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COMMENTS ON REGGE POLES AND RESONANCE CIRCLES

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A B S T R A C T

We discuss various objections raised against the resonance interpretation of the Argand diagram loops which are generated by Regge pole exchanges.

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In a recent article, "Direct-channel Resonances from Regge Pole Exchange" ¹⁾, we partial-wave analyzed the ρ Regge exchange amplitude for πN charge exchange scattering. This produced loops on the Argand diagrams corresponding to the prominent experimental N^* resonances. We proposed the interpretation that these loops are approximations to the genuine resonance circles, which are associated with second sheet poles ²⁾. There have been various objections ³⁾⁻⁸⁾ to this interpretation and it has been proposed to consider these circles as something different from resonance circles. In this article we shall discuss the various objections. We shall explain why we think that there is no evidence against the resonance interpretation of the Argand loops.

1. The distinction between the full resonance contributions and the fluctuations visible at fixed t .

The Regge amplitude $\beta(t)s^{\alpha(t)}$ does not show the fluctuations (wiggles) in s which are one of the typical features of direct-channel resonances. Can we conclude from this that the direct-channel resonances are entirely absent from the Regge amplitude ³⁾? The answer is no, as it can be seen in the following. Consider, e.g., the helicity flip amplitude $B(s,t)$ for πN charge exchange. It is crucial to note that each resonance by itself contributes a large peak, while the sum of resonances gives a curve which shows (at fixed t) only small fluctuations in s . This comes about because the resonances overlap and because all prominent resonances contribute with the same sign to B_{CEX} at fixed t ⁹⁾. Although we smooth out these small fluctuations by using $s^{\alpha(t)}$, we still retain the main contribution of the resonances. It is wrong to conclude from the absence of fluctuations (at fixed t) that the resonance contributions are absent from the Regge amplitude; it is only the small fluctuations which are missing. Note that the fluctuations at fixed t are too small to be observed experimentally for $p_L > 2 \text{ GeV/c}$ and t fixed in the near-forward or in the secondary peak regions. But the full resonance contributions are still sizeable.

The distinction between fixed t and fixed ℓ is important: by considering fixed ℓ we pick out one resonance from the prominent sequence, and, therefore, we get a pronounced peak.

Various partial waves behave in a compensating way, such that peaks are present in $\text{Im}a(\ell, s)$ but not in $\text{Im}a(s, t)$. Collins, Johnson and Squires³⁾ argue that this compensation feature is not characteristic of resonances. But we point out that the experimentally well established, prominent N^* resonances show indeed a strong correlation, and these N^* 's do cause successive partial waves to behave (approximately) in a compensating way⁹⁾.

2. Another mechanism which also generates circles?

Collins, Johnson and Squires³⁾ state: "We now see that there is another mechanism in nature, i.e., the crossed channel Regge pole contribution, which has nothing to do with direct-channel resonances but which is capable of generating such circles." On the contrary, we remind the reader that the Regge pole exchange is not "another mechanism", rather it is the usual mechanism, it is the "force" that produces the resonances. This is similar to the mechanism in N/D calculations, where an elementary meson exchange provides the "force" which generates resonances. There are only two differences between our procedure and the N/D procedure which is supposed to generate genuine resonances: (a) in the N/D scheme one explicitly unitarizes the partial-wave projection of the elementary particle exchange amplitude, while in our scheme the partial-wave projection of the Regge exchange amplitude automatically is approximately unitary. (If the Regge amplitude approximates the exact amplitude it has to satisfy unitarity approximately.) This is

achieved by the specific form of the residue functions $\beta(t)$ ¹⁰⁾;
 (b) the N/D scheme produces 2nd sheet poles while our procedure makes no statements about the 2nd sheet, because our input is a poor approximation for fixed s and $t \rightarrow \pm \infty$ (see Section 8).

We do not think that unitarity is a condition which is more deeply "dynamical" than analyticity and crossing. In the last few years, ~1958-1966, there has been a great deal of emphasis on unitarity, while crossing symmetry was somewhat neglected. In the FESR bootstrap scheme and in the partial wave projections of Regge terms crossing and analyticity play the central role. Unitarity is not explicitly enforced. Note that unitarity in the N/D scheme practically meant two-body elastic unitarity (or sometimes two coupled two-body channels). It is clear that unitarity in such a mutilated form is useless at the energies we are dealing with.

