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METHODS OF DETERMING THE QUANTUM NUMBERS OF EXCITED MESON AND BARYON STATES*

D. B. Lichtenberg**

Stanford Linear Accelerator Center
Stanford, California

At the recent Washington meeting of the American Physical Society, in a session dedicated to Niels Bohr, Leon Rosenfeld gave an enlightening talk on the principle of complementarity. The talk I shall give today is complementary to most of the talks you have heard so far. In particular, its is complementary to the talk given yesterday by Glashow. Whereas Glashow believes in unitary symmetry and used it to predict the quantum numbers of the resonances, I am going to take the skeptical attitude that the quantum numbers of particles are numbers which must be determined from experiment.

I actually believe there is something to unitary symmetry, but what I believe is not necessarily the way nature is. Hence, the necessity for making experimental tests. And unitary symmetry is not the only symmetry I shall question in this talk.

A large number of methods have been proposed to obtain information on the quantum numbers of particles. Some methods depend on dynamical assumtions, while others make use only of conservation laws. In general, it is useful to determine quantum numbers by many different methods when experimentally feasible. In this way, not only does one obtain confidence in the correctness of the result, but a number of different assumptions, either about dynamics or about conservation laws, can be tested.

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** Address after September, 1963: Indiana University, Bloomington, Indiana.

According to current ideas, the quantum numbers necessary to specify a meson or baryon, in addition to a possible quantum number associated with its mass and decay width, are its spin J, parity P, baryon number B, hypercharge Y (or alternatively strangeness which equals Y - B), isospin I, and the z-component of its isospin I_z (or alternatively charge Q). If the particle is a meson with Y = 0, then it also has a definite G parity. If the meson is also neutral, it is an eigenstate of the charge conjugation operator and can be said to have a definite C parity. The spin and parity I call "external" quantum numbers, since they have to do with symmetries in physical space, and the other quantum numbers I call "internal." It is conceivable that some strongly interacting particles will turn out to have still other quantum numbers, or in other cases that the quantum numbers I have mentioned might not all be relevant. However, I shall not consider these possibilities here.

Many people believe that it is trivial to measure the baryon number, the charge and the hypercharge of a particle, since these are presumed to be additive quantum numbers which are conserved (at least) in the strong and electromagnetic interactions which cause the particle to be produced. These additive quantum numbers are usually contrasted to the multiplicative quantum numbers parity, C parity, G parity. But parity can equally be regarded as an additive quantum number modulo 2. Then there is the possibility of a whole sequence of additive quantum numbers, modula 2, 3, 4... until one reaches a true additive number, mudolo infinity. This is not a new idea, but I suspect it has been forgotten.

There is indeed abundant evidence that B and Q are true additive quantum numbers. But there is no such evidence for hypercharge Y. For example, hypercharge might be additive modulo 4. Those who associate Y with a gauge transformation may protest, but the question is one for experiment to decide. Unfortunately, I don't know of a test which is feasible at present. An impractical test which will serve as an example would be to compare the cross section for the reaction

$$K^+ + p \rightarrow 3K^- + 4\pi^+ + p$$

with the cross section for the allowed reaction

$$K^+ + p \rightarrow 2K^+ + K^- + 2\pi^+ + 2\pi^- + p.$$

But even the "allowed" reaction will probably be too rare to be observed in the near future.

Next I'd like to discuss the question of statistics. Greenberg and Messiah[1] have written a thick paper in which they have pointed out that although it has been verified that nucleons and electrons obey Fermi statistics and that photons obey Bose statistics, there is not any really convincing evidence that pions obey Bose statistics. (This was not the first paper on the subject, but merely the most exhaustive.) I was quite surprised at this, as I had always assumed it was well known that pions were bosons. Also, there is at present no evidence that kaons are bosons, but evidence ought to be forthcoming soon in connection with the φ meson. I shall come back to this question. And there is certainly no direct evidence that hyperons are fermions.

There is a whole class of statistics called parastatistics, which are intermediate between Bose statistics and Fermi statistics. There exists the possibility of particles being parabosons or parafermions. I don't want to go into the subject of parastatistics, but want to point out that it is important to distinguish between methods of determining spins and parities which rely on assumptions about statistics and those which do not.

