The Development of Pion-Nucleon Scattering Analysis
A Personal History of Discovery

R. Gordon Moorhouse
Department of Natural Philosophy
University of Glasgow
Glasgow, Scotland

and

L. David Roper
Department of Physics
Virginia Polytechnic Institute and State University
Blacksburg, Virginia, USA

February 1974

I. Introduction

Over two decades ago Fermi and coworkers[1] at the University of Chicago began a revolution in sub-nuclear physics by measuring some differential cross sections for pion-nucleon (πN) scattering and analyzing them in terms of partial waves. An unexplained strong energy dependence was exhibited and the analysis showed that it was due to the existence of a resonance at ~200 MeV pion laboratory kinetic energy (~1235 MeV total center-of-mass energy) in the isospin I=3/2, total angular momentum J=3/2, and positive parity [parity = $-(-1)^L$ for πN, where L is the orbital angular momentum, because the π has negative intrinsic parity relative to N] partial wave. Since $J=L\pm1/2$ for πN, we see that the positive parity quality of the partial wave is equivalent to L=1, or a P-wave πN interaction, so we designate the resonating partial wave as a P_{33} wave according to the usual $L_{2I,2J}$ symbol.

Although the resonance behavior of the P_{33} wave is quite clear, even for crude data, the other partial waves were not so easily determined. Various ambiguities were discovered[2], and interest waned in πN partial-wave analysis after it was shown that one of these ambiguities was due to lack of data for the polarization of the recoil nucleon.

The 200 MeV P_{33} resonance is an extremely sharp feature of the πN total cross section. Being the easiest measurements to make, total πN cross sections appeared at higher energies very soon

---

[1] E.g., see H. L. Anderson, E. Fermi, R. L. Martin, and D. E. Nagle, Phys. Rev. 91, 155 (1953).
[2] E. Fermi, Phys. Rev. 91, 947 (1953). S. Minami, Progr. Theoret. Phys. (Kyoto) 11, 213 (1954).

after higher energy accelerators came on line, and other less prominent but quite clear "resonance" bumps occurred at ~600 MeV and ~900 MeV pion laboratory kinetic energy (~1510 MeV and ~1685 MeV total c.m. energy, respectively). Some crude analyses and evidence from pion photo-production experiments indicated[3] that the second (~600 MeV) bump was probably a D_{13} resonance, but could not rule out a P_{13} resonance, and that the third (~900 MeV) bump was probably an F_{15} resonance. Detailed analysis was impossible because of lack of any polarization data and precise differential cross sections. The need for the data was obvious, and many experimental groups were busily trying to obtain it.

This is the point at which the two authors of this article came in. So, in the next two sections of this article we shall tell our separate stories. Finally, at the end we shall tell how the history of the πN analyses developed after the crucial stage described in our personal narrations.

II M.I.T.-Livermore Analysis (Roper)

William M. (Bill) Layson and I were fellow teaching assistants in the Junior Atomic Physics Laboratory at Massachusetts Institute of Technology. I was impressed by his and another graduate student's comments about thesis research work they were pursuing with Professor Bernard Feld. Early in graduate school I thought I wanted to do research in quantum field theory, but after about two years I came to the conclusion that I would be more productive and happier with my feet on the ground rather than in the clouds. Thus I gravitated toward particle physics phenomenology, Prof. Feld's specialty. My first impressions of Prof. Feld as student advisor were greatly enhanced when arrangements were made for Bill Layson to accompany Prof. Feld to CERN in 1960-61 in order to finish his Ph.D. thesis work. When Prof. Feld returned in the fall of 1961 I quickly approached him about being my advisor.

After tossing a few possible research problems around, Prof. Feld and I soon decided to continue the work of Bill Layson[4]. Bill had obtained a set of π^--p partial-wave amplitudes by assuming the existence of the D_{13} and F_{15} resonances at 600 and 900 MeV laboratory pion kinetic energy, respectively. Prof. Feld felt that, with new data rapidly becoming available, we might be able to separate out the isospin 1/2 and 3/2 partial waves by using both π^--p and π^+-p data. So I, with great enthusiasm, set out on the arduous task of collecting all pion-nucleon scattering data. The work was slowed somewhat by the necessity to study hard for my second, and final, try at the MIT Physics Ph.D. General

[3] R. F. Peierls, Phys. Rev. 118, 323 (1960).
[4] W. M. Layson, Nuovo Cim. 27, 724 (1963).

Examination. With that hurdle out of the way by April of 1962, the future looked rosy when summer arrived. The data available and promised for the near future looked abundant; all would be well if I could just figure out how to manage the tremendous computing task of fitting the data. And I had a "lucrative" job lined up for the summer, to support my family of four, working with Dr. Michael Moravcsik at the Lawrence Radiation Laboratory at Livermore (now called the Lawrence Livermore Laboratory).

Most of the summer with Dr. Moravcsik was spent trying to determine the K-meson parity and yielded no results. But I gained valuable computer experience and had many opportunities to discuss the Livermore proton-proton scattering analysis and my proposed pion-nucleon analysis with Dr. Moravcsik, Dr. Pierre Noyes, Dr. Malcolm MacGregor, and two programmer-physicists, Richard Arndt and Robert Wright. They convinced me that I should do an energy-dependent analysis rather than an energy-independent one. Robert Wright developed a lively interest in the pion-nucleon analysis and made many suggestions about the computer aspects of the problem. Toward the end of the summer he suggested that I approach Dr. Moravcsik about the possibility that some of Wright's time during the next year be devoted to altering Richard Arndt's Livermore proton-proton computer code (POP) in order to do the pion-nucleon analysis (PIP). Mike Moravcsik agreed to this on the condition that I would include in my work an attempt to represent the high partial waves by the lowest-mass meson and baryon exchanges. Mike departed for nine months in Pakistan about the same time that I returned to MIT.