3. Unwanted circles in pp or K^+p scattering? Exchange degeneracy.

- a) Consider NN scattering. Will the presence of meson Regge pole terms produce circles in Argand diagrams, in an analogous fashion to those in $\bar{p}N$ charge exchange? Because no resonances have been observed in NN scattering (the deuteron belongs to a qualitatively different type of phenomena), such circles would be highly unwanted and would make us doubt the resonance interpretation of circles in general ^{3),7)}. We have shown in Ref. 1) that such unwanted circles do not appear. Here we shall add some further comments.

No unwanted resonances appear in NN scattering. This happens in our scheme for the same reason as in any other dynamical scheme. The meson exchange forces both in pp and in $p\bar{p}$ scattering are very strong, but while they add in $p\bar{p}$ scattering, they cancel in pp scattering. How this comes about self-consistently is explained in the following paragraph.

First we start from the assumption that no dibaryon resonances exist. We consider $\bar{N}\bar{N} \rightarrow \bar{N}\bar{N}$ scattering and the resonances in this channel, e.g., ρ , A_2 . These resonances are generated by direct forces alone (meson exchange forces), and there are no exchange forces (no dibaryon exchange force). Therefore, the resonances with even j , e.g., the A_2 , are degenerate ("exchange degenerate") with the resonances with odd j , e.g., the ρ . Starting from the absence of dibaryon forces we arrive at exchange degeneracy.

Let us now start from exchange degeneracy and show that no dibaryon resonances will be generated. We consider NN scattering. Because of the signs dictated by exchange degeneracy, (i) the ρ and the A_2 forces in the Born approximation will have opposite signs, and (ii) the combined Regge contribution of ρ - A_2 to NN will be purely real. But a purely real amplitude cannot produce (unwanted) loops. [For details, in the K^+p case, see Ref. 1).] Whether we do an N/D bootstrap with a Born input or a partial wave analysis of the Regge amplitude, in both cases we shall not get any dibaryon resonances. Starting from exchange degeneracy we arrive at the prediction that there are no dibaryon resonances.

- b) Collins, Johnson and Ross ⁷⁾ did a partial wave analysis of K^+p and pp and obtained unwanted loops in contradiction to our statements in Ref. 1) and in the preceding paragraph. In their partial wave analysis of K^+p scattering they include, as one should, the backward peak in K^+p in their partial wave analysis. It is produced by the exchange of Λ , Σ , Y_0^* 's, Y_1^* 's. These authors fail to notice that also these hyperons are exchange degenerate; they include the Λ , but they fail to include its exchange degenerate partner, the $Y_0^*(\frac{3}{2}^-, \frac{7}{2}^-, \dots)$ trajectory ¹¹⁾. This exchange degeneracy leads to a purely real backward amplitude, and therefore no loops can be produced by the backward peak.

The Y^* 's, as they appear in $\bar{K}N$ scattering, are exchange degenerate for exactly the same reason as the ρ and A_2 : absence of a force corresponding to (K^+p) exchange. (The Y^* 's, as they appear in $\Sigma\pi$ and $\Lambda\pi$, are not exchange degenerate. For more details about this point and the following see our preprint "Exchange degeneracy from FESR".) The situation for the Y^* 's is even clearer than for the ρ - A_2 , because the exchange degenerate sequence $\Lambda(\frac{1}{2}^+)$, $Y_0^*(\frac{3}{2}^-, 1520)$, $Y_0^*(\frac{5}{2}^+, 1815)$, $Y_0^*(\frac{7}{2}^-, 2100)$ is observed experimentally, while the recurrence of the A_2 has not yet been observed. Furthermore also the residue functions, the Y^* couplings to $\bar{K}N$, are experimentally exchange degenerate, not only the trajectory functions.

This experimental verification of exchange degeneracy applies to $m^2 > 0$. For $t=0$ exchange degeneracy for σ_{tot} works well, both for α and β . On the other hand, for $t < 0$, there is some evidence that exchange degeneracy is broken.

- c) Another reason why Collins et al.⁷⁾ obtain loops in K^+p is their assumption that exchange degeneracy holds only for the trajectories α but not for the residues β .

