set 1950-55.
- H. Buchholz, Die konfluente hypergeometrische Funktion. Springer, 1955.
-

Leite Lopes

Page 2

Bohr and Mattelson, COLLECTIVE AND INDIVIDUAL PARTICLE
ASPECTS OF NUCLEAR STRUCTURE
Bohr, ROTATIONAL STATES OF ATOMIC NUCLEI
ANNUAL REVIEW OF NUCLEAR SCIENCE, Vol. 7, 1957

They are all in my office, but before packing them and having them sent out I wanted to know whether to send all of these, or whether you have already gotten some of them. We don't want to ship stuff that you already have.

Do you know any cheap way to ship them by freight? Do you have any friends in the shipping business? Please answer right away.

Best of luck,

Dick
R. P. Feynman

RPF:n

P. S. More books just brought in:

Mott and Gurney, ELECTRONIC PROCESSES IN IONIC CRYSTALS
Wilson, THE THEORY OF METALS
Heitler, THE QUANTUM THEORY OF RADIATION
Frohlich, ELEKTRONENTHEORIE DER METALLE

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July 7, 1959

Professor R.P. Feynman
Norman Bridge Laboratory of Physics
California Institute of Technology
Pasadena, California

Dear Dick,

Our Very hearty thanks for the offer you and your colleagues at Caltech make to our Centro.

Enclosed is the list of books offered by Erdelyi which are of interest to us.


We are now trying to obtain that a Brazilian Air-line Company (Real-Aerovias) which flies to Los Angeles, transport the books and journals to Rio. In case they can not fetch the books in your laboratory I would ask you to order a Pasadena Company to pack the books and ship them to Los Angeles and send the bill to us that we shall send you a check.

So please wait a few days until we give you the final answer and the address to which you may send the books.

Our government and also UNESCO have promised financial help.

But the essential point is to ensure continuation our work. In this respect your offer was with with gratitude by my colleges and myself.

Best wishes.


Leite Lopes

Rio de Janeiro, September 9, 1959

Prof. R.P. Feynman
Norman Bridge Laboratory of Physics
California Institute of Technology
Pasadena, California, U.S.A.

Dear Dick,

I was disgusted to learn, on my return from Mexico, that our Administration, instead of asking Tiomno or Fialho to write you about the transportation of the books and journals, had sent you a letter in commercial style and in Portuguese. I am mad about it and am writing you to apologize. Unfortunately at that time I had to go to Mexico University for lectures while Lattes was also gone to the Kiev High Energy Conference. I had however left strong recommendations urging an early reply to you by Tiomno or Fialho or Beck as soon as they had the final word from the airline company. This company agrees to transport the books. We suggest that you hire a packing company to pack the books and send them to:

REAL - AEROVIAS AIRLINE
244 BISCAYNE BOULEVARD
MIAMI, FLORIDA

for delivery to:

CENTRO BRASILEIRO DE PESQUISAS FÍSICAS
AV. WENCESLAU BRAZ 71
RIO DE JANEIRO, BRAZIL

under the condition of freight to be paid in Rio.

Enclosed is a check of US\$100.00 for your expenses with it but I ask you to tell us if you are spending more than this amount.

Once more, thank you so much and apologies for the letter mix-up.

Best wishes and kind regards.



CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

NORMAN BRIDGE LABORATORY OF PHYSICS

October 19, 1959

Dr. Leite Lopes
Centro Brasileiro de Pesquisas Fisicas
Av. Venceslau Bras, 71
Rio de Janeiro, Brasil

Dear Leite:

We are shipping your books directly from Los Angeles to Rio de Janeiro on the SS MOREMACK SERVE, which sails on October 20 and it should arrive in two or three weeks. I am enclosing a list of the approximate contents. We didn't send them through Real-Aerovias Air-line because handling is so expensive in the United States that shipping to Miami, reloading and repacking would probably be even more expensive than to send them through to Rio de Janeiro by boat. I haven't found out how much it costs yet. If it is less than \$100, I will send you the excess; if more, I will make a donation to the Centro.

Don't worry, I didn't feel upset by any business sounding letters. The only thing that bothered me was that, until yours, none of the letters told me how to send them. Of course, I didn't send them the way you said, anyway, so that is the way things are.