Prof. Feld agreed to the condition set by Dr. Moravcsik. So Robert Wright began developing the computer code and I began getting the data into useable form and developing the equations and techniques to be used in the analysis. Robert and I wrote each other approximately once each week. I would send him new data, corrections to old data, and my ideas on how we should proceed from that point in time; he would send me his latest technique for doing a particular thing on the computer along with several questions about the next step. At the same time, I was writing to Dr. Moravcsik in Pakistan about twice a week, sending him my latest equations and thoughts about the analysis. And he would answer every letter with his usual helpful comments. And once or sometimes twice a week I would meet with Prof. Feld and fill him in on my progress and ask him questions. Now, more than ten years later, I realize much more than I did then how fortunate I was to have the constant advice of two of the world's best particle physics phenomenologists.

Our approach was to put Breit-Wigner resonances in certain partial waves and to parametrize the background for these partial waves and all other partial waves by smooth functions of the energy. (We used a power series in momentum for this parametrization.) We would then determine which partial waves were resonating by trying resonances in various partial waves. Of course, there was no question but that the P_{33} state was the appropriate resonance state for the 200 MeV bump in the $\pi^{\pm}$-p total cross sections. But there was some uncertainty about whether the D_{13}, P_{13} or both states were resonating around 600 MeV. I decided to try all three possibilities. (Our analysis showed that P_{13} did not resonate at 600 MeV.) All of the non-resonant states were started at the then known values of the scattering lengths. Our goal was to analyze all four resonance regions: 0-350 MeV, 350-700 MeV, 700-1100 MeV and 1100-1500 MeV. There was no problem in fitting the data in the first resonance region. But a foretaste of the troubles ahead was provided by our first attempts to fit the data in the second resonance region. We could not get good fits when we used all of the data. Noticing that the total cross sections were badly fitted, we then did a fit to the total cross sections alone. Then that solution was used as input for fitting all of the data, and a good fit was obtained.

The analysis took a huge amount of human and Livermore computer (IBM 7094) time. MIT sponsored a two-week trip to California for me in March, 1963 to try to speed things up. But, alas, the calculations still were not satisfactory by graduation time. Fortunately, Livermore had already awarded me a postdoctoral appointment in Moravcsik's particle physics group, so

Dr. Sidney Fernbach, Director of the Theoretical Division at Livermore, offered me a short term appointment until the thesis was completed. So a long move to California at the end of May and many long hours at the computer (including several complete nights at the Berkeley campus computer) brought the analysis to a point of reasonable completeness for the first two resonance regions.

Prof. Feld was anxiously awaiting these results at the Siena Conference[5] in Italy, so I fired off a long telegram giving the partial-waves' behaviors, including strong evidence for a P_{11} resonance near 600 MeV. (I later had a difficult time justifying that extravagant expenditure of Livermore funds. This is probably a good place to admit that I regularly made great demands for computer time at Livermore, with little recognition of the fact that the taxpayers installed that gigantic computer complex there in order to build bombs, not discover resonances.) Later, in July, Livermore sent me back to MIT to successfully defend my Ph.D. thesis.

[5] B. T. Feld and L. D. Roper, Proc. of the Siena Intern. Conf. on Elem. Part. (Italian Phys. Soc., Bologna, 1963), p. 400.

The data were much better fitted by assuming a D_{13} resonance at 600 MeV rather than a P_{13} or both D_{13} and P_{13}. Unexpectedly, however, the P_{11} state exhibited resonance-like behavior near 600 MeV even though no resonance parametrization was used for it. I tried many times to freeze the P_{11} state into some non-resonant behavior while varying all other partial waves. But every time, upon release, the P_{11} would change to look like a resonance. And a slightly better fit would be achieved by assuming a resonance form for the P_{11} state. Layson's work had indicated the possibility of a P_{11} resonance at ~900 MeV and I had hoped to look for it when I got that high in energy; but it was very surprising to observe its resonance behavior near 600 MeV because no one had ever hinted at it before and the P_{11} scattering length is rather large and negative. I spent a much time trying to eliminate the P_{11} resonance.

In December, 1963, several of we Livermore postdocs went to the APS meeting at Cal Tech. Just before leaving, I received a preliminary version of the long-awaited charge-exchange differential cross section data measured in the second resonance region by Burton Moyer's Laboratory at Berkeley. I hurriedly calculated my analysis' predictions at their energies and plotted them on the data graphs while on the plane to Pasadena. Almost without exception every data point included the computed curve within its error bar! I was elated! I think that that was the precise time when I <u>knew</u> that my analysis was correct. After Dr. Moyer's talk about their data at the APS meeting I introduced myself to him and showed him my curves versus their data. I am not sure that he believed me. I later gave several talks about my work at the Berkeley laboratory and spent many hours with the Moyer's group experimentalists discussing my results and future pion-nucleon experiments.

In January, 1964 I gave a post-deadline paper at the APS Annual Meeting in New York City. After the talk, Frank Lin, a graduate student working with Prof. Hull at Yale told me that they were also doing a pion-nucleon scattering analysis[6].