If one considers the coupled channel system $K\bar{K} \rightarrow p\bar{p}$, etc., one sees that there always exist some channels which break exchange degeneracy, e.g., $K\bar{K} \rightarrow \pi\pi$. One would then conclude that both the α 's and β 's are not degenerate. At this point Collins and Squires¹²⁾ use the quark model to argue that the α 's are degenerate, but not the β 's.

On the other hand, in a FESR bootstrap scheme it is rigorously possible to consider each of the coupled amplitudes separately. It immediately follows that the ρ and A_2 , as observed in $K\bar{K} \rightarrow p\bar{p}$, are exchange degenerate in both α and β . This conclusion has been checked experimentally for the Y^* 's in K^+p , see our preprint mentioned above.

- d) In their numerical analysis, Collins, Johnson and Ross ⁷⁾ find the disturbing result that some loops rotate clockwise. A true resonance always generates anti-clockwise loops.

The answer to this apparent problem is most easily given by quoting Collins et al., "... a single Regge trajectory... will usually produce a loop which goes round in an anti-clockwise direction..., with a combination of two or more trajectories, clockwise loops are readily produced". "Loops which are produced by an interference between the contributions of two or more trajectories are very small in general." This implies, and it is verified in the figures, that clockwise loops are very small. (There seems to be a special situation for s waves.) Since the Regge amplitude gives only a rough fit to the data, one should only trust the rough features of the resulting Argand diagrams, namely the large loops. But one should not trust fine details.

- e) We add a comment about the numerical work of Kreps and Logan ⁸⁾ who did the partial wave analysis of both helicity non-flip and flip for πN charge exchange. They get the negative result that the circles produced by ρ exchange are much too small and that there is hardly any correspondence between their circles and the experimental resonances.

The reason for this failure is that Kreps and Logan used as an input the old Regge fit by Logan and Sertorio ¹³⁾, which covered only the region $0 \geq t \geq -0.3 \text{ GeV}^2$. This (high energy) fit fails catastrophically for larger t values (at high energies). The fit for $d\sigma/dt$ is much smaller than the experimental value for $d\sigma/dt$. At $t = -0.6 \text{ GeV}^2$ the fit fails by a factor 3, at $t = -1.2 \text{ GeV}^2$ by a factor 45, at $t = -1.7 \text{ GeV}^2$ by 135, at $t = -2.3 \text{ GeV}^2$ by 480. It is clear why Logan and Sertorio's parametrization gives such a bad fit: in the expression $f \sim b(t) (E/E_0)^\alpha$ they choose $b(t) = \text{constant}$ and $E_0 = \mu_\pi = 0.14 \text{ GeV}$. But Höhler et al., and Arbab and Chiu ¹⁴⁾

have shown that the residue function in πN CEX are of constant order of magnitude (i.e., apart from zeros) if one chooses $E_0 \approx 1$ GeV. Alternatively a choice of $E_0 = \mu_\pi$ necessitates a strongly varying residue function.

4. No N^* resonances in $\pi^- p \rightarrow K^+ \Sigma^-$? Generalized exchange degeneracy.

Alessandrini and Squires ⁴⁾ consider the reaction $\pi^- p \rightarrow K^+ \Sigma^-$ and note that no meson exchange is possible (double charge exchange). The authors neglect the backward peak due to the exchanges of the various hyperons; of course this is only justified in a first approximation. In this first approximation the partial wave analysis of the Regge amplitudes gives zero. From this the authors conclude (wrongly) that this procedure predicts that no N^* resonances appear in the amplitude $\pi^- p \rightarrow K^+ \Sigma^-$.

Let us go back, for a moment, to the Finite-Energy Sum Rule for fixed t with resonance saturation at low energy. The absence of a Regge term in the $I_t = \frac{3}{2}$ channel ($K^+ \pi^+$) reduces the FESR to a superconvergence relation (SCR) and necessitates a cancellation between the N^* resonances

$$\int_{-N}^0 \dots d\nu + \int_0^{+N} \text{Im} [F_{\text{res}}(I_s = \frac{3}{2}) - F_{\text{res}}(I_s = \frac{1}{2})] d\nu = 0 \quad (1)$$

Each of the two integrals must (approximately) vanish separately, because of the next higher moment superconvergence relation. We see that the N^* resonances do not vanish, they merely cancel. If we are at low energies these N^* 's do not overlap and they do not cancel locally but only on the average. If we are at "intermediate energies", then, by definition, these N^* 's overlap strongly, and in a rough approximation they cancel even locally.