If two identical particles obey the usual statistics, then there's a relationship between the internal and external quantum numbers of the two particles:

$$(-1)^{L + S + I + Y/2} = 1$$

where L is the orbital angular momentum of the system, S the total spin, I the total isospin, and Y the total hypercharge. This formula can be derived either for two bosons or two fermions which are identical except possibly for charge. The derivation depends on additional assumptions which I won't go into here. But since additional assumptions are necessary, I won't be shocked if it is found experimentally that certain particles obey the usual statistics but don't satisfy the formula. Similarly, if a particle and antiparticle obey the usual statistics, it can be derived that

$$C = (-1)^{L + S}$$

Again the derivation depends on additional assumptions.

Now it's generally assumed that the parity of a boson-antiboson pair in a state of definite orbital angular momentum is given by

$$P = (-1)^{L}.$$

and for a fermion-antifermion pair, it is usually assumed that the parity is

$$P = (-1)^{L+1}$$

Now it's well verified experimentally that $P = (-1)^{L+1}$ for an electron-positron pair. It has also been verified that for a pion-antipion pair, $P = (-1)^L$. But I don't think it has been verified for a K and a $\bar{K}$ that $P = (-1)^L$ or that a $\Lambda\bar{\Lambda}$ or $\Sigma\bar{\Sigma}$ satisfy $P = (-1)^{L+1}$.

Let me assume the $K\bar{K}$ system satisfies the usual relations $P = (-1)^L$ and $C = (-1)^L$. Then $CP(K\bar{K}) = +K\bar{K}$. Now I want to re-do the derivation given by Leitner a few minutes ago that the decay $\varphi \rightarrow K_1K_2$ implies that the spin J of the φ is odd. Only I want to assume CP invariance rather than C or P invariance separately. If CP is conserved, then $CP(K_1) = K_1$ and $CP(K_2) = -K_2$. Thus

$$CP(K_1K_2) = -(-1)^J K_1K_2$$

where the $(-1)^J$ comes from P acting on the orbital angular momentum of the K_1K_2 system. But if the φ decays $\varphi \rightarrow K^\circ\bar{K}^\circ$ with the $K^\circ\bar{K}^\circ$ subsequently decaying into K_1K_2, then CP = +. Thus $-(-1)^J = +$, i.e. J is odd. A direct measurment of the spin of the φ by its angular distribution will prove a test of this conclusion. But note that the derivation depended on more than one assumption. Therefore, if the spin of the φ should turn out to be one, as everybody believes, this will not by itself prove that kaons obey Bose statistics.

I should now like to discuss an effect found by Huson and Fretter[2] in looking at 17 Bev/c π^- interactions in a freon propane bubble chamber. The reactions observed were

$$\pi^- + A \longrightarrow A + \begin{cases} \pi^- + \pi^\circ + \pi^\circ \\ \pi^- + \pi^- + \pi^+ \end{cases}$$

They were able to see the π°'s by conversion of the gamma rays with good efficiency. The branching ratio R was observed to be

$$R = \frac{\pi^-\pi^-\pi^+}{\pi^-\pi^\circ\pi^\circ} = 2.5 \pm 1.$$

There is a preliminary indication that there is a resonance in the 3-pion system at a mass of about 1200 MeV, but Fretter and Huson are not willing to say it's a resonance. The resolution is terrible ($\pm$ 250 MeV). The typical momentum transfers to the nucleus are sufficiently small so that the production process can be considered coherent. Also prominently seen was the decay mode $\rho + \pi$. If I is conserved, then the three pion system has $I = 1$ or 2. The fraction f of decays into $\rho + \pi$ was found to be $f \geq \frac{2}{3}$. If the decay were only into $\rho + \pi$, then R would be equal to one.

Let me write the wave function of the 3-pion system as (I , I), where I' is the isospin of the first two pions and I is the isospin of the entire system. For $I = 1$, there are the possibilities (0, 1), (1, 1), and (2, 1). For $I = 2$ the possibilities are (1, 2) and (2, 2).

First consider $I = 2$. If $I' = 1$ and if the first two pions obey Bose statistics, then the orbital angular momentum of the two pions is odd. If $I' = 2$, the orbital angular momentum of the first two pions is even. So if the pions obey Bose statistics, there can't be any interference between the amplitudes (1, 2) and (2, 2) in the total rate. Since each amplitude separately gives a branching ratio $R = 1$, this argument leads to the prediction that the total branching ratio is $R = 1$. However, interference can actually occur. Since the pions are identical particles, one doesn't know which two pions are the first two. If instead of pions 1 and 2, pions 1 and 3 combine in an odd orbital angular momentum state, then the wave function of pions 1 and 2 contains both even and odd angular momenta. Thus, interference can occur unless the pions can be distinguished by their energies.

