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ARE THERE ANY FAR-OUT MESONS OR BARYONS?

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Berkeley, California

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To save the time of readers who might look here for positive evidence, let me immediately state my answer to the question in the title: There is no evidence for any "far-out" mesons. I will show that the number of claims published corresponds reasonably well to the number of statistical fluctuations that one would expect. If far-out mesons are being produced, their cross sections are down to the 10-μb level. Some evidence for complicated baryon supermultiplets has been seen in K^+p and K^+n interactions. Even though this is mainly a paper on mesons, I will discuss this and warn that the evidence is still in some doubt.

In this paper I shall discuss the following:

I. Properties of "Far-Out" Mesons ($|Q|$ or $|I| \geq \frac{3}{2}$; cannot be formed from quark-antiquark pairs). Page 2

II. Absence of Indirect Evidence; we see no evidence for the exchange of virtual far-out mesons. Page 3

III. "Every Year We Must Expect Several 4σ Fluctuations in Some Channel." What can the victim of such a fluctuation do? Page 3

IV. Absence of Direct Evidence. Summary of Far-Out Spectra and Claims. A plea for more responsible reporting. Page 13

V. Status of Far-Out Baryons: $K^+n \rightarrow Z_0(1865)$, $K^+p \rightarrow Z_1$ (1900). Neither resonance is sure. (I apologize for changing the subject to baryons) Page 20

VI. Explaining Peaks vs. Explaining them Away. Page 26

I. PROPERTIES OF FAR-OUT MESONS

If mesons are "formed" out of quark-antiquark pairs then they can belong only to the 8 and 1 representations of SU(3), and can never be found more than one step from "home", i.e. the origin of weight space, as shown in Fig. 1.

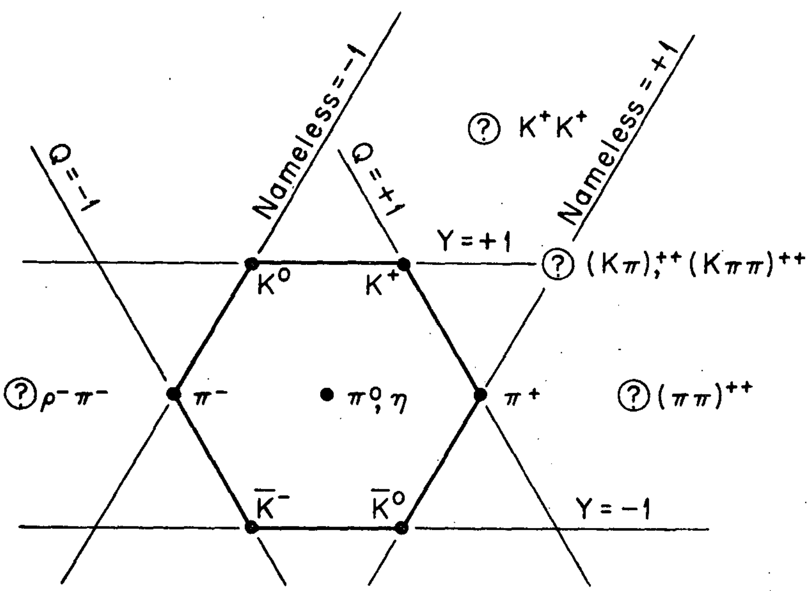

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Fig. 1. The familiar octet bounded by $|Y| = 1$, $|Q| = 1$ (and a third set of nameless lines along which V-spin happens to be constant). We shall discuss the ?'s outside the hexagon. The labels π, η, and K are, of course, just examples. We could have chosen ρ, ϕ, K^*, ··· instead.

This means that they must lie on (or within) the walls of a hexagonal citadel bounded by $|Y| = 1$, $|Q| = 1$, and a third pair of lines of constant V spin.

Great interest than attaches to evidence for any meson with $|Y|$ or $|Q| \geq 2$.[1] These candidates have been called "exotic."[2] I mildly prefer "far-out" because it combines the slangy connotation of the unusual with the idea that these mesons would be far-out in weight space.

The search for far-out particles has been discussed recently by G. Goldhaber.[2] This makes my job easier, and I want first simply to copy an idea.

II. ABSENCE OF INDIRECT EVIDENCE, i.e. ABSENCE OF EXCHANGE OF VIRTUAL FAR-OUT MESONS

As is well known, two-body reactions in the BeV region lead in general to both forward (meson exchange) and backward (baryon exchange) peaks, unless the exchange of a virtual far-out resonance is required, in which case the corresponding peak disappears. In his Hawaii lectures[2] Goldhaber introduces some nice figures supplied by Barger to illustrate this point. Barger chose four reactions listed in Table I, and reproduced here in Fig. 2. Note that the forward peaks, if allowed, run 300 μb/sr, the backward ones run 5 to 10 μb/sr. Hence the suppression is particularly dramatic for the forward (meson-exchange) peaks. Where a far-out meson would be needed, the "peak" is 1 μb/sr or less, i.e., suppressed by a factor of 300.

III. EXPECT SEVERAL 4σ FLUCTUATIONS PER YEAR

Before we go on to survey far-out mass spectra where bumps have been reported in $(K\pi\pi)_{3/2}$, $(\pi\rho)^{--}$, ... we should first decide what threshold of significance to demand in 1968. I want to show you that although experimentalists should probably note 3σ effects, theoreticians and phenomenologists would do better to wait till the effect reaches $> 4\sigma$. (Note that doubling the counts on a real 3σ peak should increase its significance to $3\sqrt{2}\sigma \sim 4.25\sigma$, so I am not suggesting an impossibly long wait.)

In appendices to our January 1967 compilation of particle properties,[3] we presented a collection of histograms in which we tried to kill the kappa and H mesons. We wounded both but killed neither, and ourselves learned something of the statistical problems that arise when each year bubble chamber physicists numbering nearly a thousand (if you include graduate students) hunt through ten-thousand mass distributions in search of striking features, either real or statistical fluctuations.

A. TOTAL FLUCTUATIONS EXPECTED (ALL CHANNELS, ALL MASSES). The number of "potential resonances"

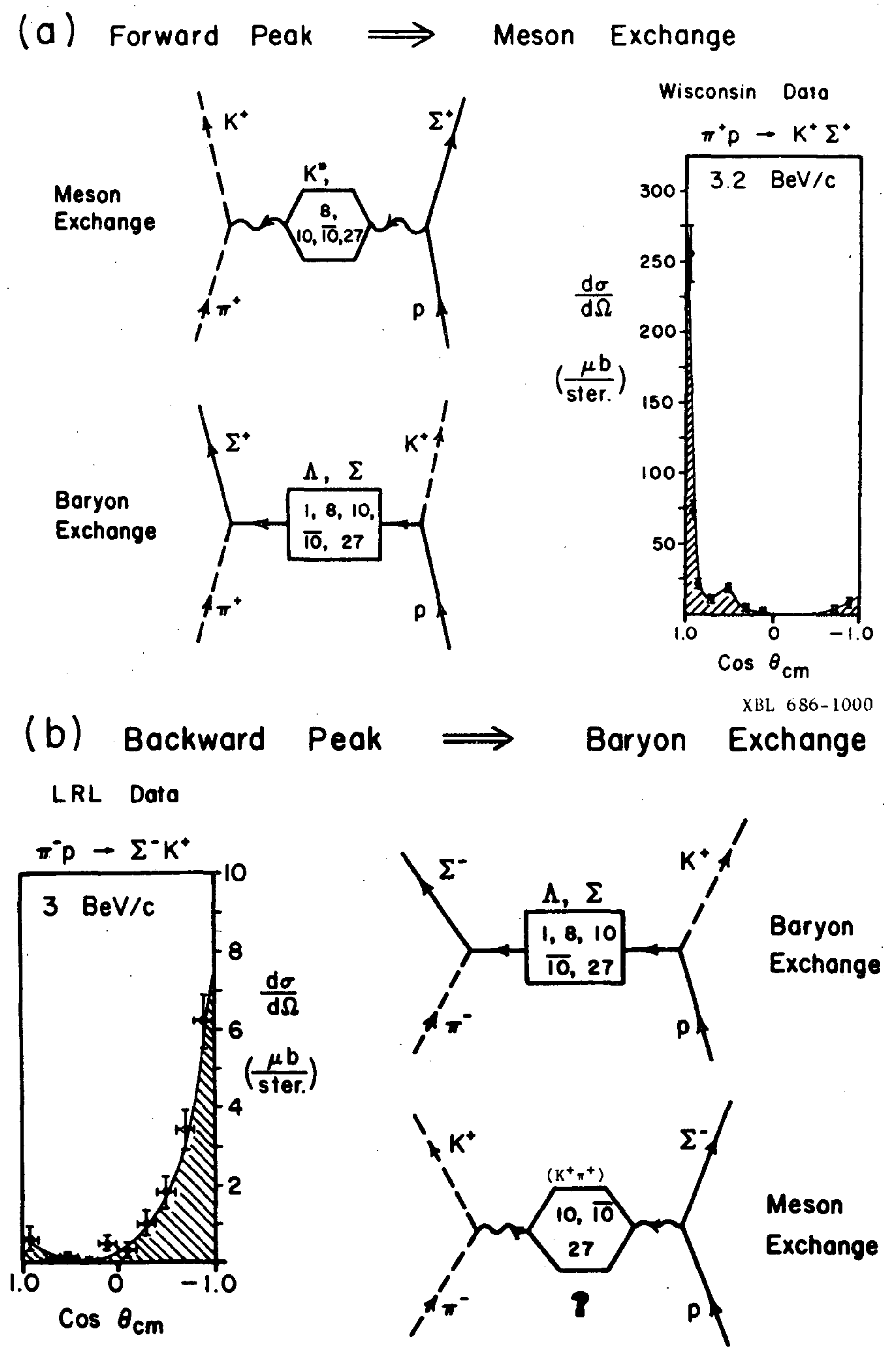

Fig. 2. Barger's summary of forward and backward peaks arising from exchange of virtual particles.