In early 1964 a paper by Bareyre, et al.[7] appeared which showed that the low-energy asymmetry of the 600 MeV bump in the π^--p total cross sections is either due to a P_{11} or an S_{11} resonance. Mike Moravcsik strongly urged that I quickly get a letter into Physical Review Letters about the P_{11} resonance. He insisted that only my name should appear on this first paper[8] announcing the P_{11} resonance. He wrote a letter to Prof. Feld expressing this

---

[6] M. H. Hull, Jr. and F. C. Lin, Phys. Rev. <u>139</u>, B630 (1965).
[7] P. Bareyre, C. Bricman, G Valladas, G. Villet, J. Bizard, and J. Sequinot, Phys. Letters 8, 137 (1964).
[8] L. D. Roper, Phys. Rev. Letters 12, 340 (1964).

opinion and Prof. Feld immediately sent back his agreement. The later complete paper[9] on the 0-700 MeV analysis contained the names of Robert Wright, Prof. Feld and me. I have always felt that Mike Moravcsik's name should have been on it also, but Mike did not think so. Wright and I later published a more detailed analysis of the 0-350 MeV data[10]

During the various talks that I gave about the analysis, snickers were usually rampant when I stated that we were fitting 1200 data with 100 variable parameters. The first unthinking comment was usually "You can fit anything with 100 parameters." However, quite justified questions were usually raised about the uniqueness of the fit. The constraints of available computer time made extensive tests for uniqueness impossible, but we were able to satisfy ourselves that the gross features of our partial-wave amplitudes were unique. Later analysis by others confirmed this.

Our analysis would not have been possible without the availability of the sophisticated nonlinear least-squares-fitting program of Richard Arndt. Richard spent several years developing this program at Livermore. I later in 1967 had the good fortune to join the faculty at Virginia Polytechnic Institute (now Virginia Polytechnic Institute and State University) along with Richard, who had earned a Ph.D. from Berkeley in 1965.

My attempts to extend the analysis above 700 MeV were largely fruitless. In retrospect it appears that the trouble lay in not having a S_{11} resonance[11] near 600 MeV and not including enough resonances[12] near 900 MeV. If I had restudied Bill Layson's results more carefully at that point I might have had better luck, because many of the resonances now known to exist were strongly hinted at in his work.

Shortly after publishing our work I was delighted to receive preprints from Bransden, Moorhouse, and O'Donnell at Rutherford Laboratory in England which confirmed the basic features of our work. Also, the Lin and Hull work confirmed our results.

The discovery of the P_{11} resonance was the spark that set off the baryon resonance explosion of the late 1960s. In Tables 1 and Figure 1 are listed the pion-nucleon resonances of the Dalitz-Horgan quark model fit to the known pion-nucleon resonance masses. Several new resonances were predicted. In particular, notice that there are nine resonances (six I=1/2 resonances and

---

[9] L. D. Roper, R. M. Wright, and B. T. Feld, Phys. Rev. 138, B190 (1965).
[10] L. D. Roper and R. M. Wright, Phys. Rev. 138, B921 (1965).
[11] B. H. Bransden, P. J. ODonnell, and R. G. Moorhouse, Phys. Letters 11, 339 (1964); Phys. Rev. 139, B1566 (1965).
[12] R. Horgan, Nucl. Phys. B71, 514 (1974).

three I=3/2 resonances) near 900 MeV (~1700 MeV total c.m. energy). It remains to be seen whether scattering data between 700 and 1100 MeV can uniquely determine this many resonances. There are probably several more resonances belonging to higher SU6 supermultiplets besides those given in Tables 1 and Figure 2 for the 1900 MeV total c.m. energy region.

Table 1. Pion-nucleon states (total c.m. energies) according to Dalitz and Horgan for the lowest quark model SU6 supermultiplets.

L	$(J)^P$						
			I=1/2 (N^*)				
3	$(7/2)^+$						1998
3	$(5/2)^+$			1707	1863		1962
2	$(5/2)^-$			1692			
2	$(3/2)^-$		1535	1702			
1	$(3/2)^+$			1704	1846	1891	1942
1	$(1/2)^+$	938	1443	1759			1937
0	$(1/2)^-$		1541	1708			
					I=3/2 (Δ)		
L	$(J)^P$						
3	$(7/2)^+$				1927		
3	$(5/2)^+$			1910	1927		
2	$(5/2)^-$			(1900)			
2	$(3/2)^-$		1650				
1	$(3/2)^+$	1233	1705	1910	1927		
1	$(1/2)^+$			1879	1927		
0	$(1/2)^-$		1650	(1900)			