Generally we can say: whenever there is a force missing in the t or u channel, there will be a corresponding generalized exchange degeneracy, i.e., a relation between direct channel resonances.

Unfortunately we cannot yet test experimentally whether one has generalized exchange degeneracy for the $N_{\frac{1}{2}, \frac{3}{2}}^*$ in $\pi^- p \rightarrow K^+ \Sigma^-$ and the $\gamma_{0,1}^*$ in $K^- p \rightarrow \pi^+ \Sigma^-$.

5. Different Regge exchanges generating the same resonances?

Let us compare the two s channel processes $\pi\pi \rightarrow \pi\pi$ and $\pi\pi \rightarrow N\bar{N}$. The Regge behaviour of the former is governed by meson exchange of the latter by baryon exchange.

How can two different Regge poles (at high energies) give rise to the "same" set of resonances? ³⁾ (The couplings can be very different.)

FESR impose a connection between these two different high energy exchanges. According to Finite Energy Sum Rules and with the assumption of resonance saturation at low energies, the parameters of the two different Regge exchanges are determined by the low energy average over the same set of resonances. (This does not mean that the different Regge exchanges look the same, because the relative strength of the various resonances can be very different in different coupled channels.)

There is of course a qualitative difference between the well-established connection $LE \rightarrow HE$ and the connection $HE \rightarrow LE$. (LE = low energy, HE = high energy.) The former uses the FESR and is a connection on the average over low and intermediate energies. The latter uses the partial wave analysis and is a local connection at intermediate energies. However, if the resonances are strongly correlated and overlapping (as they are experimentally at intermediate energies) then the averaging process does not destroy much, and the

connection $LE \rightarrow HE$ can be reversed. Under these circumstances the well-established connection $LE \rightarrow HE$ implies that also the connection $HE \rightarrow LE$ will work approximately.

A closely related point has been explicitly tested by Rubinstein et al.¹⁵⁾: they performed the partial wave analysis in the three coupled channels $\pi\pi \rightarrow \pi\omega$, $\pi\pi \rightarrow \pi H_{\lambda=0}$, $\pi\pi \rightarrow \pi H_{\lambda=1}$, where λ denotes the helicity of the H . They obtained the direct channel resonances at the same position in all three channels although the spin structure is very different in the three channels. At first sight this might look like an accident since the fact that these three channels are coupled had not been explicitly incorporated. But, as we explained above, this is not an accident since the three different Regge terms are generated from the same set of direct channel resonances via FESR.

Obviously, more work must be done in connection with external particles of high mass and/or spin in order to investigate to what extent a local duality holds in those cases at moderate centre-of-mass energies.

6. A new form of the interference model?

- a) Alessandrini, Amati and Squires⁶⁾ postulate that the loops generated by the Regge exchange have nothing to do with direct channel resonances. They are therefore faced with two types of loops appearing in the peripheral partial waves, the loops coming from Regge exchanges, and the loops coming from the well-established resonances (P_{33} , D_{13} , F_{15} , F_{37}). They propose that these loops must be added in order to obtain the experimental loops; in contrast we proposed that the loops obtained from Regge are approximations to the loops generated by the resonance.

$$\begin{aligned} \text{ref. 6} \quad \left(\begin{array}{c} \text{experimental} \\ \text{loop} \end{array} \right) &= \left(\begin{array}{c} \text{genuine} \\ \text{resonance} \\ \text{loop} \end{array} \right) + \left(\begin{array}{c} \text{Regge} \\ \text{loop} \end{array} \right) \\ \text{ref. 1} \quad \left(\begin{array}{c} \text{"} \\ \text{"} \end{array} \right) &\approx \left(\begin{array}{c} \text{"} \\ \text{"} \end{array} \right) \approx \left(\begin{array}{c} \text{"} \\ \text{"} \end{array} \right) \end{aligned} \quad (2)$$

Alessandrini et al., propose to consider the partial wave projection of the Regge amplitude as a background to which the genuine resonances must be added. Since this "background" has a strongly varying phase and magnitude, the parameters of the resonances must be very different from the parameters determined in the isobar model, because the latter uses a constant or slowly varying background. (3)