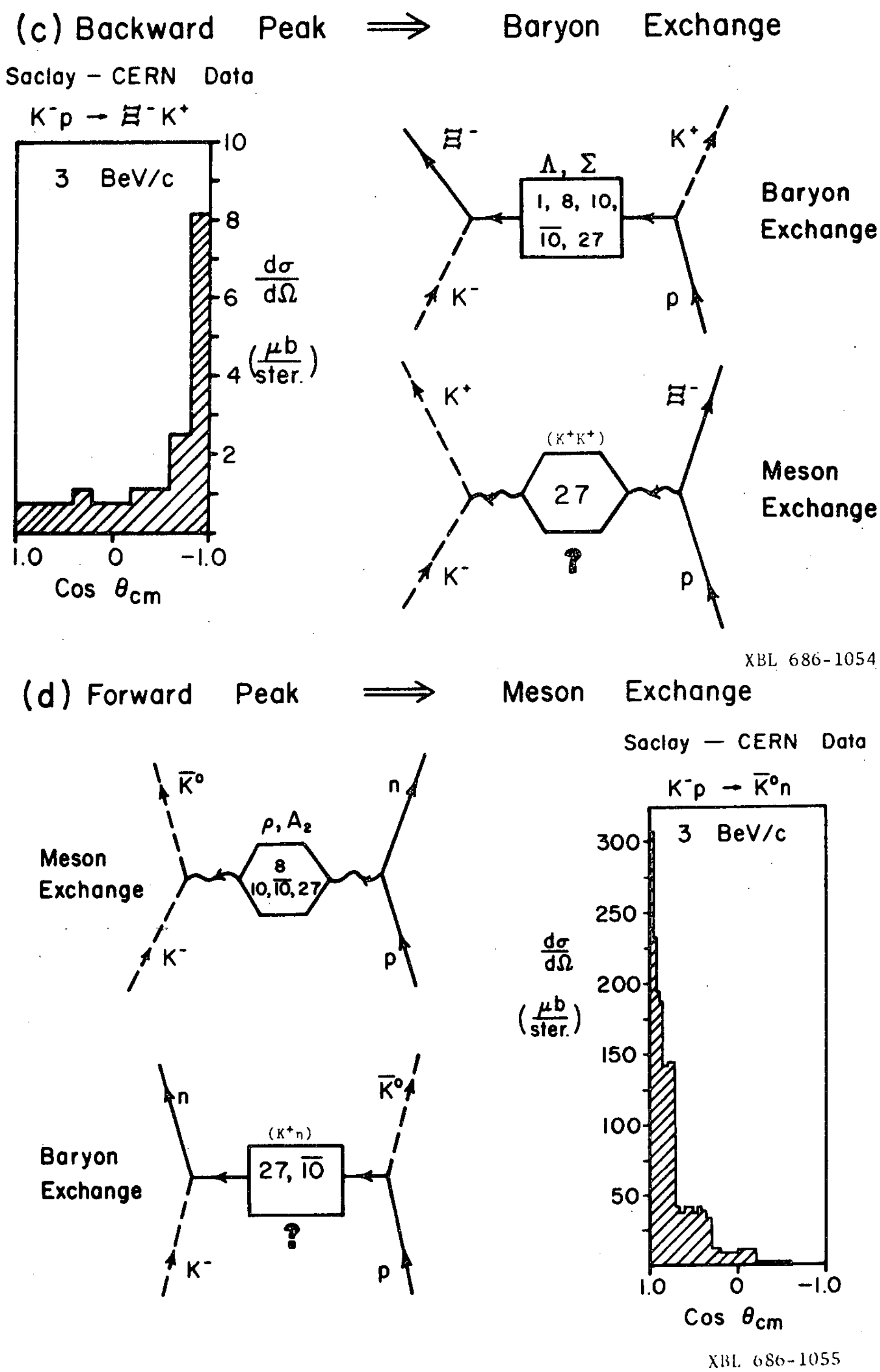

Fig. 2 (continued). The reactions are listed in Table I.

Table I.

Reactions shown in Fig. 2, which shows forward and backward peaks. 2b and 2c show backward peaks only; 2d shows a forward peak only.

		Q and Y carried by exchanged						
		Meson			Baryon			Peaks
Fig.	Reaction	Y	Q	Example	Y	Q	Example	expected
2a	$\pi^+ p \rightarrow K^+ \Sigma^+$	1	0	K^*	0	0	Λ	both
2b	$\pi^- p \rightarrow K^+ \Sigma^-$	1	2	far-out	0	0	Λ	backward
2c	$K^- p \rightarrow K^+ \Xi^-$	2	2	far-out	0	0	Λ	backward
2d	$K^- p \rightarrow K^0 n$	0	1	ρ	2	1	far-out	forward

scanned by physicists each year is the product of the following:

Number h of histograms plotted. (I show in 1 below that this is on the order of 15,000 i.e. $h \sim 15{,}000$.

Number f/h of possible deceptive fluctuations in each histogram. (I show in 2 below $f/h \geqslant 10$.)

The product $h \times b/h$ is then $> 150{,}000$. But a 4σ upwards fluctuation should happen once every 32,000 potential bumps, and therefore once every few months! Let me now justify these numbers.

1. Number h of histograms plotted each year. E. C. Fowler, R. J. Plano, and I run an annual survey of bubble-chamber data processing, and Table II gives some recent data. It shows that about 1.4 million events were completely measured in the United States in the year ending August 1967. If we make a wild guess of another 50% measured elsewhere, we find about 2 million measurements mainly of events with four outgoing prongs.

If such a 4-prong event gives a 4-constraint fit (no missing particles) there are 10 mass combinations to calculate (six 2-body + four 3-body). But if it gives a 1c fit (missing neutral) or a 0c "fit" (more than one missing neutral) then the number of mass combinations of five 4-vectors shoots to 25 (10 + 10 + 5). In this discussion I shall assume that half the 4-prongs require a missing neutral, so that on the average there are 17 mass combinations per event.

This reasoning on multiplicities, extended to all combinations of all outgoing particles and to all countries, leads to an estimate of 35 million mass combinations calculated per year.

How many histograms are plotted from these 35 million combinations? A glance through the journals shows that a typical mass histogram has about 2,500 entries, so the number we were looking for, h, is then 15,000 histograms per year. (Our annual survey also tells us that the U. S. measurement rate tends to double every two years, so things will get worse.)

2. Number f/h of bumps/histogram. Our typical 2,500-entry histogram seems to average 40 bins. This means that therein a physicist could observe 40 different fluctua-

Table II.

Hydrogen bubble chamber events measured in U. S. in year ending August 1967 (excluding about 300,000 image-plane-digitizer measurements made to study Σ leptonic decay).

Outgoing prongs	Outgoing particles	Number of mass combinations		Events measured (thousands)	Mass combinations (millions)
2	2 or 3	1, 3	avge = 2	500	1
4	4 or 5	10, 25	avge = 17	1200	21
6	6 or 7	56, 119	avge = 88	70	6
Total U. S.:				~1,700	~28
Assume 20% were remeasurements:				~1,400	~23
×1.5 (?) to include other countries:				~2	~35
Divide by 2500 events/histogram; yields 15,000 histograms.					

tions one bin wide, 39 two bins wide, 38 three bins wide, ··· . This arithmetic is made worse by the fact that when a physicist sees "something": he then tries to enhance it by making t-cuts, looking both inside and outside N^* bands, selecting on ρ bands, etc. Fortunately however, the arithmetic seems to be made far better by the exercise of considerable judgement and restraint. Thus most physicists are properly skeptical of fluctuations which are only one bin wide, particularly if their resolution is comparable with one bin. Thus most physicists look for some other supporting evidence of a resonance -- some change in some angular distribution for instance. This fortunate imposition of good judgement is unfortunate only in that it makes it impossible for me to make a good estimate of the desired fraction f/h.

