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INTERNATIONAL ATOMIC ENERGY AGENCY

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THE ROLE OF SYMMETRY PHYSICS - SOME CONCLUSIONS FROM THE OXFORD CONFERENCE SEPTEMBER 1965

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1. INTRODUCTION

Four years ago, in September 1961, some of us here today were privileged to hear R.P. Feynman give the concluding address at the Aix-en-Provence Conference on Elementary Particles. For those who heard it, this was a memorable experience - an address of classic brilliance and eloquence. Right at the outset, Feynman made it clear that he did not conceive his talk as a summary of the Conference so much as a discussion of its flavour and of its conclusions in a wider sense. He wanted to ask himself what is most characteristic of the meeting, what new positions we are in at the present time, what kind of things may we expect in the future. In approaching my task today, in all humility, I would like to take Feynman's address as setting a pattern and to ask the same questions he did once again. I shall make no attempt to summarize the Conference, for this has already been done so brilliantly by speakers in the plenary sessions.

For the 1961 Conference, Feynman concluded that the thing most characteristic of the meeting was the bringing into focus of the reality of a few resonances η, ρ, ω, K^* and the beginning of a philosophy of resonances. The theme for 1965 is undoubtedly once again the same. We have lived through a year of rare achievement in a phenomenological correlation of the resonances. This has been a vintage year in symmetry theory with the emphasis shifting decisively from the mysterious "intrinsic" to the more recondite dynamical symmetries. We have learnt, perhaps in a heuristic manner, how to extract results from symmetry ideas and in the process we believe we understand better the power as well as the limitations of the symmetry method. But with all this, though the subject stands transformed, today we are still far from a complete picture of the dynamical mechanism responsible for the symmetries and particularly their persistence. One sometimes has the feeling that we may perhaps be near the close of one chapter in strong interaction physics, with possibly a new one to begin. All this has happened with frightful suddenness and I would thus like to spend my time in speaking of the symmetry situation as I understand it.

Last year, at the time of the Dubna Conference, strong interaction physics had reached a decisive stage with the discovery of an approximate SU(3) symmetry as a direct generalization of the isotopic-spin symmetry SU(2). Like SU(2), one had assumed that SU(3) represented something "intrinsic" - a symmetry in the un-understood tradition of the SU(1) symmetries responsible for charge and baryon number conservation. An internal, an intrinsic, symmetry, in our present thinking, has nothing whatever to do with the structure of space-time as we know it. All scattering matrices must respect it.

About the same time as the Dubna Conference - an ocean away at Brookhaven and Argonne - Gürsey, Radicati and Sakita following Wigner's ideas of nearly thirty years ago, discovered a new dynamical - as opposed to an internal - symmetry. This was the famous rest-symmetry SU(6) with its magic multiplets of 35 and 56.

On the one hand it started off a speculated chain of generalizations; first the compact rest-symmetry, U(6) x U(6), for accommodating still more resonances in one single multiplet; then the non-compact symmetries like U(6) x U(6) x O(3,1), U(6,6) and possibly even U(6,6) x O(3,1), to accommodate in one single "tower" an infinity of multiplets themselves. A second and still more feverish search which began with non-relativistic SU(6) was for its relativistic completion- this to discover symmetries of the S-matrix. The search led directly to the (broken) symmetries of SL(6,C) and $\tilde{U}(12)$ and another set of remarkable correlations - mainly for form factor physics. But with all this undoubted achievement, there is also the realisation that symmetries are no complete substitute for dynamics, nor - and this is important - should one expect them to be. It is the limitations of the symmetry method - rather more than their known and outstanding successes - that I shall make the theme of my remarks here.

2. DYNAMICAL GROUPS IN GENERAL

Dynamical groups, relativistic or non-relativistic, are no strangers to physics. In fact it is of the essence of "model-making" in atomic or nuclear physics that a model is (as a rule) soluble if it admits of a simple (dynamical) group.