We shall now show that the resonance residues determined à la Alessandrini et al., in different channels will be inconsistent. As a first example we compare $\pi^- p \rightarrow K^+ \Sigma^-$ with $\pi^- p \rightarrow K^0 \Sigma^0$. In the first reaction no meson Regge pole exchange is possible (no double charge exchange; we neglect for brevity the backward peak). Therefore there is no "background" which could affect the determination of the resonance parameters, and the N^* residues determined à la Alessandrini et al. will be the same as the residues determined in the isobar model. On the other hand, in the second reaction we have K^*-K^{*0} Regge exchange. It provides, according to Alessandrini et al., a strongly varying background which affects the determination of the resonance parameters. Therefore, their resonance residues will differ from the residues in the isobar model where the background is assumed to be constant or slowly varying. But the N^* residues in the two reactions must be the same apart from an SU_2 Clebsch-Gordan coefficient. Therefore, either the isobar model or the Alessandrini ansatz leads to a contradiction. The situation for $\sigma_{tot}^{(\pm)}(\pi N) \equiv \frac{1}{2} [\sigma_{tot}(\pi^- p) \pm \sigma_{tot}(\pi^+ p)]$

is similar and easily accessible for experimental tests.

From a duality point of view one must not forget that local duality at a given energy does not work equally well in all reactions. In our case it works better for $I_t = \frac{1}{2}$ than for $I_t = \frac{3}{2}$. (In πN non-flip it works better for $I_t = 0$ than for $I_t = 1$.) In these cases where duality does not work well locally but only after some averaging, Alessandrini et al., agree with the isobar model. In the cases where duality works well locally, the two models disagree.

Of course one can always avoid any explicit inconsistency by working directly with $\sigma_{\frac{1}{2}}$ and $\sigma_{\frac{3}{2}}$, where the subscript refers to the isotopic spin in the direct channel.

- b) The ansatz of Alessandrini et al., leads to the consequence that: "The sums over s and u channel poles must be superconvergent by themselves". In the case of the forward non-flip $\pi^+ p$ amplitudes this implies that the well-established resonances (N, P_{33} , D_{13} , F_{15} , F_{37}), which all add up, must be cancelled by a set of new, yet undiscovered resonances with negative g^2 and an equally large combined strength. Moreover, this undiscovered set would have to be approximately in the same energy region, from threshold to $W = 1920$, otherwise only the lowest moment superconvergence relation could be satisfied, but not the next higher moment ¹⁶⁾. This undiscovered set would also have to occur for angular momenta $j(s)$ similar to the well-established resonances, otherwise the superconvergence relation would only be satisfied for $t = 0$, but not for $t \neq 0$. The available experiments definitely rule out these postulated resonances with negative g^2 .

- c) Alessandrini et al., state: "... our modified interference model... is readily suited... for bootstrap-like calculations". The opposite is true; in their model the direct channel resonances completely decouple from the exchanged Regge poles. The resonances cancel among themselves to give zero. Therefore it is impossible to use s channel particles in order to predict properties of t channel particles. But such a connection is at the root of any bootstrap.

7. Ancestors? The high ℓ behaviour.

Alessandrini, Freund, Oehme and Squires⁵⁾ do the partial wave analysis of the ρ Regge amplitude in the direct channel for $\ell \gg s$. They obtain loops similar to those obtained in Ref. 1) for $\ell \sim s$ and name them "ancestors". We shall explain why we think that these "ancestors" are spurious effects which should be absorbed in the error bars of the analysis.

- a) Consider the angular distribution of $\text{Im} B_\rho$ at $p_L = 1.4 \text{ GeV}/c$ ($W = 1880 \text{ MeV}$). It has two zeros. If we partial-wave analyze it we shall obtain a maximum for $\ell = 2$ independent of the exact shape of the angular distribution¹⁷⁾. The situation is very different for the ancestor, at $\ell = 6$. A partial wave analysis of our input with $\ell = 6$ gives a zero in a first approximation, because of the many cancellations. (E.g., the contribution of the forward peak alone to a_6 can cancel to give zero, or be positive or negative, just depending on the exact shape of the forward peak.)
- b) The first group of ancestors are roughly four units of angular momentum above the prominent peripheral N^* resonances, e.g., 1920, 2190, 2420. But the phenomenological phase shift analysts (Lovelace, Bareyre, etc.) have noted that already the partial wave one unit above the prominent sequence is very weak¹⁸⁾.