My colleague Gerry Lynch has instead tried to study this problem "experimentally" using a "Las Vegas" computer program called Game. Game is played as follows. You wait until an unsuspecting "friend" comes to show you his latest 4σ peak. You draw a smooth curve through his data (based on the hypothesis that the peak is just a fluctuation), and punch this smooth curve as one of the inputs for game. The other input is his actual data. If you then call for 100 Las Vegas histograms, Game will generate them, with the actual data reproduced for comparison at some random page. You and your friend then go around the halls, asking physicists to pick out the most surprising histogram in the printout. Often it is one of the 100 phoneys, rather than the real "4σ" peak. Figure 3 shows two Game histograms, each one being one of the more interesting ones in a run of 100. The smooth curves drawn through them are of course absurd; they are supposed to be the background estimates of the inexperienced experimenter. But they do illustrate that a 2σ or 3σ fluctuation can easily be amplified to "4σ" or "5σ"; all it takes is a little enthusiasm.

In summary of all the discussion above, I conclude that each of our 150,000 annual histograms is capable of generating somewhere between 10 and 100 deceptive upward fluctuations; to be conservative, I used the number 10 for the number f/h.

Then, to repeat my warning at the beginning of this section; we are now generating at least 100,000 potential bumps per year, and should expect several 4σ and hundreds

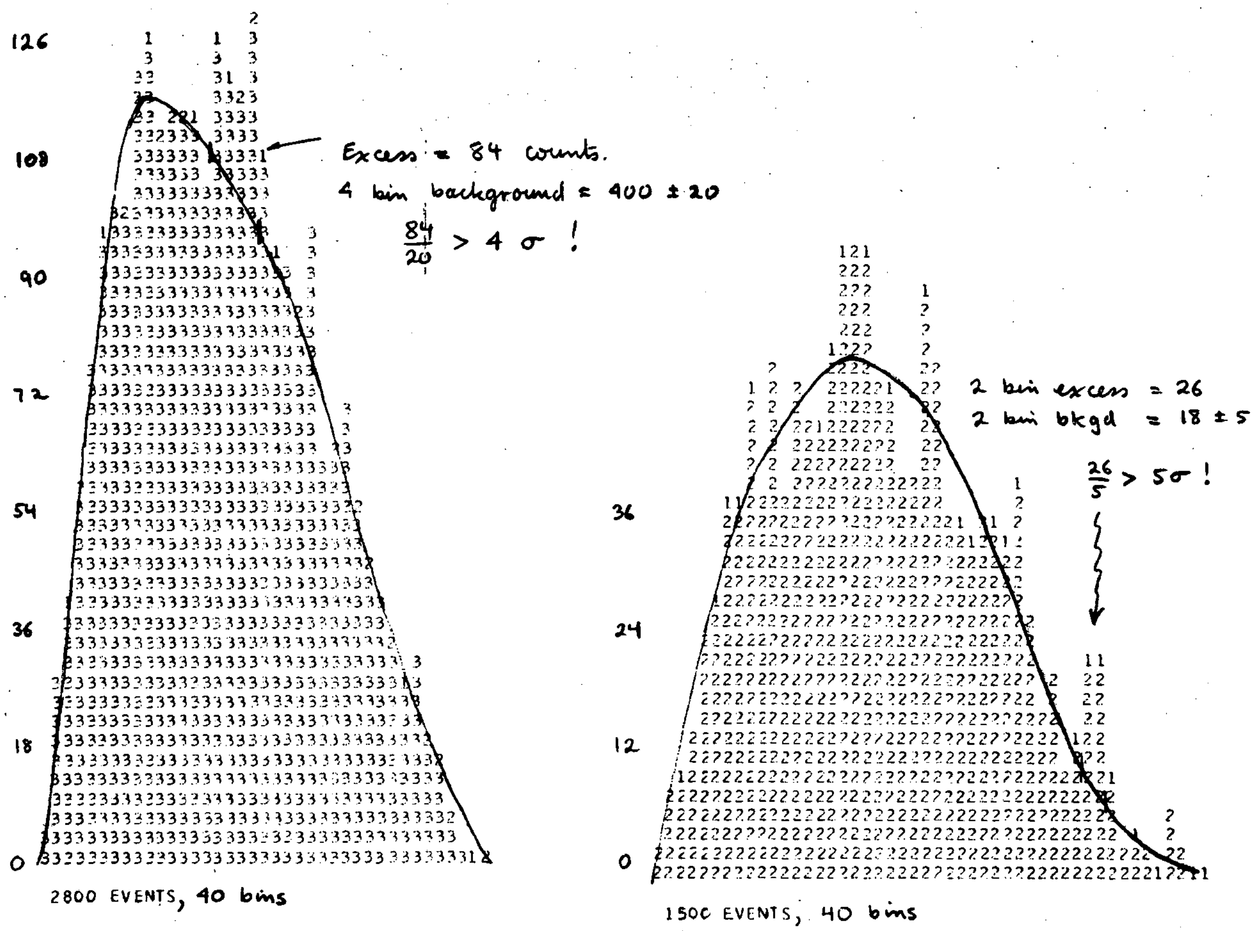

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Fig. 3. Two "Las Vegas" histograms generated by G. Lynch's program GAME.

of 3σ fluctuations. What are the implications? To the theoretician or phenomenologist the moral is simple; wait for nearly 5σ effects. For the experimental group who have just spent a year of their time and perhaps a million dollars, the problem is harder. I suggest that they should go ahead and publish their tantalizing bump (or at least circulate it as a report.) But they should realize that any bump less than about 5σ constitutes only a call for a repeat of the experiment. If they, or somebody else, can double the number of counts, the number of standard deviations should increase by $\sqrt{2}$, and that will confirm the original effect.

In connection with this point, Paul Murphy pointed out at the conference most experimentalists look at their data by the time that half the events are measured anyway. Murphy suggested that they publish their data as two independent experiments, so that one could confirm or deny the other. After some thought I conclude that splitting the data does no good. On the other hand, as I have just said; if you have a 4σ effect on half your data, it had better increase to $4\sqrt{2}\sigma$ by the time it is all processed; otherwise the effect is fading away.

B. FLUCTUATIONS EXPECTED IN ANY SINGLE BIN: e.g. THE KAPPA. Here it is easier to estimate the number of potential kappa peaks scanned each year. About 1/4 of all events measured seem to have $\geqslant 3$-body final states with at least one K and one π. There are probably two $K\pi$ combinations in these events. Table II tells us that we are producing about a million $K\pi$ mass combinations per year. As before, we gather them into histograms containing about 2500 $K\pi$ combinations, so we generate about 400 histograms annually. Each of these contains only one bin at the kappa mass, so each year we scan 400 potential kappas and should expect about one 3σ effect. It is my impression that this agrees with the rate of new claims for the kappa.

Let me also summarize the past history of the kappa. When it first appeared it was more convincing than many other resonances that have since weathered the tests of time. But the kappa has not been satisfactorily corroborated and by now we had best assume that it was a spurious fluctuation. I copy the thoughts from our 1967 study.[3] So far there have been five impressive bumps, and a number of smaller ones. But every time an experiment has been run to confirm a kappa claim, it has failed. At the same time (if it is a large statistics experiment) the second ex-