I hope your library is getting back on its feet and everything is progressing in the usual slow and generally sure manner.

Sincerely yours,

R. P. Feynman
R. P. Feynman

RPF:n
Enc.

Regards to everyone - I got a call from the Ford foundation and told them the Centro was the most important place for physics in all South America - that you attracted students from most countries in S.A. and were "South Westernseminhemisphere renowned". Lets see what happens. Fortunately, I didn't have to lie.

I miss you all - I'll have to come and visit again sometime soon.

CENTRO BRASILEIRO DE PESQUISAS FÍSICAS

Caixa Postal

46

Avenida Wenceslau Braz, 71

Rio de Janeiro, DF — Brasil

End. Tel.:

BRASFISICAS

March 31, 1960

Professor Richard P. Feynman
Norman Bridge Laboratory of Physics
California Institute of Technology
Pasadena, California, U.S.A.

Dear Dick:

Your books and journals have arrived several weeks ago. Books and journals also keep coming in from Brookhaven, the Atomic Energy Commission and so on. We learned that you had kindly written to your colleagues asking for help to our library. I do not need to tell you that we are deeply grateful to you for all you have done for us. The Ford Foundation gave us a grant of \$100,000.00 for books and journals. Thus we are hopeful that we shall be able to reconstruct the library.

It was a pity we could not bring you this Summer. There was a chance to bring you by Fulbright Program but they wanted a positive reply at the time I phoned you. I am hopeful, however, that you will be able to come soon, perhaps next year. In this respect there is a possibility that the U.S. National Science Foundation could help us.

Enclosed you will find a program of a Summer School which will be held here before the Rochester Conference. Some time ago, I sent you a book *Introdução a teoria atomica da materia* which was published here and I hope you received it. I am sending you another one on high school physics which Tiomno and I have translated and adapted from an American edition for use here. Needless to say, this book has been attacked by the traditional teachers and professors here. But it has had a great success.

Work has been going on and last year I worked on the superselection rules, about which I have been maintaining correspondence with Wigner.

All of us are working with all drive we can to improve the situation of our Centro. And we all have saudades from you. I have received an invitation to go to the Rochester Conference and if I can find the money here, I hope to go and see you then.

With all best wishes,

Yours as ever,



No 388

Rio de Janeiro, November 21, 1962.

Professor R.P. Feynman
Physics Department
California Institute of Technology
Pasadena, California
U.S.A.

Dear Professor Feynman:

Physicists from several countries in Latin America have taken the initiative to hold a "Summer" School in Physics, every year, to take place alternatively in one of the cities of this part of the continent. The first session was held in Mexico City in 1959; the second one took place in Rio in 1960; the third session was in Buenos Aires in 1961 and the fourth one has just taken place this year in Mexico again.

I am in charge of organising the Latin American School of Physics in Rio de Janeiro next year. We thought of organising the course in the month of July through August 10, 1963, in our Centro.

The tentative plan consists of an experimental survey on elementary particles by R.A. Salmeron, a Brazilian physicist now at CERN and by a physicist from the Leprince-Ringuet group in Paris, probably A. Lagarrigue. Samuel Mac Dowell, of this Centro, will also be a lecturer.

I am writing you to invite you to give lectures on a subject of your choice.

The course should be on a graduate level and is intended to young physicists who have started working in theoretical physics. It may consist of 12 to 15 lectures.

We are planning to invite also a specialist to give a survey on the theory of many-particle systems.

I have asked the support of the Brazilian National Research Council. The prospects are that this Council will give a grant-in-aid of the order of \$ 10.000,00 daily to the lectures besides the air ticket.

I regret that the limited funds of this Centro do not allow us to offer a better contribution as a man like you deserves. I do hope, however, that your interest in our program and effort will make possible your participation in our School.

With all best wishes,

Sincerely yours


J. Leite Lopes
Scientific Director

No 112

Rio de Janeiro, April 22, 1963.

Professor Richard P. Feynman
Physics Department
California Institute of Technology
Pasadena, California
U.S.A.

Dear Dick:

Thank you for the telegram. The planning of the Latin American School of Physics has not been easy. It was decided to have lectures on both physics of elementary particles and on physics of systems of particles to attend the main interests of Latin American research groups. On the side of systems, Richard Prange will lecture on field theoretic methods applied to the theory of metals.