It is certainly impossible to extract information about $\ell = \ell_{\text{prominent}} + 4$ from experimental data, or from the Regge amplitude, which gives only a rough fit to the experimental data.

Since the high ℓ behaviour of the expression

$$a_\ell = \frac{1}{2} \int_{-1}^{+1} dz P_\ell(z) A(z) \quad (4)$$

depends on the unknown fine details of the angular distribution, it is more useful to consider, for high ℓ , the Froissart-Gribov definition:

$$a_\ell = \frac{1}{\pi} \int_{z_0}^{\infty} dz Q_\ell(z) \mathcal{D}_z A(z) \quad (5)$$

where

$$\mathcal{D}_z A(z) = \frac{1}{2i} [A(z+i\varepsilon) - A(z-i\varepsilon)]$$

At high ℓ expression (5) is dominated by the contribution from the nearest singularity ¹⁹⁾, here assumed to be the pole at $t = m_\rho^2$. This is just the Born approximation:

$$a_\ell \rightarrow a_\ell^{\text{Born}} \sim e^{-\zeta \ell} \quad \text{for } \ell \rightarrow \infty$$

$$\cosh \zeta = 1 + \frac{m_\rho^2}{2q^2} \quad (6)$$

On the other hand the ancestor circles go as

$$a_\ell(\text{ancestors}) \rightarrow \frac{1}{\Gamma(\ell)} \quad (7)$$

and are much smaller than the Born approximation.

Because the nearest singularity in t dominates for high ℓ , it is a very poor approximation to leave out this singularity. This was done by Alessandrini et al., ⁵⁾, see their Fig. 2b. Note that two amplitudes may look very similar in the

physical region (where we perform the numerical partial wave analysis), and all the same have different nearest singularities in t . On the other hand, Fig. 2a of Ref. 5) represents an amplitude with the correct pole at $t = m_\rho^2$.

A careful numerical partial wave analysis according to (4) of an analytic expression containing the singularity at $t = m_\rho^2$ must automatically give $a_\ell \sim a_\ell^{\text{Born}}$ for high ℓ . We therefore suggest that definition (4) be used for $\ell \lesssim \ell_{\text{prominent}} + 1$, and the Born approximation for $\ell \gtrsim \ell_{\text{prominent}} + 2$. Within the expected errors the two expressions join smoothly for $\ell \sim \ell_{\text{prominent}} + 2$ (20). The "ancestors" computed for $\ell \sim \ell_{\text{prominent}} + 4$ belong into the error bars introduced by the incomplete knowledge of the amplitude in the physical region.

We conclude that the possibility (b) enumerated by Alessandrini et al., is the correct one: there is a qualitative distinction between "ancestor" maxima and the prominent resonance circles; the former are much smaller than the expected error, the latter larger than the expected error.

8. Second sheet poles and the Froissart-Gribov definition. The large t behaviour.

- a) The Regge amplitude $s \alpha(t)$ does not contain poles (corresponding to resonances) in the second sheet at $s = m_{\text{res}}^2 - i(m\Gamma)_{\text{res}}$. In Ref. 1) we gave the example of the Γ function and its asymptotic expansion to illustrate that it is quite natural to have an asymptotic expression (Regge term) which is very good for real s , but very bad for $\text{Im } s \sim -(m\Gamma)$. We suggested that the presence of circles, for real s , should be trusted, while the absence of second sheet poles should be disregarded. Here we are going to give a clear-cut demonstration of the difference between real s and $\text{Im } s < 0$.

We assume that we have some expression which fits the true amplitude $a(s,t)$ for intermediate s and the whole angular region, 0° to 180° , $0 \geq t \geq -4q_s^2$, fairly well. (Whether this fit is a Regge fit does not matter in this context right now.) If we have a reasonable fit to $a(s,t)$ then we also have a reasonable fit to its low Legendre coefficients $a_\ell(s)$, where $\ell = \text{integer}$ and $s = \text{real}$.

$$a_\ell(s) \text{ for } \begin{array}{c} \underline{s = \text{real}} \\ \text{and } \ell = \text{integer} \end{array} \longleftrightarrow a(s,t) \text{ for } \begin{array}{c} \underline{0 \geq t \geq -4q_s^2} \\ \text{and real } s \end{array} \\ \text{("circles")}$$

Since the true partial wave amplitude contains sizeable resonance circles, our approximate partial wave amplitude must contain these resonance circles approximately.