One can also treat the $I = 1$ case. Here, interference occurs between the states (0, 1) and (2, 1), since the first two pions have even angular momentum in both cases. Thus, the branching ratio is not unique. But if the pions can be distinguished by their energies, there is no interference between the state (1, 1) and the other two states. This allows one to put limits on the ratio R. These limits are

$$\frac{f}{2-f} \leq R \leq \frac{2-f}{f}.$$

Now if $f = \frac{2}{3}$, then $R \lesssim 2$, which doesn't quite agree with the experimental value $R = 2.5$. But there are only 50 events, and there is agreement within statistics. Also, the energy resolution is poor, so one cannot distinguish too well which are the first two pions. The width of the ρ may provide a limitation even when the resolution is improved. So everything is compatible with $I = 1$, but not quite so compatible with $I = 2$.

I'd like to spend most of my remaining time on the Adair analysis and extensions of it. The Adair analysis has fallen out of favor as a method of measuring spins of particles because one must look at particles produced near the forward direction. Physicists don't like to throw away most of their events. But in a coherent production process, like the one seen by Huson and Fretter, all particles are produced near the forward direction. Therefore, all coherently produced events are suitable for inclusion in the Adair analysis. This can be made plausible by the following simple qualitative argument. Let bosons of spin J be produced coherently from a spin O nucleus with a spin wave function $_J$ given by

$$\psi_J = \sum_m \alpha_m X_J^m$$

where the X_J^m are the spin eigenfunctions of the produced boson with respect to the beam direction and the amplitudes α_m are functions of the production angle. Now the α_m must be of the form

$$\alpha_m = \sum_\ell \alpha_{\ell m} Y_\ell^{-m}(\theta, \phi)$$

where θ, ϕ are the production angles. The particular m-value results from the fact that the total angular momentum can have no component in the beam direction. Now it is a property of spherical harmonics near $\theta = 0$ that

$$|Y_\ell^1| / |Y_\ell^0| = \frac{\sqrt{\ell(\ell+1)}}{2}\,\theta \approx \ell\,\theta .$$

Thus if L is the maximum orbital angular momentum entering the production process then near the forward direction the coefficient α_1 of X_J^1 will in general be less than $L\theta$ times as large as the coefficient α_o of X_J^o. Thus, to a first approximation, if events are selected in which mesons are produced at angles less than θ, where

θ L = 1, the mesons will have a spin wave function which is predominantly X_J^o. Now if the radius of the target nucleus is r, the maximum orbital angular momentum entering the production process is of order kr = L, where k is the barycentric momentum of the produced meson. But for the process to be coherent, the maximum momentum transfer q to the nucleus is of order qr = 1. Using kr = L, qr = 1 and Lθ = 1, one obtains $\theta = q/k$ as the maximum angle which is useful for the Adair analysis. But this same angle is easily seen to be the cutoff angle in a coherent process. Therefore, all events may be included in the analysis.

If the produced boson is known to be in a definite state of polarization such as X_J^o and decays into two spinless particles the decay angular distribution is a unique function of its spin. In decays into particles with spin, the angular distribution is not necessarily a unique function of the spin, but some information can be obtained.

If the incident particle is a pion or kaon, and the spin and parity of the produced boson are related by $P = (-1)^J$, then conservation of parity implies that the production amplitude vanishes in the forward direction. Nevertheless, the Adair analysis can be extended to apply to this case also. Since

$$|Y_\ell^2| / |Y_\ell^1| = \frac{1}{4}\sqrt{(\ell+2)\cdot(\ell-1)}\ \theta \approx \ell\theta$$

near the forward direction, the same arguments go through as before, except the spin wave function of the boson will be given by $\psi_J = X_J^1 + X_J^{-1}$. Again the angular distribution for the decay into two spinless particles is unique, but is different from the opposite parity case. Because the angular dependence of the production amplitude depends on the parity of the meson, the method is useful to obtain information about the parity as well as the spin.