In high energy physics there will be lectures by A. Lagarrigue and R. Salmeron from Paris and CERN respectively on the experimental results on strange particles and resonances. Mac Dowell will give some lectures on the analytical behaviour of the scattering amplitude. So, we would like you to give lectures on a subject which you consider to be more interesting (three lectures a week from July 8 to August 10). You know what will be more attractive to you to lecture on and we know that your choice will be good.

My wife is already working to get an apartment in Copacabana and a maid for your family.

We all look forward to see you again in Brazil.

With all best wishes

Sincerely


J. Leite Lopes

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

NORMAN BRIDGE LABORATORY OF PHYSICS

April 30, 1963

Professor J. Leite Lopes
Centro Brasileiro de Pesquisas Fisicas
Av. Wenceslau Braz, 71
Caixa Postal 46
Rio de Janeiro, Brasil

Dear Leite:

It seems to me that the one subject you do not have well represented is the theory of high energy particles, strangeness rules, isotopic spin, etc. (assuming Mac Dowell takes up dispersion theory and Reggi Poles) perturbation theory, weak interaction theory, etc. So in the dozen or so lectures I could make a set called "Elementary Particle Theory" somewhat like a book of mine on "Theory of Fundamental Processes" but probably more advanced. For example, I would include the eightfold way, but put in less on quantum electrodynamics, and will assume previous knowledge of γ -matrices, etc. Is that OK and at the right level, or should we go down instead of up in level?

As usual, I suspect we should try to be more elementary, rather than more advanced - but we can decide that between us when we arrive.

Thank you very much.

I hope to hear about the apartment as soon as you know so my wife can stop worrying.

Looking forward to seeing you all soon.

Dick
R. P. Feynman

P.S. Another possibility you might prefer *if* lectures on phenomena in Solid State - something like Kittel's book but somewhat more advanced. Pick whatever suits you. I prefer, only slightly however, the elementary particle theory.

RPF:n

No 137

Rio de Janeiro, May 14, 1963.

Professor R.P. Feynman
Physics Department
California Institute of Technology
Pasadena, California
U.S.A.

Dear Dick:

Many thanks for your letter. We are choosing your lectures as Phenomena in Solid State. There is a great interest here and in other places in Latin America in this field. We have bought a Varian paramagnetic resonance spectrometer thanks to a Ford Foundation grant, for a group which is starting work in the Centro. We have now about ten Latin American students with fellowship in our laboratory. The financial situation however, has been very difficult.

My wife reserved an apartment for you in Copacabana - it has a living room, one bedroom, bathroom, kitchen, bedlinen, dishes, cups, etc, and furniture. They asked US\$ 130.00 a month. It seemed quite reasonable to me and worthwhile to reserve it. It has telephone and you will pay for it as well as for light and gas besides the rent. My wife will also get a maid for you and will see to it about the baby bed.

In a few weeks I will send you the air ticket.

Looking forward to see you soon

Sincerely



J. Leite Lopes
Scientific Director

-39-

CALIFORNIA INSTITUTE OF TECHNOLOGY

CHARLES C. LAURITSEN LABORATORY OF HIGH ENERGY PHYSICS
PASADENA, CALIFORNIA 91125

January 21, 1977

Professor J. Leite Lopes
Director, High Energy Division
Centre de Recherches Nucleaires
67037-Strasbourg Cedex
France

Dear Professor Lopes:

Please accept my sincere apologies for the delay in replying to your invitation to participate in the VIII International Symposium. I will be happy to accept and look forward to seeing you again.

Sincerely,

A handwritten signature in cursive script, reading "R. P. Feynman". The signature is written in dark ink and is positioned above the printed name.

Richard P. Feynman

RPF;ht

844 Alameda Street,
Altadena, California.

Dear Maria Louisa, Leite, Sergio, and everyone,

Greetings for Christmas and the New Year! We hope this finds you well and happy.

We have thought about you so many times, and I had planned to write long before now. Richard and I enjoyed knowing all of you so very much, and we appreciated so much all the wonderful things you did for us. Your party was so nice, and I got so much pleasure from the beautiful, beautiful flowers. We wish we could do something nice for you. We surely hope we may have you visit us some time.