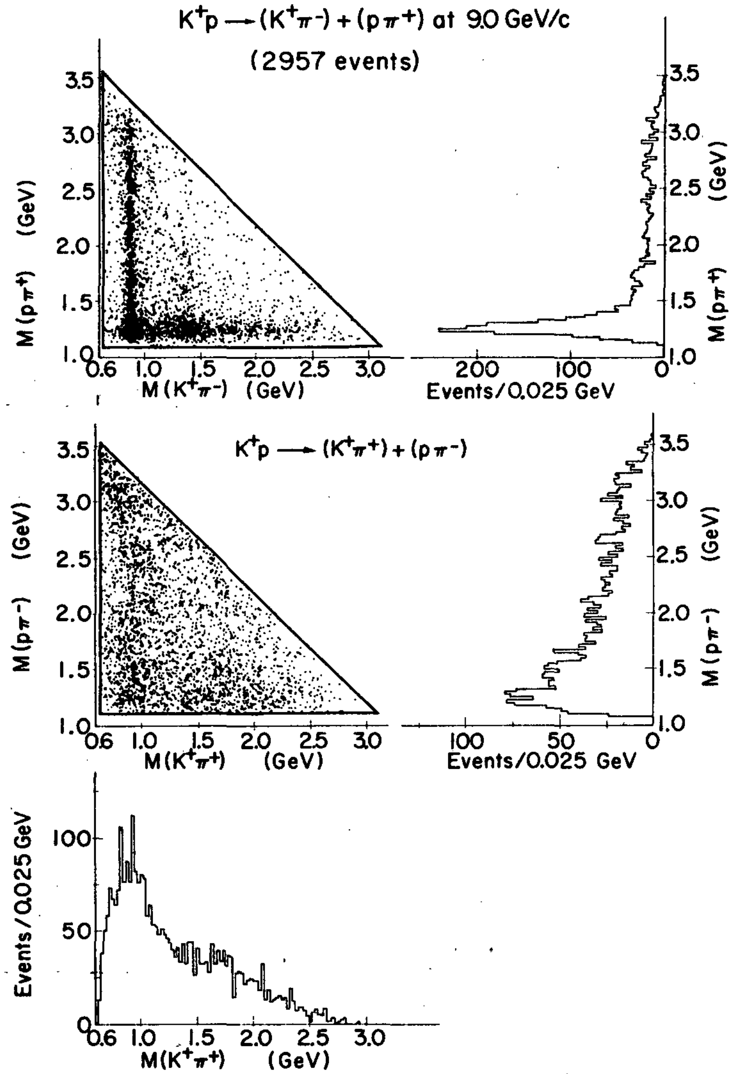

Fig. 4. (a) $(K\pi\pi)^{++}$ (and other) spectra from 9-GeV/c $Kp\pi^+\pi^-$. The path length in this typical experiment corresponds to ~ 5 events/μb. This figure should serve as a warning: It shows that there are no significant bumps with cross sections corresponding to more than ~ 10 μb in either K^+p or $p\pi^-$ [with the exception of $\Delta^0(1236)$]. But notice that in the $p\pi^-$ state, which can be better studied in the s channel, we now know there are about a dozen resonances! (Figure from Ref. 12.)

periment often reports the kappa in a different channel. The result is that the kappa's existence currently depends entirely on three uncorroborated "sightings." This strikes me as similar to the problem with flying saucers; they keep appearing, but always in different places, and can never be reproduced. It seems to me that we are going to have to learn not to be fooled by frequent large fluctuations.

IV. ABSENCE OF DIRECT EVIDENCE FROM MASS SPECTRA

I will now survey briefly the four far-out spectra noted in Fig. 1, and along the way make some complaints about the partisan way that claims are presented, and call for less-biased, more-helpful reporting.

A. $(\pi\pi)^{\pm\pm}$ AND $(K\pi)^{++}$. To my surprise I find among these simple spectra no suggestion of the production of any resonances with cross sections greater than 10μb. For an example, see Fig. 4a.

I think there is a need for a high-statistics spectrometer study of the reaction $\pi^+ p \to n\, X^{++}$, in the same way that Maglic group at CERN pioneered studies of $\pi^- p \to p\, X^-$.

B. $\rho^-\pi^-$ (1320, $\Gamma = 150$). This peak was reported a year ago by Vanderhagen et al.[3a] in the reaction 5 GeV/c $\pi^- d \to pp\pi^-\pi^0\pi^-$. Figure 4b gives their $\rho^-\pi^-$ spectrum. They report a 4σ bump (it looks more like 3σ to me) of 39 events above background, corresponding to a 15-μb signal. As I discussed in Sec. III, the only conclusion I can draw is that the experiment should be repeated. A lower-energy experiment (2.26 GeV/c) has recently been reported by Benvenuti et al. at the 1968 Spring Meeting of the APS in Washington, D. C.[3b] They have about the same number of events as reported by Vanderhagen et al. but their spectrum ends at 1300 MeV, so they have nothing to say about the Vanderhagen bump. They point out $(\rho\pi)^{--}$ bumps at 970 and 1180 MeV, but I feel that the valley between these peaks is merely another 2σ fluctuation, enhanced slightly by enthusiastic ρ selection. (It is well known that selection of ρ's from a 3π spectrum peaks up this spectrum at 1000 and 1300 MeV. This was first pointed out by Benson et al.[10] and has recently been very clearly demonstrated by Fung et al.[11])

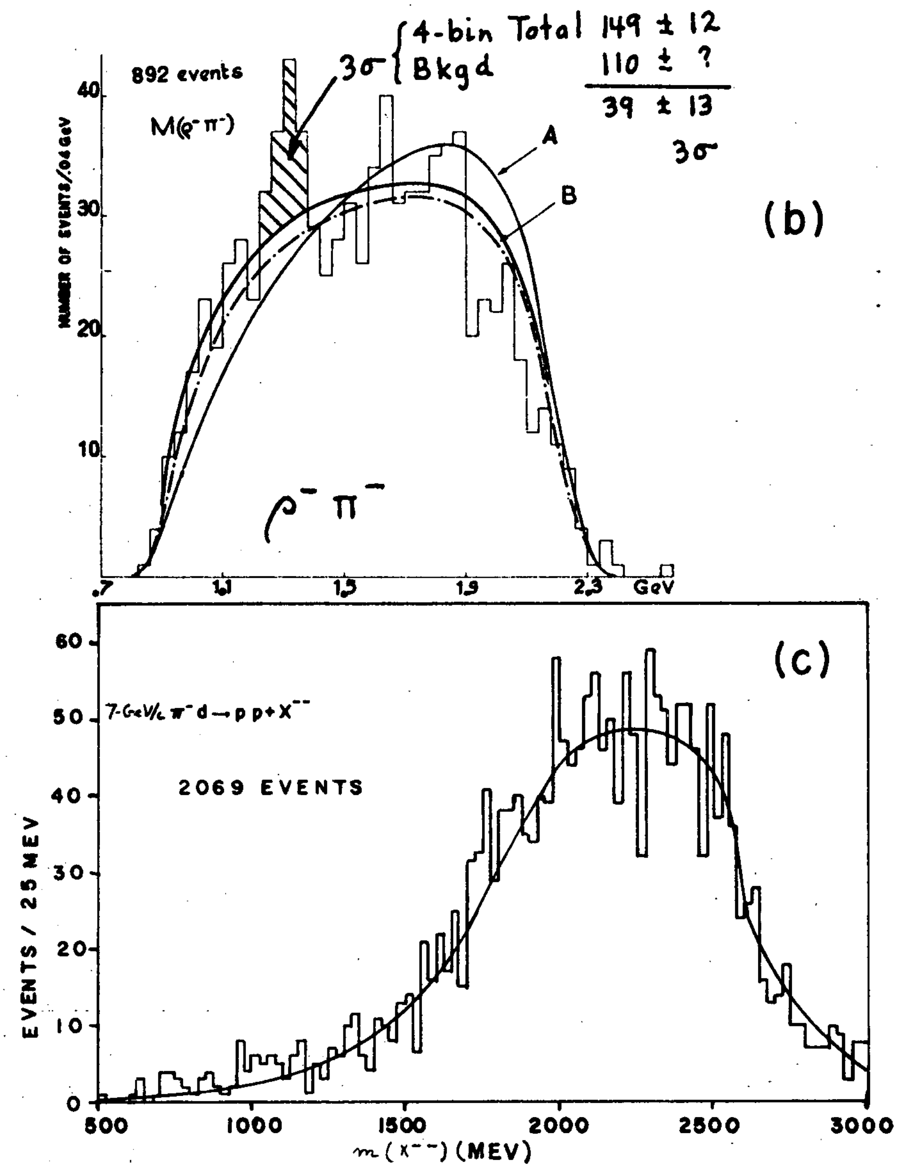

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Fig. 4. (b) Peak in $\rho^-\pi^-$ from 5-GeV/c $\pi^-d \rightarrow pp\pi^-\pi^0\pi^-\Sigma$. Curve A is phase-space normalized to all events. Curve B is 80% phase space, 20% Deck effect, and is normalized outside the $\rho^-\pi^-$ peak. (From Vanderhagen et al., Ref.3a.) (c). Mass spectrum for doubly charged meson system X^{--} produced in the reaction $\pi^-p \rightarrow ppX^{--}$. Any bump present corresponds to < 8 μb. (From Katz et al., Ref. 3b.)

C. OTHER DOUBLY CHARGED SPECTRA. These spectra show nothing above 10μb. As an example, I reproduce in Fig. 4c the spectrum shown by Katz et al. (Rochester) at the 1968 spring meeting of the APS in Washington, D. C.[3b]

D. $(K\pi\pi)_{3/2}$ (1270) AND $(K\pi\pi)^{++}$(1120).

1. $(K\pi\pi)_{3/2}$ (1270, Γ = 60). There have been two sets of claims for this resonance. It was originally seen by Böck et al. (CERN) in 3- to 4-GeV/c $\bar{p}p$ reactions, but not seen by Baltay et al. (Yale, BNL) at 3.7 GeV/c. In my rapporteur's talk at the September 1965 Oxford conference[4] I combined the latest CERN spectrum (their original 182 events had by then risen to 257) with the Yale distribution, and found the sum rather unimpressive.