The most instructive case is that of the first system ever treated in quantum physics - the source and fount of all our wisdom in dynamical group theory - the hydrogen atom. The group-treatment is so beautiful and yet so simple that I would like to go over it in some detail.

Consider the Schrödinger equation for a static Coulomb potential, with the energy:

$$E = \frac{1}{2} p^2 - r^{-1}$$

Pauli, Bargmann, Fock, Hülthen and others made the following discovery in the 1930's; introduce the angular momentum operator

$$\underset{\sim}{L} = \underset{\sim}{r} \times \underset{\sim}{p}$$

and a rather complicated operator - the so-called Lenz vector -

$$\underset{\sim}{M} = \frac{1}{\sqrt{-8E}} \left(\underset{\sim}{L} \times \underset{\sim}{p} - \underset{\sim}{p} \times \underset{\sim}{L} \right) + \frac{\underset{\sim}{r}}{r} \quad .$$

Now the set of the six operators $\underline{L}$ and $\underline{M}$ possess the following remarkable properties.

(1) The two combinations

$$\underset{\sim}{I} = \tfrac{1}{2}(\underset{\sim}{L} + \underset{\sim}{M})$$

$$\underset{\sim}{K} = \tfrac{1}{2}(\underset{\sim}{L} - \underset{\sim}{M})$$

commute with each other. Both $\underset{\sim}{I}$ and $\underset{\sim}{K}$ obey the same commutation relations as the conventional angular momentum vector $\underset{\sim}{L}$;

$$[L_i, L_j] = i\epsilon_{ijk} L_k$$

Thus, like $\underset{\sim}{L}$, $\underset{\sim}{I}$ and $\underset{\sim}{K}$ may be considered as generators of rotations in two distinct three-dimensional spaces. One may therefore quantise $\underset{\sim}{I}$ and $\underset{\sim}{K}$ independently with eigen-values i and k = $(0, \tfrac{1}{2}, 1, \ldots)$. In more abstract language $\underset{\sim}{I}$ and $\underset{\sim}{K}$ are group generators of the Algebra $O(3) \times O(3)$. This is also the Algebra of a group of rotations in four dimensions $O(4) = O(3) \times O(3)$.

(2) Most remarkable of all, the Hamiltonian can be written as

$$-4E = \left[\underset{\sim}{I}^2 + \underset{\sim}{K}^2 + \tfrac{1}{2}\right]^{-1}$$

Thus in general the energy of level (i,k) equals :-

$$-4E(i,k) = \left[i(i+1) + k(k+1) + \tfrac{1}{2}\right]^{-1}$$

Each level - each multiplet - has the degeneracy (2i+1)(2k+1).

(3) There is one further restriction on possible values of i and k. Note trivially that $\underset{\sim}{L}.\underset{\sim}{M} = 0$. Thus

$\underset{\sim}{I}^2 = \underset{\sim}{K}^2$ and therefore i = k.

Setting

$$n = 2i + 1 = 2k + 1$$

we finally get for the level (multiplet) mass:

$$E = -\frac{1}{2n^2}.$$

(4) For the energy level (i,k), $\underset{\sim}{L} = \underset{\sim}{I} + \underset{\sim}{K}$ varies between $|i - k| \leqslant l \leqslant i + k$. With $i = k$, each level (multiplet) n contains spin-values $0 \leqslant l \leqslant (n - 1)$. Hence then we have a spin-containing symmetry <u>par excellence</u>. The symmetry of the Hamiltonian is $O_I(3) \times O_K(3) = O(4)$. This symmetry is much larger than the angular momentum symmetry $O_L(3)$ which is contained in $O(4)$. All levels are labelled by two quantum numbers (i,k) with the subsidiary condition $i = k = \frac{n-1}{2}$. Each level – each multiplet in modern usage – possesses a degeneracy $(2i + 1)(2k + 1) = n^2$ and encompasses spin states with spins ranging from 0,1,2, ..., (n – 1), with a "mean mass" $-\frac{1}{2n^2}$.