In order to discuss 2nd sheet poles, i.e., the poles of $a_\ell(s)$ at $s = m_{\text{res}}^2 - i(m\Gamma)_{\text{res}}$, we consider the Froissart-Gribov definition of a_ℓ , Eq. (5). The poles of $a_\ell(s)$ are caused by the divergence of the integral, or in other words by the asymptotic behaviour of $\Theta_z A(z)$ ²¹⁾:

$$a_\ell(s) \text{ for } \begin{array}{c} \underline{s = m^2 - im\Gamma} \\ \text{and } \ell = \text{integer} \end{array} \longleftrightarrow a(s,t) \text{ for } \begin{array}{c} \underline{t \rightarrow \pm \infty} \\ \text{and } s = \text{real} \end{array} \\ \text{("2nd sheet poles")}$$

Because our input $a(s,t)$ is certainly bad for $t \rightarrow \pm \infty$, our output $a_\ell(s)$ is meaningless in the second energy sheet, where the resonance poles are ²²⁾. Therefore, the absence of 2nd sheet poles in our approximation is irrelevant. We cannot obtain any direct information about 2nd sheet poles from our partial wave analysis. On the other hand, we can trust our output $a_\ell(s)$ for real s , i.e., we can trust the circles, because our input $a(s,t)$ is reasonably good for $0 \geq t \geq -4q_s^2$.

If we know that there exists a genuine resonance pole, we can conclude that the loop generated by F_{Regge} is an approximation to the genuine resonance loop. [We certainly should not add the two loops as proposed in Ref. 6).]

- b) Can we deduce anything about the existence or non-existence of 2nd sheet poles by considering the Froissart-Gribov definition ⁷⁾? In order to carry out the integration we must know the asymptotic behaviour of $\mathcal{D}_z A(s, z)$ for fixed s and $z \rightarrow \infty$. Regge theory tells us that it goes like $z^{\alpha(s)}$. Therefore we must know, right at the start, what $\alpha(s)$ is; in other words, we must feed in knowledge about the leading trajectory of direct-channel resonances. But this is exactly what we wanted to get out at the end. Therefore, for our purpose here, the Froissart-Gribov definition is useless from a practical point of view. From a theoretical point of view we see that everything is consistent: if we feed in at the beginning that $\mathcal{D}_z A \sim z^{\alpha(s)}$ then the integration gives, among other terms, $a_\ell(s) \sim 1/(\ell - \alpha(s)) \sim 1/(s - m^2 + i\Gamma)$. We obtain therefore a genuine 2nd sheet pole at the end.
- c) We now consider a different question and ask: is it possible to modify the t dependence of the ρ Regge pole input, $F_\rho \sim s^{\alpha(t)}$, such that it has the correct behaviour for $t \rightarrow \infty$ at fixed s ²³⁾? If we could make this modification, then the partial wave analysis of F_ρ in the direct channel would not only give circles (corresponding to the N^* 's), but even genuine 2nd sheet poles. In that case the modified ρ Regge input would also have these 2nd sheet poles. We now prove that this modification of F_ρ is impossible. If it were possible, then $F_\rho \approx F$ for fixed s and $t \rightarrow \infty$; but this would just be the incorrect statement that one tower of t channel resonances is sufficient to give saturation for high energies t and an angle which goes to $z_t = +1$. Theoretically one tower $\alpha(t)$ is insufficient because it cannot build up the expected Regge behaviour $t^{\alpha(s)}$ for $t \rightarrow \infty$ and $z_t \sim +1$ ²⁴⁾. Experimentally we know that the high recurrences have very small elasticities, e.g., for $\sigma_{\text{tot}}(\pi N)$ the $N^*(3030)$ gives less than 1/10 of the measured cross-section.

In other words: one t channel partial wave at any given high energy t is insufficient to saturate the full imaginary part of the amplitude.