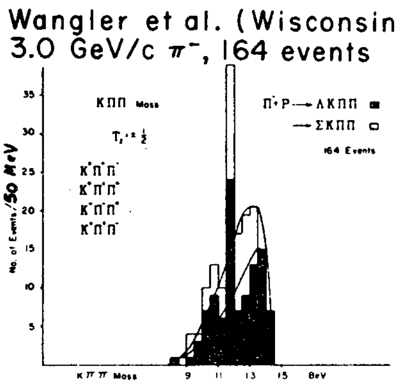
In addition there was short-lived evidence for a bump in the $K^+\pi$ spectrum from 3.0 GeV/c $K^+p \rightarrow \Delta K^*\pi$. In this 1966 Berkeley rapporteur's talk,[5] Goldhaber announced that this too has washed out.

What may seem new is a second paper by French et al. (CERN, Birmingham).[6] This is the original group that published as Böck et al. This paper presents a (another?) $K^*\pi$ peak from 3- to 4-GeV/c $\bar{p}p$. It does not warn of any conflicting data -- it mentions neither my Oxford talk nor Goldhaber's Berkeley talk. But don't be confused, the events are just the same ones that they gave me to include in Fig. 62 of my Oxford talk.

2. $(K\pi\pi)^{++}$(1170). Here we come to a set of five independent claims, which might tend to suggest to the reader that there is really a resonance, but I doubt it. The early claims are summarized in Fig. 5, which is page 52 of my Oxford talk.[4] It shows the original evidence of Wangler et al. (Wisconsin),[7] which is certainly striking, and some possible corroboration by Miller et al. (Purdue).[7] However it also shows the spectra of Hardy et al.[7] (LRL), which fail to confirm the bump. The lower right-hand curve is the sum of the data available in 1965. At that time there was no suggestion that the I spin might be unusual.

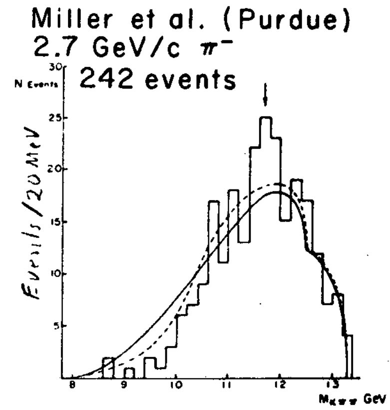
Next, Bishop et al. (Wisconsin)[7] discovered something of a bump in $(K\pi\pi)^{++}$ produced by 3.5-GeV/c K^+p reactions. And now there is a preprint by Goshaw et al.[6] (Wisconsin)[7] which seems to be the data of Bishop et al. increased by 5/3. Figure 6 is the relevant page of their pre-

Figure 51



Invariant mass histogram of the $K\pi\pi$ system produced with Λ's and Σ's. The upper solid curve is phase space and the lower solid curve is phase space, modified for a Y_1^*. The Λ events are shaded and the Σ events are unshaded.

Figure 53



$K\pi\pi$ mass spectrum for 242 events of the type $YK\pi\pi$.

Figure 52

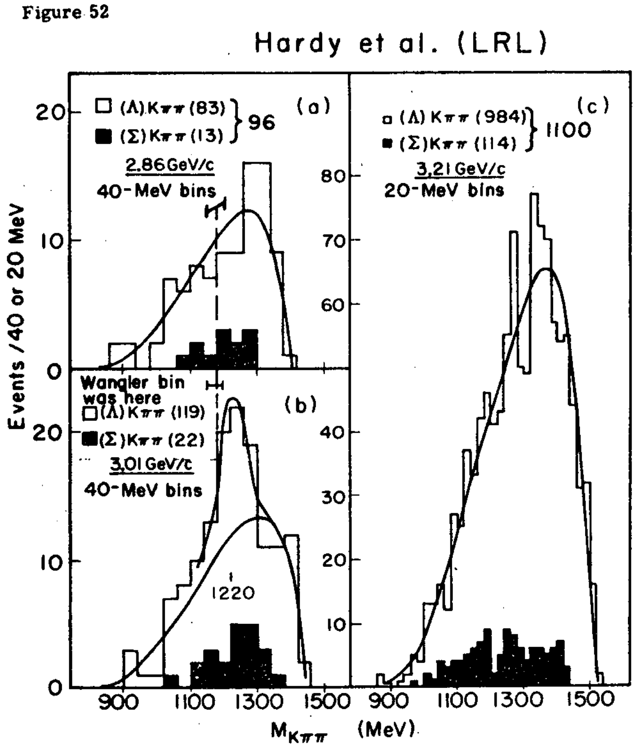
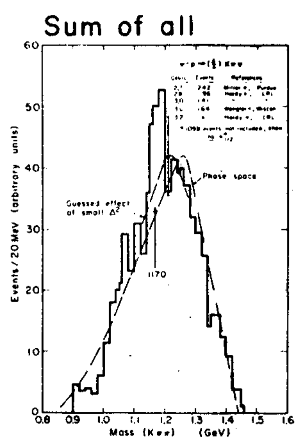


Figure 54



Figures 51, 52, 53, 54. $K\pi$ spectra giving evidence for and against a bump at 1170 MeV; Fig. 51 from Wangler et al. (Ref. 41), Fig. 52 from Hardy et al. (Ref. 42), Fig. 53 from Miller et al. (Ref. 43), Fig. 54 is the sum.

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Fig. 5. $(K\pi\pi)^{++}(1170)$. First reaction: $\pi^- p$. (From Rosenfeld's Oxford review, Ref. 4.)

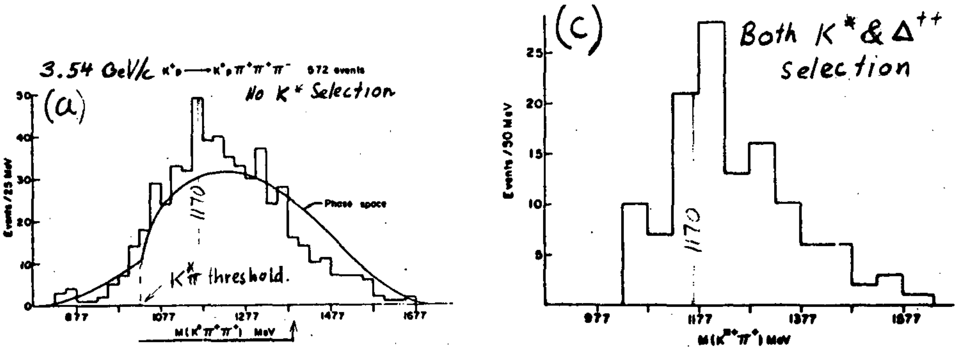
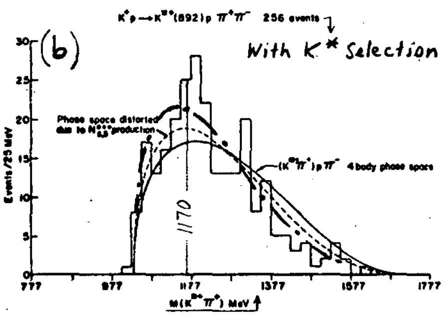


Fig. 6

Fig. 7



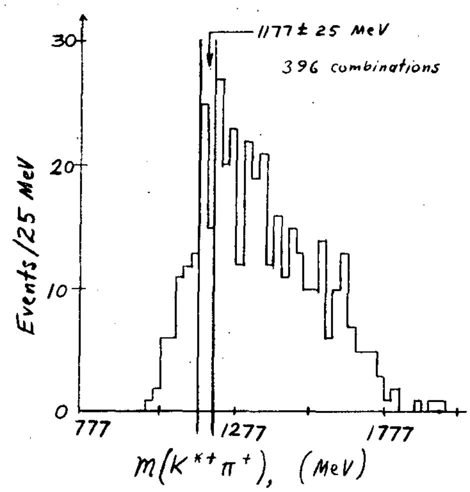


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Fig. 6. Possible evidence for a $K^{*+}\pi^+$ bump in 3.54-GeV/c $K^+p \to K^0 p \pi^+\pi^+\pi^-$, from Goshaw et al.[7] The solid curve in (b) is phase space for $K^*p\pi\pi$ production; the dashed curve is further modified to take into account Δ^{++} production. I have even further modified the dotted curve in two ways to give the dot-dash curve: (1) 4-body phase space (assuming K^* production) can be thought of as 5-body phase space (assuming $K\pi$ production) multiplied by a δ-function, $\delta(m^2(K\pi) - 893^2)$. But of course the $K\pi\pi$ Dalitz plot is really covered by two bands of finite width. The correction (area covered by finite bands)/(area covered by δ-function) peaks the spectrum towards low $K^*\pi$ mass. See Refs. 10 and 11. (2) There is a preference for small momentum transfer to the $K^*\pi$ system, which further peaks its spectrum towards low mass. See for example Appendix II of Ref. 4.