When the parity is $P = (-1)^J$, the production cross section is likely to be small. This is because the amplitude vanishes at $\theta = 0$ and in addition rapidly goes to zero beyond the coherent cutoff angle. Berman and Drell[4] pointed out to me the usefulness of coherent production processes for determining of spins and parity of mesons. In addition they have done a detailed analysis of coherent production processes. By making use of the invariance properties of the production amplitudes, they were able to obtain essentially the same results I have outlined, but in a way which makes use of the polarization tensors of the produced mesons. Their method deals with relativistically covariant quantities throughout and is therefore more rigorous than the one I have discussed. In addition, using their method, it becomes clear that one should measure the decay angular distribution with respect to the incident direction as seen in the rest frame of the produced meson. This is not strictly the Adair angle, but the difference is small in coherent processes. Also, Berman and Drell have considered the possibility of accidental cancellation in the forward production amplitude, but I shall not discuss this point.

I also wanted to discuss a very beautiful method by Byers and Fenster[5] for determining spins and parities of baryons, but I see I do not have time. I hope Ticho will discuss that in some detail in the next talk.

REFERENCES

1: O. W. Greenberg and A. Messiah (to be published).
2: F. R. Huson and W. B. Fretter, Bull. Am. Phys. Soc. 8, 324, (1963).
3: R. K. Adair, Phys. Rev. 100, 1540, (1955).
4: S. M. Berman and S. D. Drell (to be published).
5: N. Byers and S. Fenster, (to be published).

DISCUSSION:

G. GOLDHABER: I would like to make a comment in favor of Bose statistics for pions. If you can remember in the distant past to 1959, we did an experiment studying angular correlations in angles between π mesons and found a distinct difference between like pions and unlike pions. Namely, like pions like to come off at small angles with each other; this was analysed in a paper by Goldhaber, Goldhaber, Lee and Pais,* where we strongly suggested that this is due to Bose statistics for pions.

LICHTENBERG: Yes, but your result might also be due to final state interactions.

*G. Goldhaber, S. Goldhaber, W. Lee and A. Pais, Phys. Rev. 120, 300, (1960).

I certainly agree that there is some evidence in favor of Bose statistics for pions, but want to point out that there are possible alternative explanations. Thus, there is not really convincing evidence, and I want to take an extremely skeptical point of view.

GOLDHABER: Then we would have to reconcile ourselves with having found the first pion-pion resonance.

LEITNER: I want to comment with regard to the φ. We did look at the angular distribution of its decay and we do find that it's not isotropic, so that the angular distribution cannot be reconciled with the spin of the being equal to 0.

LICHTENBERG: The spin of the φ could be 2. Since it's so narrow, one wouldn't expect it to be 0.

HOLLADAY: I would like to point out that in the tau decay there were 2 π's and a π^-. One cannot understand the energy distributions of those π's if you put those 2 π^+'s in odd angular momentum states. So to that extent one can rule out Fermi Statistics for the 2 π^+'s.

LICHTENBERG: Unfortunately I am neither Greenberg nor Messiah. I saw this paper only at the Washington meeting, and they discussed the evidence pretty thoroughly. I do not want to be in the position of defending every example that is brought up at the present time. In this example, the centrifugal barrier would inhibit odd angular momentum.

HOLLADAY: They also made this point. Actually I believe that their point was that one didn't know whether the 2 pions obeyed Bose statistics or some sort of para-statistics. One can rule out Fermi Statistics in any case. The fact that the K_1 has spin 0 and decays some of the time into two neutral pions, is already evidence that it cannot be a Fermi system.

LICHTENBERG: Yes, all the people who discuss whether pions obey Bose statistics or not, talk in terms of para-statistics where the particles obey trilinear or more complicated kinds of commutation rules. Pions certainly do not obey Fermi statistics.

SUDARSHAN: I want to make two more short comments. First that as far as I know, the first statement about statistics in relation to an experiment was made in a paper by Dell'Antonio, Greenberg and myself.* This is a rather thin paper rather than a thick paper. The other point is in regard to the question of the parity of the antiparticles. It was originally raised by Foldy several years ago, in a very beautiful paper, around 1957. But somehow, nobody seems to have paid any attention to it.

LICHTENBERG: I apologize to Foldy.**

SCOTTI: Let me say nobody mentioned the name Giovanni Gentile***, who is connected with parastatistics about 20 years ago.

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* G. Dell'Antonio, O. Greenberg, and E. Sudarshan, University of Rochester report NYO-102Y1 (1962).

** L. L. Foldy, Phys. Rev. 102, 568, (1956). Foldy also discussed the question of the connection between spin and statistics in this paper, pointing out that the usual relations might not apply to all particles. However, Foldy did not discuss the experimental evidence in any detail, either in regard to the parity of a particle-antiparticle pair or the connection between spin and statistics.

*** G. Gentile, Nuovo Cimento 17, 493 (1940).