If we want a good representation of $B(s, t)$ for $s = \text{small}$ $t = \text{large}$, then we can use either (i) the leading s channel Regge pole $t^{\alpha_{N^*(s)}}$ or (ii) a large but finite number of t channel meson resonances (daughters) $j_t = 0, 1, 2, \dots, \alpha_p(t)$. If we let t become larger, the number of direct t channel resonances will become larger. As an input in our calculations we are not interested in (i) because we want the N^* 's to be the output, and we are not interested in (ii) because we want to feed in only the leading Regge pole F_p , but not the infinite number of daughters with $0 \leq j_t < \alpha_p(t)$ and $t \rightarrow \infty$. Veneziano²⁵⁾ has constructed an explicit model amplitude which exhibits this last point.

9. Conclusions

Let us distinguish two cases: (A) The peripheral partial waves, where we have genuine resonances (1520, 1688, 1910, 2190, 2420); (B) The central partial waves where resonances are not yet well established.

In case (A) there are clear-cut positions. Alessandrini et al.,⁶⁾ propose that the Regge loops have nothing to do with resonance loops (and the two types of loops must be added), while we maintain that the Regge loops are approximations to the genuine resonance loops.

In case (B), Refs. 3)-7) conclude from the fact that the Regge amplitude produces loops similar to those observed in phase shift analysis that there are probably no genuine resonances in these partial waves. On the contrary, we have explained in this article that there is no evidence against the resonance interpretation of

these loops. In particular, all arguments in Refs. 3)-7) (except the bump argument) apply equally well against the resonance interpretation in case (A) (well established N^* resonances), and even the bump argument is not good, because the Roper resonance does not produce a bump in πN elastic scattering, but shows up as a beautiful peak in the missing mass experiment $p+p \rightarrow p+MM$ at high energies and very small t . On the other hand we cannot give conclusive evidence for the resonance (2nd sheet pole) interpretation, because we have to stick to real energies in our analysis.

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- 9) This implies that the N^* 's are highly correlated, e.g., all prominent N^* 's (1520, 1688, 1920, 2190, 2420) have their first zero in B_{CEX} at approximately the same t value, $t \approx -0.5 \text{ GeV}^2$.
- 10) It is for this reason that this method (partial wave projection) is not useful for bootstraps: one must feed in more information about the t channel poles than just their masses and widths, one must feed in e.g., the position of zeros like those of A' at $t \approx -0.1, -1.2$, etc. In contrast the FESR bootstrap is symmetric with respect to input and output: we feed in the masses and widths of the t channel poles and get out the masses and widths of the s channel poles; see C. Schmid, Phys.Rev.Letters 20, 628 (1968).
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- 16) Since both Alessandrini et al., and we assume that the Regge amplitude is a good approximation above some energy in the range 3.5 - 6 GeV/c, the SC integral can be cut off somewhere in this range. The first two moment SCR tell us that resonances with positive g^2 and those with negative g^2 must cancel semi-locally, e.g., in the regions 0 - 2 GeV/c and 2 - 4 GeV/c separately. In other words: it is impossible to push the negative g^2 resonances to very high energies.
- 17) $\ell = 2$ corresponds to an F wave, since $B \sim f_2 = \sum \dots P'_\ell$.
- 18) P. Bareyre et al., *Phys.Rev.* 165, 1730 (1968), note that in all their solutions the G wave at the position of the F_{15} resonance is weaker than 3^0 .
- 19) Since we have $Q_\ell(z) \sim \ell^{-\frac{1}{2}} z^{-\ell-1}$ for $\ell \rightarrow \infty$.
- 20) This is illustrated in Fig. 2a of Alessandrini et al., ⁵⁾.
- 21) The divergence of the integral for real s and $t \rightarrow \pm \infty$ produces a resonance pole in $a_\ell(s)$ for real s and complex ℓ at $\ell = \alpha_i(s)$. Continuity tells us that the same pole shows up for real ℓ and complex s at $s = s_i(\ell)$.
- 22) This point has been emphasized by C. Lovelace in a talk at a parallel session of the Vienna Conference.
- 23) F_ρ means the amplitude corresponding to the leading ρ trajectory (parent trajectory). By our definition F_ρ does not contain daughter poles.

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