Fig. 7. 396 $K^{*+}\pi^+$ combinations produced by 4.6-GeV/c $K^+p \to K^0 p\pi^+\pi^+\pi^-$. These events are selected in exactly the same way as the 266 events of Goshaw et al., Fig. 6(b).

print. Their peak still does not seem striking to me. Accordingly I present Fig. 7, which shows a comparable number of events at slightly higher energy - 4.6 GeV/c K^+. This figure was prepared by Chumin Fu of the Goldhaber-Trilling group (LRL), using exactly the selections, bin sizes, etc. of Goshaw et al. It neither confirms nor denies the $(K\pi\pi)^{++}$ bump.

One other comment on the paper of Goshaw et al. They suggest that the resonance may be a manifestation of the "triangle singularity" suggested by Month et al. During the period 1964 through 1966, it was widely believed that at certain energies, in analogy to classical rescattering, there might be a quantum-mechanical singularity, and we might see a resonance. In 1967, Christoph Schmid showed that this could not be so,[8] and I think all the other theoretical exponents of the triangle singularity now agree that although rescattering can rearrange events within the Dalitz plot, it cannot enhance the whole plot.

The final claim on $(K\pi\pi)^{++}$ (1170) is by Barnham et al. (Birmingham, Glasgow, Oxford).[7] They have 10-GeV/c K^+ events, and find a bump at 1170 MeV, but not in the channel where Goshaw et al. have their bump. I must say this begins to remind me of the kappa, which always shows up in a new reaction and never reproduces itself.

Having just spent an evening sorting out all these conflicting claims, I would like to express mild irritation at the way they tend to be written up. The style is too frequently "The bump of interest was discovered by X et al. (Ref. 1) and confirmed by Y et al. (Ref. 2) although higher-energy reactions do not show the effect." (No reference to relevant experiments which fail to support the bump even though three rapporteurs have recently discussed and buried it.) Frequently too, the statement is missing that the data in Fig. X is not all new, but merely supercedes their earlier publication, based on half the events. I feel that, in general, authors could be much more helpful to the poor confused reader.

E. K^+K^+ AND $(KK\pi)^{++}$. This is not a channel in which one could dismiss a 3σ peak as a fluctuation, since so far the total number of mass combinations reported is only about 1000. However there are no noteworthy peaks at all.

Figure 8 is taken from Goldhaber's 1966 Berkeley rapporteur talk.[5] It shows that an original CERN KK bump[7]

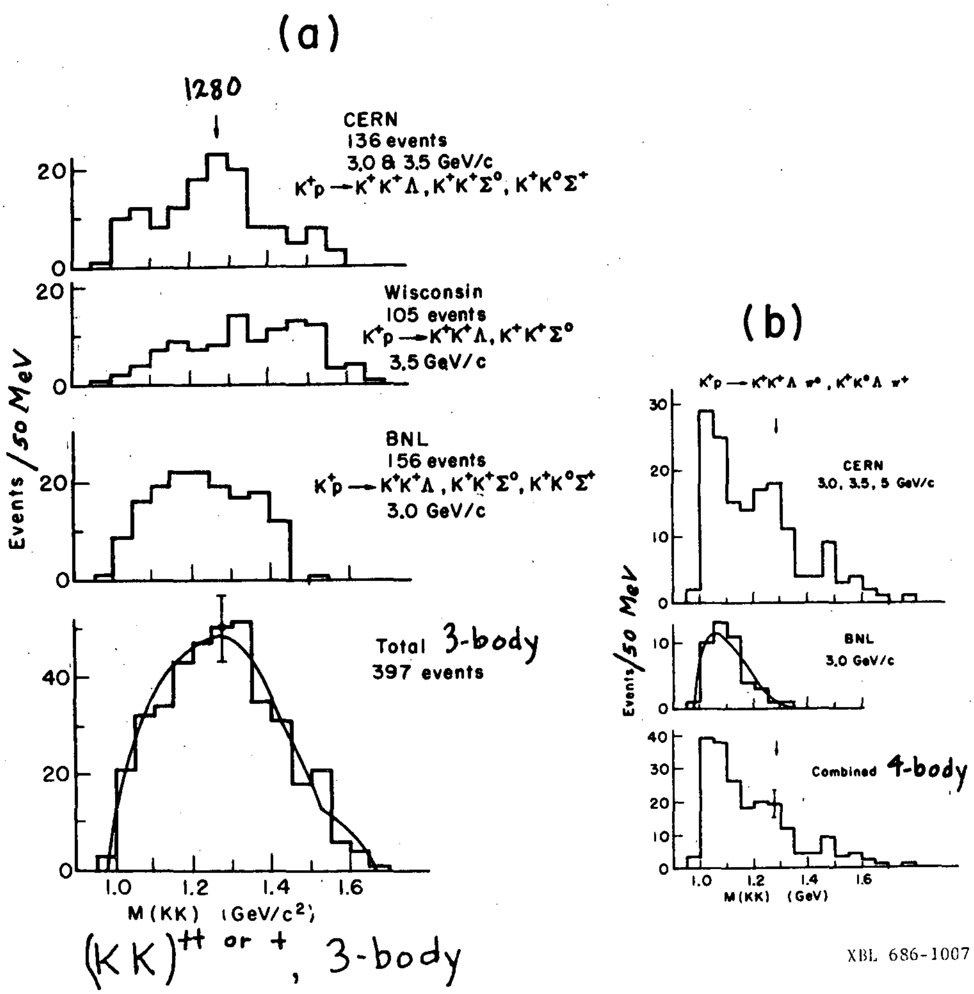

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Fig. 8. Summary of KK(1280) Y = + 2 bump from G. Goldhaber's 1966 Berkeley review talk, Ref. 5.

(made mainly with 3 GeV/c K^+) has now washed out. Since then Ferbel[9] has looked at 400 events of the form 13-GeV/c $K^+p \rightarrow \Lambda X^{++}$ and reports no bump larger than 8 μb. Also Alexander et al. have amassed 600 such events at 9 GeV/c. Their data are in Fig. 9; they cover the X^{++} mass range up to 3 GeV, and still show nothing interesting.

SUMMARY OF MESONS

Strongly interacting mesons certainly interact in states with far-out quantum numbers. For example, at the conference, Schlein pointed out that his $K^+\pi^-$ and $K^0\pi^0$ phase shifts are quite different, so that he needs an $I = 3/2$ amplitude. But there is no evidence so far that the forces are strong enough to produce resonances.

V. A SWITCH TO BARYONS. STATUS OF THE FAR-OUT $Z_0(1865)$ AND $Z_1(1900)$*

So far we have considered evidence for far-out mesons and found little; this is consistent with the fact that no such mesons are listed on our wallet sheets.[7] However, the baryon table of the wallet set does list a possible far-out* candidate, $Z_0(1865)$, accompanied by a warning "resonance ... not established." Even in a talk on mesons it does not seem right to ignore this possible evidence for complicated quark structures -- if I were really convinced that there exist far-out baryon resonances of five quarks ($qqq\,\bar{q}q$), I would not be so surprised at the discovery of far-out meson resonances of four quarks ($\bar{q}q\,\bar{q}q$).

The evidence for Y = +2 baryons was reviewed by J. Meyer at the 1967 Heidelberg Conference,[13] so here I shall merely repeat the warnings of some of my friends[18] as to the experimental difficulties with $Z_0(1865)$ and some difficulties in interpreting $Z_1(1900)$.

$Z_0(1865)$. Cool et al.[14] and Bugg et al.[15] have accurately measured the total cross sections $\sigma(K^+d)$ and $\sigma(K^+p)$, both of which have peaks at 1.2 GeV/c (1900 MeV). The K^+p peak is the candidate for the resonance called $Z_1(1900)$, which is discussed below. The difference between $\sigma(K^+d)$ and $\sigma(K^+p)$ peaks allows us (in principle) to extract a large $I = 0$ peak,

*The baryons with which we are now familiar fit into SU3 singlets, octets, and decuplets, and can be considered as being made of three quarks. "Far-out" baryons would then be members of any other supermultiplets.