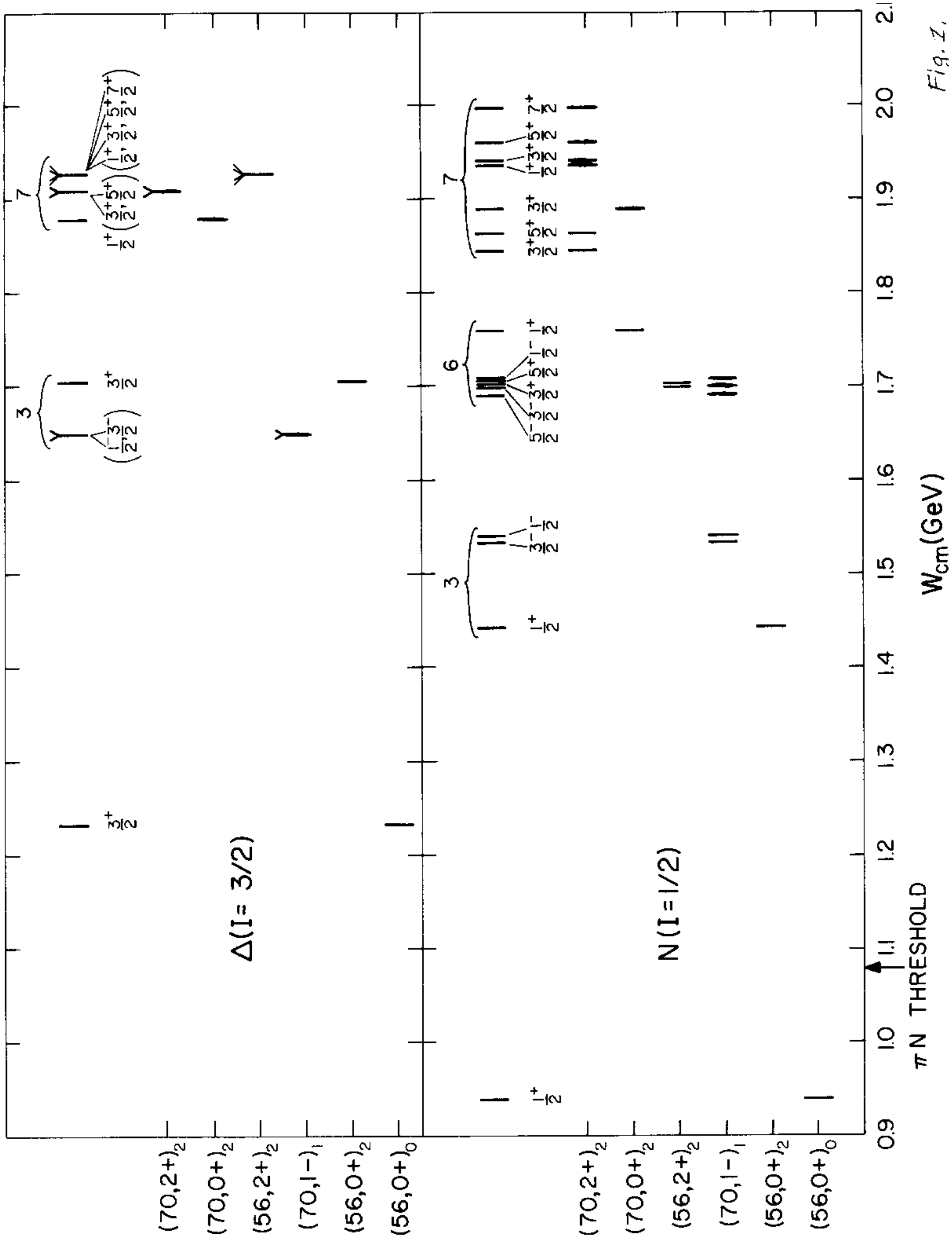

Figure 1
Pion-nucleon states according to Dalitz and Horgan for the lowest quark model SU6 supermultiplets.

III. The Rutherford Analysis (Moorhouse)

In 1963 it was already known that, in addition to the P_{33} resonance of mass 1230 MeV (from here on all masses will be given in terms of mc^2 in MeV) well established in the 1950's by Chicago pion-nucleon scattering experiments and partial-wave analysis, there also existed resonances of masses about 1520 MeV (most likely a D_{13} state),about 1690 MeV (an F_{15} or D_{15} state),and about 1920 MeV (states unknown then); the evidence was from pion-nucleon scattering, both in total cross sections and gross features of angular distributions, and from pion-photoproduction experiments (photon + nucleon -> pion + nucleon). At that time, 1963, this author was a staff member of the Rutherford Laboratory in England whose proton accelerator was due to operate in 1964, one of the first planned experiments being pion-nucleon scattering in the mass range 1500-1750 MeV.

There were many corridor discussions with the experimenters, particularly Paul Murphy, in which the then recent and forthcoming increase in world pion-nucleon scattering data became evident. Also, at a Rutherford Laboratory colloquium Claude Lovelace presented some interesting theoretical work by himself and
Paul Auvil on information to be gained from those points where the pion-nucleon scattering differential cross-section was very small. A stronger theoretical strand was the contemporary interest in Regge-pole theory, and I became interested in formulating the pion-nucleon scattering amplitude as a superposition of 'Regge poles' in the direct channel (symbolically: pion + nucleon -> 'Regge poles' -> pion + nucleon) since this would give an ansatz on parametrization of these amplitudes as a continuous function of both energy and scattering angle. Brian Bransden and I discussed analyzing the pion-nucleon scattering data using such a parametrization which would give information on the Regge poles, some of which could incorporate the above 'known' resonances. Bransden, probably rather wisely, was not so enthusiastic, considering that such an ansatz involved too many unproved assumptions. In July of 1963, at the Scottish Universities Summer School, or perhaps earlier, I returned to Bransden with a proposal that we formulate the scattering amplitude as a sum of partial waves parametrizing the imaginary part of each wave as a continuous function of energy and angle, the real part being expressed in terms of the imaginary part through a partial-wave dispersion relation, thus incorporating some theoretical constraints on the scattering amplitude. Almost immediately thereafter we wrote the central part of a computer program to analyze pion-nucleon scattering using this ansatz; we chose to use partial-wave dispersion relations in the inverse, $T^{-1}_{2I,2J}$, of the partial-wave amplitudes, $T_{2I,2J}$, as suggested by Bransden.

The time until the end of 1963 was occupied with writing the complete program and with my trying to implement it on the Rutherford Laboratory computer with help from the newly recruited third collaborator Pat O'Donnell (who had just moved to Durham University from Glasgow along with Bransden), but the effective computer job turn-around time for us low-priority users was about three days and the debugging progress was abysmally slow; great sufferers also were certain experimental physicists. Fortunately, Bill Walkinshaw (a division Head at Rutherford) accepted a very generous offer by the newly founded Deutsches Rechenzentrum at Darmstadt, Germany to let those physicists compute at a nominal price on their new IBM 7090, for which there was little immediate German usage, and Walkinshaw let us in on the tail of the experimental physicists. Pat O'Donnell and I arrived in Darmstadt on Fassnacht in February of 1964 and immediately obtained about six debugging runs per day and extensive long computer runs at night. He and I spent the next few months commuting to Darmstadt for periods of two to three weeks, sometimes alternately and sometimes together.