3. MORALS FROM THE HYDROGEN PROBLEM

The hydrogen atom has a number of lessons to teach us.

(1) The dynamical spin-containing symmetry $O(4) = O(3) \times O(3)$ arises peculiarly for the case of the $\frac{1}{r}$ potential. The fact that the symmetry group is $O(4)$ (and not merely the angular momentum group $O(3)$) is a consequence of the $\frac{1}{r}$ law. Any deviation from this idealization (a spin-orbit coupling term, for example) will destroy the spin-containing symmetry $O(4)$. In this sense the existence of the $O(4)$ symmetry is a "dynamical accident", dictated by the dominance of the Coulomb potential.

(2) If one scatters an atom of hydrogen in a level n_1 with another excited in level n_2, it is far from clear that the potential for the scattering problem will be the same $\frac{1}{r}$ potential. Thus to expect that a general S-matrix element may possess the $O(4)$ symmetry would be a completely new assumption - utterly unrelated to the spectrum-producing symmetry.

(3) It has been noted by Barut, Budini, Fronsdal, Gell-Mann, Dothan, Ne'eman, Bacry and others that one can formally adjoin to the 6 generators of $O(4)$ another set of four, making up a total of 10 generators for rotations in a non-compact (open) de Sitter space - a space like the Lorentz space, but with one time and four space-directions ($X_0^2 - X_1^2 - X_2^2 - X_3^2 - X_4^2 =$ constant). The ten generators make up the Algebra of the group $O(4,1)$. It so happens that one of the unitary multiplets of

O(4,1) is indeed the entire sequence - the entire tower;

$$(i,k) = (0,0),\ (\tfrac{1}{2},\tfrac{1}{2}),\ (1,1),\ \ldots$$

The complete hydrogen spectrum, with all its excited levels, corresponds in this description to a single unitary representation of a (formal) group structure O(4,1). The structure is a symmetry of the problem by courtesy only. This is because at best O(4,1) is a symmetry of the free Hamiltonian $\frac{1}{2}p^2$ with the Coulomb potential left completely out. It is a "broken symmetry" - a highly broken symmetry indeed - yet it provides a "useful limiting symmetry", for it yields, at one go, the entire sequence of the levels.

(4) The co-variant version of the Schrödinger equation - the Bethe-Salpeter equation - equally admits of the O(4) symmetry group. This was first shown by Wick and Cutkosky.

This gem of a derivation of a dynamical symmetry for the hydrogen problem illustrates most of the approaches one has followed in connection with dynamical symmetries of elementary particles. Basically these approaches fall into two distinct classes:

(a) The Composite Models. Assume there exist entities analogous to electrons and protons, a triplet of quarks (q) with non-integral charges - or Van Hove-Nambu-Schwinger triplets with integral charges. Such triplets automatically allow for building in of the intrinsic symmetry SU(3). Set up a (non-relativistic) Schrödinger equation with a spin-unitary-spin independent force. This is where the basic physics goes in. One may or may not use group theoretic notions to solve the three-quark (qqq) or the quark-anti-quark ($q\bar{q}$) bound state equations. But the non-relativistic strong-binding limit is indeed the SU(6) limit. I do not wish to go into any details of the calculations made. Professor Dalitz has covered these admirably in his talk. One may, however, distinguish two distinct variants of the basic model:

Model I: The Atomic Model of Elementary Particles*, where one assumes that the inter-quark force is of a completely different order of magnitude than the strong forces we are used to and which determine the S-matrix in the relatively low-energy collisions (10-15 Bev) of the known composite particles. If quarks do exist and are very massive - and if we also believe that the origin of all mass (including quark mass) is dynamical in the last analysis - the high quark mass could be a manifestation of such a super-strong force.

Model II: The Liquid-drop Model. The inter-quark forces are assumed to be of the same variety and have the same symmetry characteristics as the forces responsible for baryon-meson scattering, etc. One may go even further and picture all

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* One wonders how long one may persist with the use of the word "elementary particle" for the known baryons and mesons. Only if no quarks are ever discovered would one be justified - perhaps in a bootstrap sense - to continue to use this word at all.

collision processes (particularly those at high energies) as proceeding entirely through the medium of the constituting quark matter.