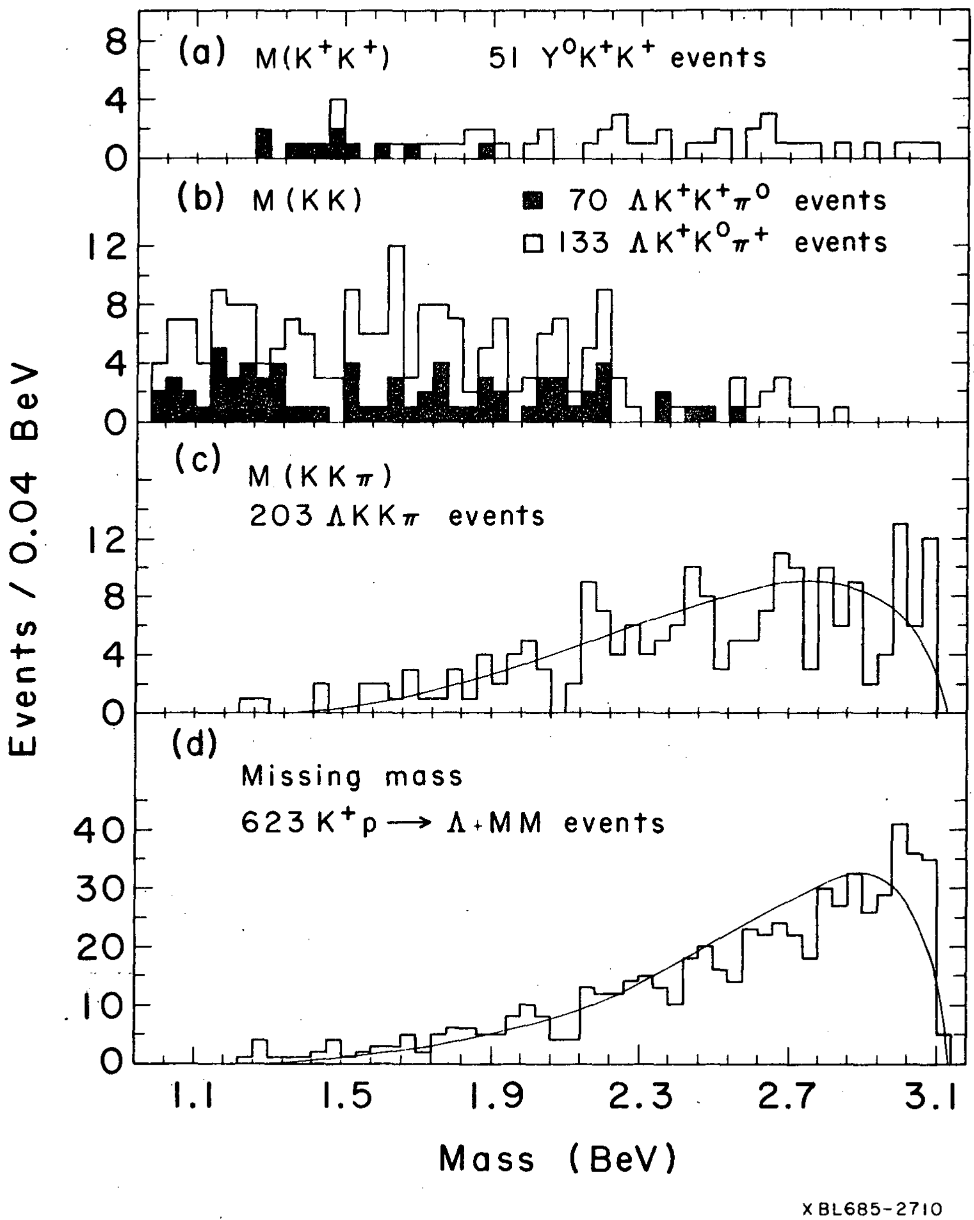

Fig. 9. Spectrum of Y = 2, Q = 2 events from 9-GeV/c K^+p. (From Alexander, Firestone, and Goldhaber, Ref. 9a.)

$Z_0(1865)$. But I now want to point out a recent experiment by Carter et al.[16] which tests this extraction procedure and shows that, as currently carried out, it leads to discrepancies of several millibarns at at least one momentum.

The experiment of Carter et al.[16] is to precisely measure $\sigma(\pi^+d)$, $\sigma(\pi^+p)$, and $\sigma(\pi^-p)$, the latter of course by charge symmetry being equal to $\sigma(\pi^+n)$. One then expects

$$\sigma(\pi^{\pm}d) = "\sigma(\pi^+p)" + "\sigma(\pi^-p)" - \sigma_{GW}(\text{shading}), \qquad (1)$$

where the quotation marks mean that each $\sigma(\pi N)$ has been averaged over its internal momentum in the deuteron, and σ_{GW} is the Glauber-Wilkin[17] correction for the mutual shadowing of nucleons in deuterium. Naturally we expect σ_{GW} to be positive. But look at Fig. 10 from Carter et al.[16] The solid lines A and B are $\sigma(\pi^-d)$ and $\sigma(\pi^+d)$; they should agree except for small Coulomb effects (and they do). Let us just consider their average. The bumpy dashed line is $\sigma(\pi^+p) + \sigma(\pi^-p)$, and the smoother dotted line labeled $"\sigma(\pi^+p)" + "\sigma(\pi^-p)"$ has been averaged over the Hulthén distribution for the internal momentum in the deuteron. It can easily be seen that, although the averaged curve has been somewhat smoothed, it has not been smoothed enough. Thus one would expect the difference between the dotted line and the average πd line to be nearly constant, reflecting a mutual shielding which changes only very slowly with energy. Instead this difference, which averages a reasonable value of 1 to 2 mb, rises to ~ 3 mb at 1.4 GeV/c and goes negative at 800 MeV/c. So something seems to be wrong with the current folding procedures tested in Eq. (1); it may be that the right prescription calls for averaging over an effective internal momentum distribution which is wider that that given by the simple impulse approximation.

We can now return to the 6-mb peak called $Z_0(1865)$, which is extracted from the difference between K^+d and K^+p cross sections; I want to make two contradictory comments. 1) Uncertainties in the folding are amplified in the subtraction:

a) After one inserts the right Clebsch-Gordan factors, the I = 0 cross section σ_0 turns out to be

$$"\sigma_0" = 2\sigma(K^+d) - 3"\sigma(K^+p)" - 2\sigma_{GW}, \qquad (2)$$

so we see that the uncertainties in folding are multiplied threefold, and the shading uncertainties are doubled.

b) Then one still has to unfold "σ_0", and there is no unique prescription for so doing, so that errors are further magnified.

2) Despite these warnings, it is going to be difficult to make the I = 0 peak go away, unless the data change slightly. I should point out that the peak depends mainly on the Brookhaven data of Cool et al.,[14] and are not really very quantitatively confirmed by the Rutherford data of Bugg et al.[15] [Just to get some idea of the maximum uncertainty possible, I have tried making the nonsense unfolded subtraction $\sigma_0 = 2\sigma(K^+d) - 3\sigma(K^+p)$. In the case of the BNL data one is left with a broad 1-mb peak, but for the RHEL data little is left.]

In conclusion on $Z_0(1865)$, it is clear that more experimental work and more thought about Eq. (2) is needed before we can wholeheartedly accept this bump.

$Z_1(1900)$. Here there is no question of extraction; one can see a bump directly in the K^+p s-channel, as shown in Fig. 10, which is taken from Bland et al.[19] Figure 11 not only shows the peak originally discovered[14] in $\sigma(K^+p)$, but also shows that it can be associated with a sudden rise in the inelastic cross section, particularly of $K^0\Delta^{++}$. To decide whether we have a K^+p resonance, we need partial-wave analysis, either of the elastic channel, or of the $K\Delta$ channel. Meyer[13] summarizes the status of the elastic partial-wave analyses, and concludes that the data are still inadequate to uncover a resonance. (Fortunately a K^+p elastic scattering experiment with a polarized target is scheduled to run soon at Argonne National Laboratory; this should remove some bothersome ambiquities).