The first thing we did, as a test of our method, was to analyze positive pion-proton (π^+p) elastic scattering from threshold (1080 MeV) to about 1300 MeV center-of-mass energy; this included the already well-known P_{33} resonance of 1230 MeV mass. The positive pion proton system has the simplifying property of being in a pure isospin state, I=3/2; also in this energy range, where the production of a second pion (an inelastic effect) is kinematically possible, pion production just happens not to occur, giving a further simplification by effectively eliminating the need for inelastic parameters.

In these circumstances the choice of parametrization as a function of energy for the various partial waves was rather easy, but it was most heartening to find that our method and program achieved immediately a creditable fit (χ^2 minimum[13]) to the data

[13] Fits to the data are obtained by finding the minimum value of a certain well-known quantity, conventionally known as χ^2, which measures the agreement between the data values predicted by the physical model and the data values found from experiment, with appropriate weighting for the experimental errors.

$$\chi^2 = \sum \left(\frac{\text{Quantity(predicted)} - \text{Quantity(experimental)}}{\text{Quantity error (experimental)}} \right)^2$$

The procedure is to vary the parameters of the physical model with the object of obtaining as close as possible an agreement with the data which corresponds to as small as possible a value of χ^2. The attainment of such a good agreement with experiment with values of the mass and width of the P_{33} resonance within the range found by previous workers.

Our χ^2 was a complicated function of the parameters, consequently no mathematical theory was available to simplify the problem and, also, we had a large number of parameters, making impossible a simple grid search by a computer. (If there are 30 parameters and one evaluates χ^2 for 10 values of each parameter with 1 second computer time for each evaluation then the time taken would be 10^{30} seconds.) So the computer program which evaluates χ^2 has to be linked, within the computer, to a computer program which makes a rather sophisticated search in the parameter space to find minima of χ^2.

In fact we were faced with an unusually large number of parameters, about twenty to begin with, in our first simple minimization of χ^2 for the P_{33} resonance region described above, and up to seventy finally. It is surprising we were optimistic enough to commence a search for minima of a complicated function in a fifty-dimensional, or more, parameter space but we were helped as will be related, by the special (perhaps against reasonable expectation) features of the pion-nucleon system. Also, on a technical level, we found a minimization program newly written by M. J. D. Powell, a numerical analyst at a neighbouring atomic energy laboratory, which satisfied all our requirements and has since been much used by a number of elementary particle physicists.

Our fit to the π^+p data in the first resonance region, besides confirming our method, helped to fix the partial-wave amplitudes at the topmost energy 1300 MeV, which was to be the bottom-most energy of our next step. There was already a careful partial-wave analysis by Vik and Rugge[14] at this single energy, 1300 MeV, (remember, our partial-wave analysis covered a continuous range of energies) with three alternative solutions. We already had some theoretical prejudice as to which of these solutions was the best from the work of Jim Hamilton and Sandy Donnachie and of G. Kane and T. D. Spearman[15]. Our first analysis confirmed our prejudice and we used this particular Vik and Rugge solution as a guide to our partial waves at 1300 MeV in the next stage which

---

then signifies two things: (i) that, primae faciae, the physical model used can represent reality and (ii) that the parameters for which χ^2 is minimum are near their real physical values.

[14] O. T. Vik and H. R. Rugge, Phys. Rev. 129, 2311 (1963).

[15] A. Donnachie, J. Hamilton, and A. T. Lea, Phys. Rev. 135, B515 (1964). G. L. Kane and T. D. Spearman, Phys. Rev. Letters 11, 45 (1963).

was to fit both π^+p and π^-p elastic scattering data from 1300 MeV to 1580 MeV, covering the region of the second resonance D_{13}.

We decided to limit the D_{13}-wave parameters of our model so that a resonance in that particular wave was forced, but to allow the data-fitting program to determine the exact position and width of the D_{13} resonance. At that time we had little or no idea that there were any other resonances within this energy region and our main purpose was to determine the exact D_{13} energy and width and then to step to higher energies. Even with this assumption, however, it was not easy to obtain any good fits to the data and many attempts were unsuccessful, with consequent rethinking of the details of the parametrization and the ranges of the parameters. Pat O'Donnell liked to leave the ranges of the parameters relatively wide so that the computer minimization program was rather free to make its own best choice. My reaction in case of difficulty was to restrict certain key parameters to seemingly likely values on the hypothesis that we knew better than the computer. This probably reflects some difference in our psychology to my discredit. Over a period of four months from February to June 1964 we achieved one good fit to the data helped by the fact that the D_{13} resonant wave is very prominent and that other waves are presumably largely determined by their interference with it (though the exact details of this determination lie hidden in the thought processes of the computer program) and that we did not lightly allow other waves to become prominent. Our fit displayed a large and resonant D_{13} wave, of course, but also large P_{11} and S_{11} waves, whose interpretation was not immediately evident to us.