The first model has been investigated extensively by Nambu; the second by Dalitz, Morpurgo, Tavkhelidze and others. Clearly the existence or lack of existence of quarks and the character of any inter-quark forces presents the most crucial question-mark which hangs over strong-interaction physics. It is also clear from the relative success of the somewhat crude models above how close in some respects our "elementary particle physics" may be to the physics of nuclei and atoms of yesteryears.

(b) The Phenomenological Group-Theoretic Approach. One may defer the problem of existence of quarks except as a mathematical auxiliary. Believing, as most of us do, that for systems of such tight binding, one is in the relativistic domain, one may feel shy of setting up Schrödinger equations. One may agree to work instead with abstract groups and their generators as distillations of a (spin-unitary-spin independent) dynamical situation. It is important to realise that for relativistic quantum physics, with all its complexities, this may well be the only type of "model-making", the only type of description possible. The art then lies in working with that (relativistic) formalism which goes most readily to the heart of the dynamics.

The heuristic approach outlined above has been further developed in two different but essentially complementary ways:

(i) Start with the known multiplets as phenomenological entities; like secret-field theorists (which most of us are) one sets up a phenomenological field theoretic framework to describe these. One needs necessarily a relativistic formulation of the symmetry and a setting-up of relativistic Lagrangians approximately invariant for it. Realizing the difficulties of working with strong interaction Lagrangians, one then tries to abstract from the formalism as much of the general features of the S-matrix as possible.

(ii) An alternative approach is the one followed by Gell-Mann (and following him by Fubini, Amati and others). This consists in expressing generators of any conjectured rest-symmetry group in terms of (hypothetical) quark fields; one tries to make an immediate bridge between dispersion theory (rather than via Lagrangian theory) and dynamical symmetries. Gell-Mann has eloquently described this method in detail in his lecture; he has particularly emphasised the hope that this may bring a synthesis for weak, electromagnetic and strong interactions.

Whichsoever approach one follows, the end speculations - speculations before they are experimentally confirmed - made in respect of what one might expect are identical. From this point on I shall divide my remarks into two parts. First, remarks concerning rest-symmetries and conjectured multiplet spectra; second, on possible symmetries of the S-matrix.

4. CONJECTURED REST-SYMMETRIES

(1) Let us start with SU(6). As is well known, some possible baryon and meson multiplets are: 56, 70, 700, ... and 1, 35, 189, 405, ... respectively, satisfying mass formulae of the type* $m = M_o + a\, \underset{\sim}{J}^2 + b\,(Y^2 - \underset{\sim}{I}^2/4) + cY$.

(2) The next important advance came with the generalization of SU(6) to the rest-symmetry U(6) x U(6), where (in making (formal) composites) quark and anti-quark spins are treated independently in the two groups U(6) x U(6). The multiplets are larger than SU(6) multiplets but not very much so; for example

	U(6) x U(6)		SU(6) decomposition
Mesons	$(6,\overline{6})$	=	$1^- + 35^-$
	$(21,\overline{21})$	=	$1^+ + 35^+ + 405^+$
Baryons	$(56,1)$	=	56^+
	$(126,\overline{6})$	=	$56^- + 700^-$

The assumption of multiplets as quark-anti-quark composites (with zero relative angular momentum) specifies their parity unambiguously (in contrast to SU(6)). Even though the symmetry can accommodate the $X^o(960)$ particle, its power appears not so much in the multiplets predicted as in (the relativistic) S-matrix analysis in connection with the $\tilde{U}(12)$ symmetry where the U(6) x U(6) rest-symmetry first arose.

* This is the phenomenological formula due to Gürsey and Radicati for baryons. For mesons, as is well known (though not well understood why), one uses squared masses. There are additional terms to split off singlets and octets within the nonets. In the present discussion I shall consistently ignore these.