But a partial-wave analysis of the $K\Delta$ channel has recently been completed by Roger Bland[20] of the Goldhaber-Trilling bubble chamber group at Lawrence Radiation Laboratory. Figure 12 is from Bland's thesis, and gives his Argand diagrams. Be warned that the errors on this plot have to be interpreted cautiously since for an inelastic channel there is no readily available reference phase. Hence, in the analysis leading to Fig. 12 it was necessary to assume a constant phase for the M1' amplitude (higher partial waves predicted by the Stodolsky-Sakurai ρ-exchange model). We see that only P_3 (the notation is L_{2J+1}) looks much like a resonant circle, and even here there is trouble with the velocity along the circle. In momentum the 1900-MeV peak centers not far from 1160 MeV/c, and the data points plotted

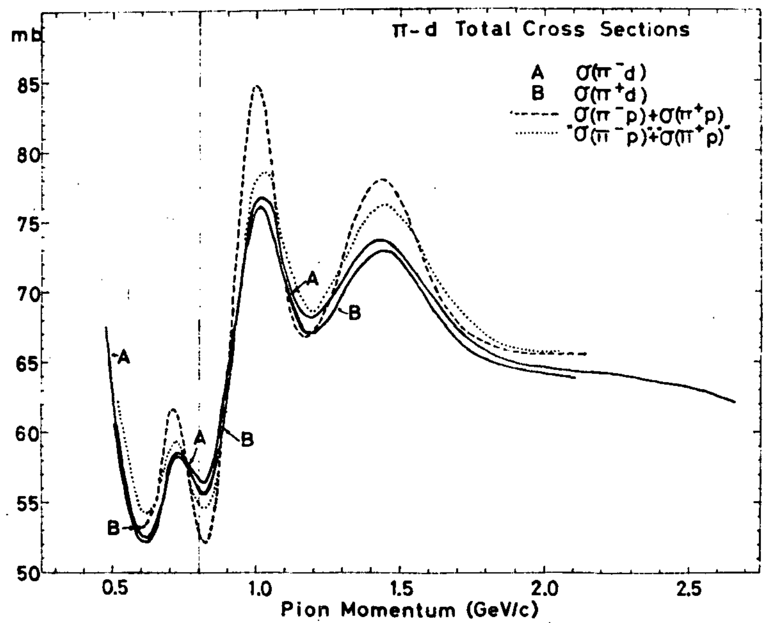

Fig. 10. Comparison of total cross sections for pions on deuterium and on nucleons, from Carter et al. Ref. 16.

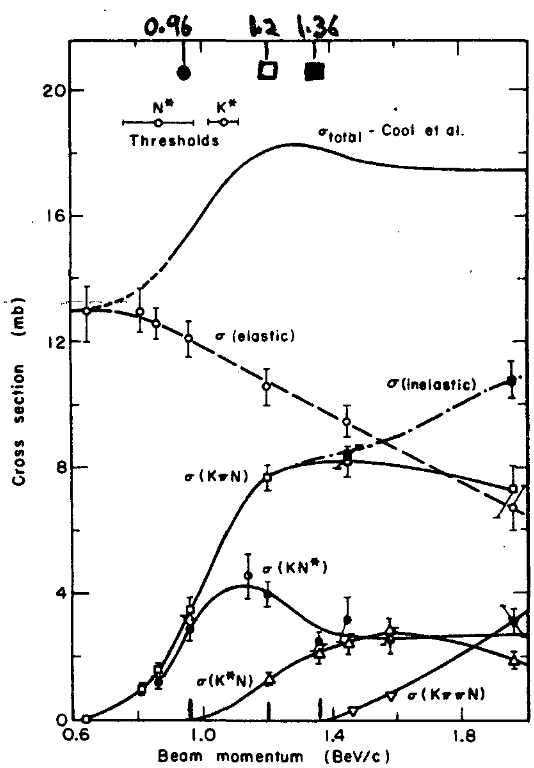

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Fig. 11. Total and partial K^+p cross sections, from Ref. 19.

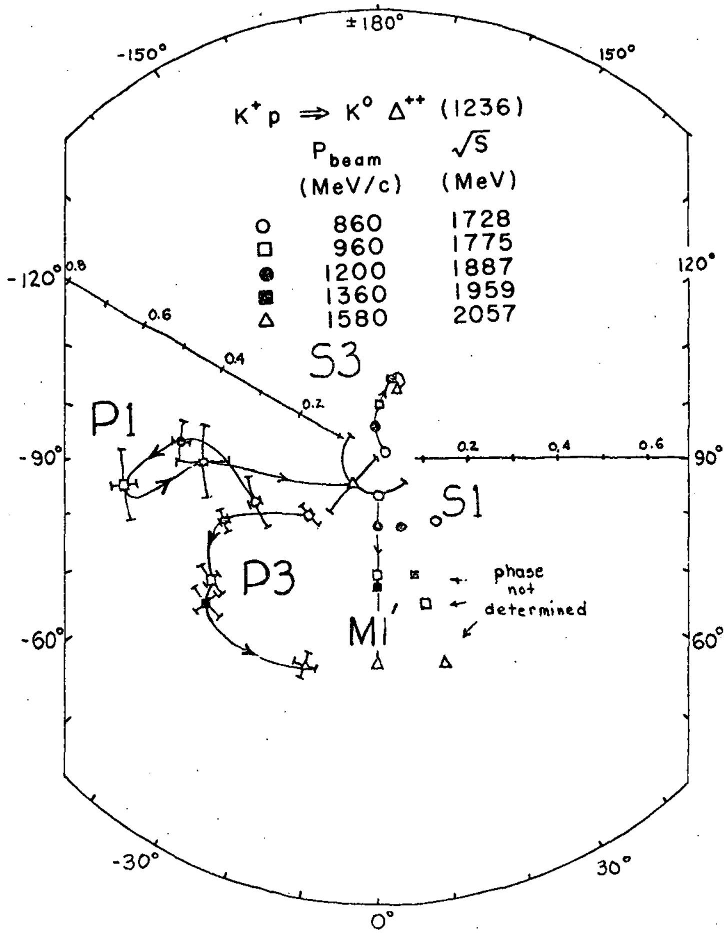

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Fig. 12. Argand plot of the partial-wave amplitudes for $K^+p \rightarrow K^0\Delta^{++}(1236)$. Phases are measured relative to the M1' amplitude (see text) which is assumed to be constant in phase. The points at 1580 MeV/c are from Vic Seeger (Goldhaber-Trilling Group, Lawrence Radiation Laboratory). From Ref. 20.

at 960 MeV/c and 1360 MeV/c fall about $\Gamma/2$ on either side. At the top of Fig. 11, I have drawn circles and squares at the relevant momenta . Thus one would expect a resonant amplitude to be changing faster with energy at 1160 MeV/c than on either side. Instead it is almost stationary. I conclude that we cannot yet decide if there is a $Z_1(1900, 3/2^+)$ resonance. Bland's P_3 solution indeed looks more like a resonance than most of those labeled "probable" by Lovelace[21] on his Argand plots for πp elastic scattering.

But there should be a difference in acceptance criteria for ordinary vs far-out resonances, simply because we expect the former, and not necessarily the latter.

SUMMARY OF BARYONS

To summarize my discussion of Z_0 and Z_1 resonances, I will say that we have never put $Z_1(1900)$ on our list of baryons, and we are now not sure if we should have included $Z_0(1865)$. Thus for far-out resonances, the situation is the same for baryons as it was for mesons; there are certainly appreciable forces between particles like K^+ and p (which taken together have far-out quantum numbers), but it has not yet been established that these forces are strong enough to produce resonances.

VI. "EXPLAINING" PEAKS vs "EXPLAINING THEM AWAY"

Before I leave the question of how to interpret the peak in $\sigma(K^+p)$ at $K\Delta$ threshold, I want to express one general piece of understanding that has slowly dawned on me.

In the last section, I said:

"There is a peak, but no partial waves resonate, so we explain away the $Z_1(1900)$ bump as a threshold effect."

But if P_3 had followed a slightly more suggestive counter-clockwise circle, I would have said:

"There is a peak, which motivates us to study the partial waves in this region, and sure enough the threshold channel resonates, so we classify the peak as $Z_1(1900, 3/2^+)$."

Note that the presence or absence of the nearby threshold is irrelevant to the final interpretation, which depends only on the shape of the Argand plots.

I emphasize this point because experimentalists before claiming a resonance always try to explain it away.. And so they should if their bump is just the kinematic reflection of some other resonances. But I just showed that nearby threshold effects cannot necessarily explain away a bump. The same applies to t-channel and other complicated effects (Deck effect, etc.). After all, we all believe that resonances (in the s-channel) are the result of forces (in the t-channel), and theorists may even be starting to be able to calculate s-channel output from t-channel input. Right now, there is no way to foretell whether an effect in some other channel will explain a resonance, or explain it away.

I know of only two ways to identify a resonance. The first is to find a bump so clear and narrow that we recognize it without having really to understand it. The second is to see if it seems to correspond to a pole in the partial-wave amplitude. If there is a pole, this amplitude (for two-bodies in, two out) when plotted on an Argand diagram, will follow a characteristic counter-clockwise "circle." Even with knowledge of the partial waves, it may still turn out to be very hard to decide in many cases -- if recognizable resonant circles show up simultaneously in several channels, the decision is simple, but we will seldom be so lucky.

ACKNOWLEDGMENTS

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FOOTNOTES AND REFERENCES

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