Early during this period, February-June 1964, a letter had been published by Bareyre, Valladas and others from Saclay Laboratory, near Paris, which pointed out that, if one graphed the isospin I=1/2 total cross section for pion-nucleon scattering as a function of energy and subtracted from it the probable contribution of the D_{13}(1520) resonance (which caused a bump in the total cross section at that energy), then there remained another bump at ~1420 MeV which might be due to an S-wave or a P-wave resonance. This was extremely interesting, though far from evidence for another resonance. Then in April there appeared the Letter by Roper, giving the results of his partial-wave analysis, with a resonance in the P_{11} wave known almost immediately, and since, as the Roper resonance. Bransden had been at the Siena Conference, so we knew from Feld's report of the existence of the M.I.T.-Livermore analysis but did not know its exact results. The publication of Roper's letter came at an advanced stage of our own analysis and so did not affect our techniques or results, but it inspired us to write a Rutherford Laboratory Report (July 1964) which was published shortly thereafter, giving the results of our own analysis and pointing out that, like Roper, we had a large P_{11} wave but that a resonance interpretation was not so evident from our results.

We gave our results in terms of δ (the real phase shift) and η (the inelasticity) of each partial wave, as did Roper, and at this stage we had given little thought to resonance theory and were looking for the phase shift δ to go through 90°, as an indication of resonance, as in the case of the P_{33} and D_{13} resonances. At some time in July, I presented our results to an evening seminar in Oxford with a vacation-time audience of about six people and speculated about the possibility of P_{11} and S_{11} resonances, whereupon Dick Dalitz introduced the idea (already known in other contexts to some physicists) of plotting the amplitudes on an Argand diagram (Figure 2).

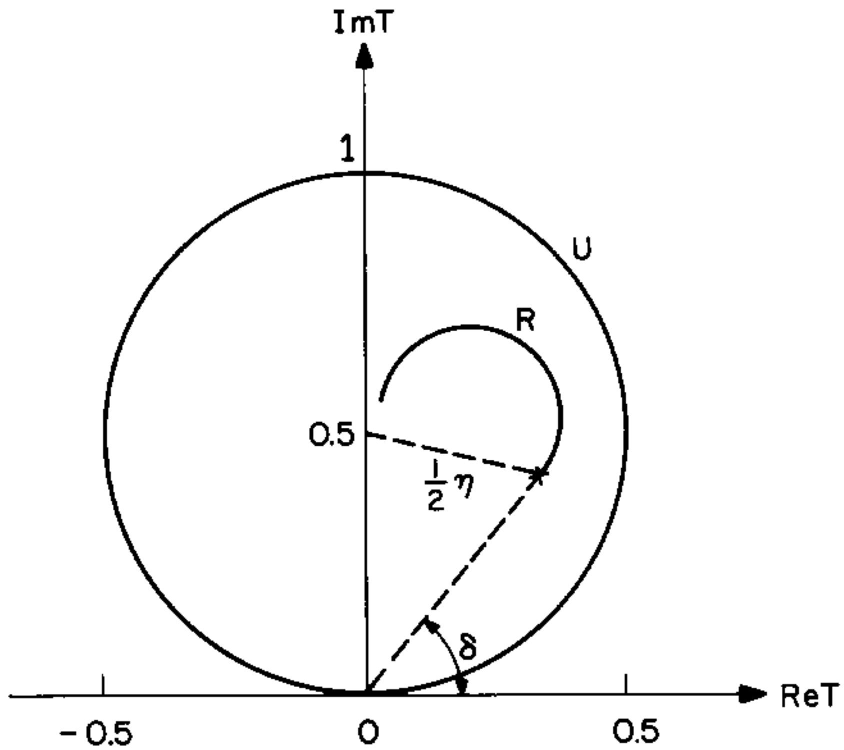

Figure 2
Argand diagram for a (complex) partial-wave amplitude T, a function of the energy E, of the pion and nucleon in the elastic scattering process. If the scattering particles have an ideal resonance in this partial wave then T describes a circle in the Argand diagram, such as R, as E varies. In the diagram X represents a typical point of T which may be alternatively specified by the phase shift angle, δ, and radial distance η/2, which are shown. The unitarity (conservation of probability) conditions require that η ≦ 1, so that all points X must lie within or on the unitarity circle, U.

In such a diagram for the amplitude to swiftly traverse (as energy varies) a considerable portion of an anti-clockwise circle is an indication of resonance; this technique of resonance spotting has been of primary importance in pion-nucleon partial-wave analyses. More work at Darmstadt led to a second fit to the data, with partial waves similar to our first solution, and in November of 1964 we issued a Rutherford Laboratory Report (later published in Physical Review) complete with Argand diagrams (Figure 3) of the D_{13}, P_{11} and S_{11} waves and a discussion of possible resonances in the two waves (the D_{13} resonance being beyond question); we were rather positive about an S_{11} resonance.