As you know, I left several weeks before Dick did and went to Bolivia and Peru. I stopped for a few days in Soa Paulo with Dick while he gave those lectures, and that was when I had my purse snatched. It had all my air tickets in it, so it made things complicated for a while. Sorry we made you take a lot more classes for Dick, Leite. We were very fortunate. We have finally gotten the travelers checks and most of the other items straightened out so that the loss wasn't too bad.

Peru and Bolivia were very interesting. I had to fly all the way to Lima and then back to La Paz from Lima, since the Braniff flight doesn't stop in La Paz. The flight between Rio and Lima is very beautiful, especially over the Andes, and well worth taking again. You feel dreadful when you first land in Bolivia on the 14,000 foot high Alto Plano (I don't know what that is in metres.) after leaving sea level three hours before. You feel that way for about twenty-four hours and then it goes away. The country is very dry and treeless, and immediately you begin seeing Indians in those wonderful picturesque costumes. You know them, of course. The women wear about eight or ten short full skirts and three or four shawls--all in beautiful colors--and men's round derby hats; and the men wear ponchos and short pants and those pointed caps with ear flaps. At the time I was there the Indians had just killed a number of white land owners in the Alto Plano outside La Paz and were creating a number of incidents. Cesar has probably had several exciting stories circulating around the Centro by now. Anyhow, one had to be a little careful. All the people there live behind very high thick walls with every entrance bolted. There seems to be no real protection from either the police or the government. Everyone seemed to be expecting a revolution at any time. It was interesting, but I don't think I'd like to stay very long.

From La Paz I took a little train to Lake Titicaca and crossed that by steamer (It took all night; the lake is very large.), and continued by train again to Cuzco. There were lots of llamas in the valley which leads from the lake to Cuzco and lots of them around Cuzco. They say they are all domesticated; there are no wild ones any more. I was amazed at how many of the original Inca walls are standing in many streets of Cuzco; I had thought they would be in only a few streets. The buildings have been finished out in adobe, and the people who live in them and make up the major population of the town are mostly Indians. I visited Macchu Picchu, the Inca town on the top of a high mountain which is most easily accessible to tourists. The Incas built many other cities on the very tops of a number of other peaks in the vicinity, but many of them are inaccessible unless one is trained in mountain climbing. I have seen many ruins (Dick and I drove to so many sites in Mexico and Yucatan.)--ruins that were older and more interesting

as ruins go than Macchu Picchu, but the scenery at Macchu Picchu is unbelievably beautiful, since one is surrounded by high forest-covered mountain peaks. In its way it is as amazingly beautiful a setting as Rio, and that is saying a lot.-- There are many Inca ruins around Cuzco, and remains of ancient farming terraces and roads now fallen into disuse. It must have been very wonderful to have seen it as it was under the Incas.

From Cuzco I flew back to Lima and Dick met me there. Lima is a very beautiful Spanish city that has much art and culture. The museums have marvellous collections of those wonderful portrait pots that the Chimus and Mochichas and other pre-Inca groups made, many wonderful Nasca and Paracas textiles taken from Indian graves. Everything in the museums is excellently displayed in a fine educational manner, and they have a number of very good publications. I was much impressed. I hope that you get to see these collections some time.

Dick went to Japan for a two weeks conference immediately after we got back. He thought Japan was very beautiful and he had a wonderful experience there. He lived in a real Japanese hotel (with no furniture except mats, a little low table, a cushion to sit on, and an arm rest), ate nothing but Japanese food, and found he could say almost nothing with his hard learned Japanese!

Dick is in another conference now (on low temperature physics--liquid helium) in Houston, Texas, and he has another one in January in Rochester, New York, and is guest professor at the University of Chicago in March. We're having Christmas here at home in Altadena, though.

I'm sending Sergio a little Japanese kite because it is flat and light. Maybe you have them there. Just stuff it with paper if you want to hang it up, or it should fly. I'd like to send him a little book once in a while, but I suppose it would cause you more trouble and expense than it would be worth.

We would like so much to hear how you are and what you are doing.

Thanks so much for everything. And all best wishes to all of you.

Sincerely,

Mary Louise Feynman
(Feynman's wife)