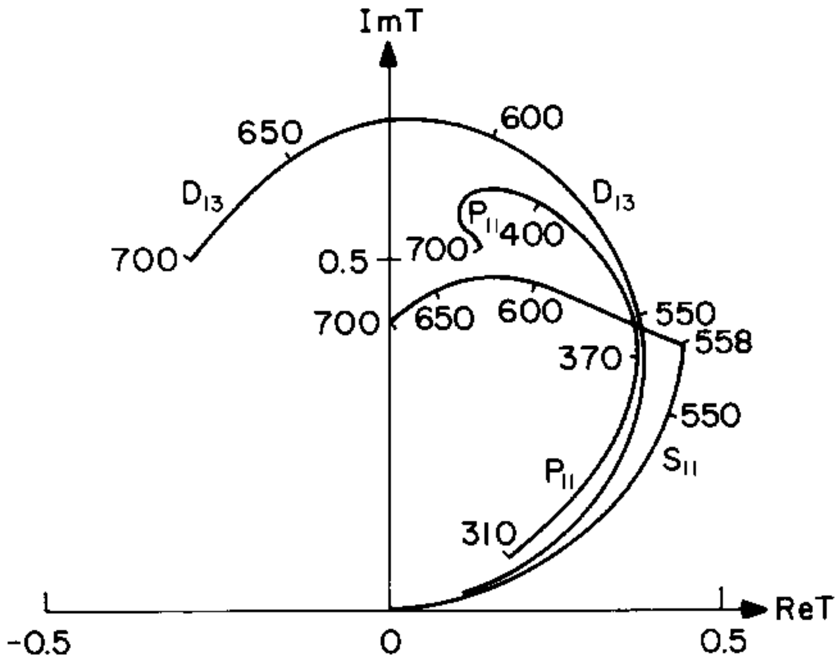

Figure 3
Argand diagrams of the D_{13}, P_{11} and S_{11} partial waves from the 1964 analysis of Bransden, Moorhouse and O'Donnell. The D_{13} describes a rather good circle as a function of energy, but the resonant circles in the other waves are somewhat distorted by background (the P_{11} curve is now know to extend over the imaginary axis before curving back).

Shortly after the publication of our first Letter, there appeared a Letter from some London University physicists, Paul Auvil, Claude Lovelace, Sandy Donnachie, and Andrew Lea[16], presenting a partial-wave analysis in the same energy region with similar results. Unlike Roper's and our analyses the data were not fitted by continuous functions of energy, but were fitted semi-independently, at each discrete energy having data, with theoretical guidance before and after fitting from partial-wave dispersion relations. Later, at a Royal Society of London Discussion Meeting in February of 1965, Lovelace presented evidence for an S_{31} resonance at 1650 MeV and a possible higher P_{11}

[16] P. Auvil, A. Donnachie, A. T. Lea, and C. Lovelace, Phys. Letters 12, 76 (1964).

resonance, given with his usual high-spirited and uninhibited attacks on other research workers in the same field as is especially well-remembered by one natural target who happened to be speaking at the same meeting.

One of my pleasures was to regularly attend the Oxford Thursday evening particle physics seminars and one experimental talk mentioned the strong η-meson production in the process π^-+proton-> η+neutron just above the threshold energy and consequently in the energy region of our S_{11} phenomenon (Figure 3). As a result Archie Hendry and I used a two-channel reaction formalism (the two channels being η+nucleon and π+nucleon, both in an S-wave) to simultaneously fit the η-production data (assumed from its angular distribution to be mainly s-wave) and also the S_{11} amplitude of our partial-wave analysis. Hendry and I found about April of 1965 that this fitting process definitely indicated an s-wave resonance at about 1530 MeV with formation from (and decay into) both the η-nucleon and π-nucleon channels[17]

IV. Rationalizing the Resonance Population Explosion

In the summer of 1965, Dick Dalitz at Oxford, who was well aware of all these developments, was due to give a lecture series at a Summer School at Les Houches in the French Alps and his mind turned to some qualitative theoretical developments. In 1963 Gell-Mann, and also Zweig, had introduced the idea of baryons, such as the nucleon, being formed of three imaginary particles called quarks, and mesons being formed of one quark plus one anti-quark. These quarks, which might indeed be purely mathematical objects, served as bases for the newly discovered SU3 symmetry, and if endowed with spin 1/2 would also serve as bases for the larger SU6 symmetry which Radicatti and Pais pointed out produced a few remarkable agreements with hadron properties. Morpurgo and Becchi had shown that regarding the quarks as real objects undergoing non-relativistic motion at the bottom of a deep potential well gave agreement not only with some electromagnetic properties of the nucleon but also with those of the P_{33} resonance, when similarly regarded as a particle made out of three quarks. Greenberg had observed that the 56-dimensional representation of SU6, which contains most of the baryons that were well known at that time, such as the nucleon and the P_{33} resonance, is symmetric in the quark coordinates and had hypothesized that further baryons might also obey this symmetric rule.

[17] A. W. Hendry and R. G. Moorhouse, Phys. Letters 18, 171 (1965).

In his Les Houches lectures[18] (July,1965) Dalitz systematically developed the symmetric quark model, including orbital motion of the quarks. The next higher energy state than the orbital ground state $L^P=0^+$, of the quarks (P=parity) has orbital angular momentum $L^P=1^-$ and, when combined with a 70-dimensional SU6 representation, can make a symmetric state. This $L^P=1^-$ 70-dimensional representation, as shown in Figure 4, contained the old D_{13} resonance and the newly discovered S_{11} and S_{31} resonances.

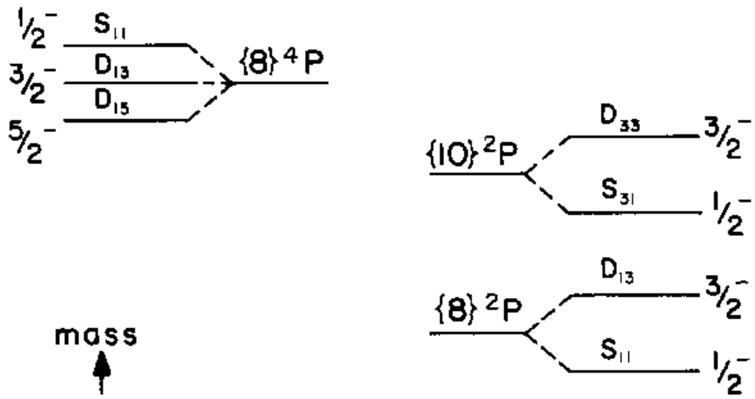

Figure 4

The {70} 1^{--} supermultiplet of the L-excitation quark model. The $\{SU3\}^{2S+1}L$ multiplets are shown split into the J^P submultiplets. The nucleonic state corresponding to each submultiplet is indicated in pion-nucleon scattering notation.

Also it contained a D_{15} resonance and a possible further S_{11} resonance (Figure 5) newly available to Dalitz in a pre-preprint of a 0-1700 MeV discrete-energy partial-wave analysis of Bareyre, Bricman, Stirling, and Villet[19].

[18] R. H. Dalitz, Lectures in Theoretical Physics (Gordon and Breach, New York, 1966).

[19] P. Bareyre, C. Bricman, A. V. Stirling, and G. Villet, Phys. Letters 18, 342 (1965).

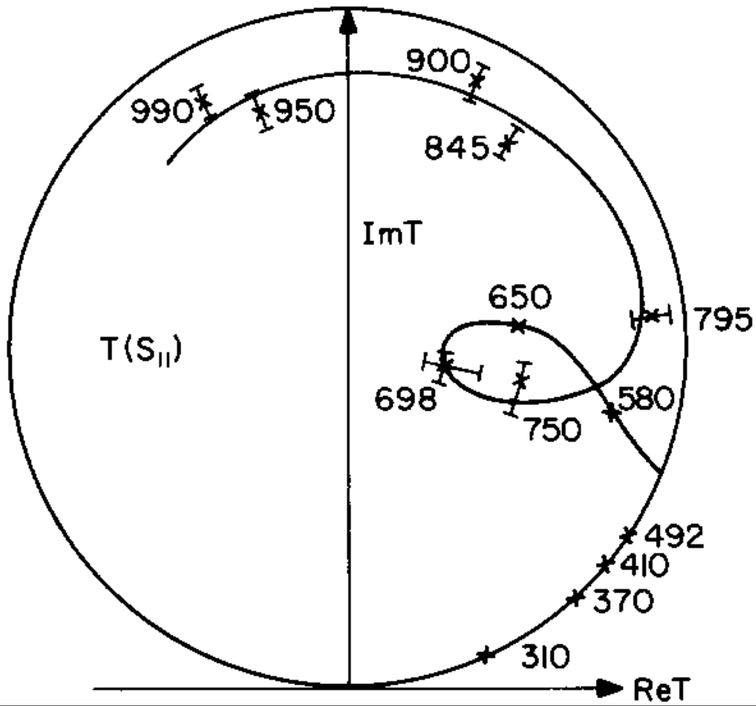

Figure 5
The scattering amplitude T(S11) obtained by Bareyre et al. in their phase shift analysis of the pion-nucleon scattering data is plotted on a Argand diagram as a function of pion laboratory kinetic energy in MeV. After a strong cusp at the nn threshold, the amplitude first follows a looped path (where the analysis of Hendry and Moorhouse17 indicates a resonance close to 600 MeV) and then rapidly traces out the upper part of a second circular loop (which is interpreted to reflect the existence of a resonant state at about 900 MeV).

Other resonances such as the Roper resonance could be fitted into other multiplets. (Later on all the resonances of the 70-dimensional $L^P=1^-$ multiplet would be discovered, with no further negative-parity resonances below 1800 MeV.) At the Oxford International Conference[20] in 1965 Peyrou gave the baryon resonance raporteur talk, concentrating largely on pion-nucleon partial-wave analyses, particularly that of his compatriots, Bareyre et al. Dalitz, also in a principal talk, presented his work on the quark model interpretation.

With these presentations to an international conference a certain revolution was completed and the succeeding years consolidated the new regime[21]. The revolution was the large number of new

[20] C. Pevrou, Proc. 1965 O~ford International Conference on Elementary Particles, (Rutherford High Energy Laboratory, (1966).

[21] Of particular importance was the raporteur's talk at the 1968 Heidelberg Conference in which Claude Lovelace presented a

pion-nucleon resonances discovered, which brought about a new way of regarding resonances. Previously explanations of, say pion-nucleon, resonances were sought in detailed dynamical calculations involving the exchange of elementary particles between the pion and the nucleon. Indeed, an explanation of the existence of the nucleon itself was sought in this way, the nucleon being regarded as a P_{11} state of the pion-nucleon system. Such a calculation should certainly have predicted the Roper, or P_{11} resonance - but it did not[22]. This philosophy was not disproved or even totally abandoned - indeed it could coexist with the quark model - but simply fell into relative disuse over the succeeding years in the face of a naive quark model which has greater predictive power though being still illogical and incomplete. The contribution of the pion-nucleon partial-wave analyses to the revolution was the discovery of the "hidden" low-angular-momentum resonances, and eventually of, it appears, all of the resonances up to a certain energy. In fact the existence of the resonances of the 70-dimensional $L^P=1^-$ multiplet, and no other 'low energy' negative-parity resonances is still one of the most convincing pieces of evidence for the quark model - along, incidentally, with the success of the quark-model predictions on the photon + nucleon (i.e. electromagnetic) formation of these same resonances. (Photoproduction is a whole other chapter in the resonance story.)

---

large number of new resonances, including firm evidence for all those classified in the quark model as belonging to the 70, $L^P=1^-$ multiplet. These results were mainly from the work of Lovelace, Donnachie, Kirsopp, and Lee at Cern and Johnson, Grannis, Hansroul, Chamberlain, Shapiro, and Steiner at Berkeley.

[22] After its discovery, more complicated multichannel calculations on similar lines did "produce" the Roper resonance. See, e.g., E. N. Argyles and A. Rotsstein, Phys. Rev. 174, 1689 (1968).