** The group was considered also by Marshak and Okubo. This is different from the chiral group of Feynman, Gell-Mann and Zweig.

(3) "L-Excitations": or kinetic supermultiplets U(6) x U(6) x O(3): Gell-Mann, Sudarshan, Mahanthappa and others proposed a further generalization of U(6) x U(6) arising from orbital excitations in orbital angular momentum. Some possible multiplets are:

Mesons $(6,\overline{6},0)^-$, $(6,\overline{6},1)^+$, ...

Baryons $(56,1,0)^+$, $(56,1,1)^-$, ...

where 0,1,2, ... refer to ℓ-values.

Can one embed the known higher meson or baryon resonances in any one of these higher rest-symmetry schemes? Gatto, Costa and others have made a powerful case for assigning the known positive parity nonets $J^{PC} = 0^{++}, 1^{++}, 1^{+-}, 2^{++}$ to $(6,\overline{6},1)$. Figures 1 and 2 give the Gatto-Rosenfeld charts, with a possible mass formula*

$$M = M_o + a\,(\underset{\sim}{J}^2 - \underset{\sim}{L}^2 - \underset{\sim}{S}^2) + b\,(\underset{\sim}{I}^2 - Y^2/4) + c\,Y. \qquad (1)$$

(4) Non-compact symmetries: If indeed two of the multiplets $(6,\overline{6},0)^-$ and $(6,6,1)^+$ have already made their appearance, and if all symmetries are – as we believe – dynamical, there is no fundamental reason why physics of elementary particles should not follow the pattern of the hydrogen problem. There may indeed exist non-compact symmetries; some possible ones being the following:

(a) The "L-excitation" Tower:

The sequence: $(6,\overline{6},0)^-$, $(6,\overline{6},1)^+$, $(6,\overline{6},2)^-$, ...

constitutes but one irreducible representation of the non-compact

*The chief competition with Gatto assignments would come from the possibility of these resonances forming part of a $(21,\overline{21})$ of U(6) x U(6).

algebra, $U(6) \times U(6) \times O(3,1)$. If this indeed were to be the case, the constants a,b in the mass formula (1) may be expected to be universal numbers. One may then conjecture that the next set of meson resonances will fill 3^-, 2^-, 1^-, 0^- nonets with mass values lying between 1200 and 2100 Mev.

(b) Alternatively consider $U(6,6)$ (containing $U(6) \times U(6)$ as the maximal compact sub-group). Some of its simple representations are:

The Meson-Tower : $(1,\bar{1})^+$, $(6,\bar{6})^-$, $(21,\overline{21})^+$, ...

The Baryon-Tower : $(56,1)^+$, $(126,\bar{6})^-$, ...

In terms of quarks all these are S-wave bound structures, the higher-spins contained in each multiplet coming from increasing numbers of quarks and anti-quarks contained in each rung of the tower. The chief difference of the two non-compact schemes is the appearance of higher SU(3) multiplets like $\underline{27}$ and $\underline{35}$ etc. in $U(6,6)$*.

(c) Finally, and if the patience of the strong-interaction experimental physicist does not get completely exhausted, there may even be the possibility of towers associated with a "doubly non-compact" rest symmetry of the type $U(6,6) \times O(3,1)$, with a "double tower" and a possible mass formula:

$$M = \lambda N + a\,(\underset{\sim}{J}^2 - \underset{\sim}{L}^2 - \underset{\sim}{S}^2) + b\,(\underset{\sim}{I}^2 - Y^2/4) + c\,Y$$

where N = number of quarks + anti-quarks in the $U(6) \times U(6)$ specification. (N = 3 for $\underset{\sim}{56}^+$, 5 for $\underset{\sim}{700}^-$, 2 for $\underset{\sim}{35}^-$, 4 for $\underset{\sim}{35}^+$, etc. ....). Clearly, sub-nuclear spectroscopy has come

* Considerable pioneering work on non-compact rest-symmetries is due to Barut and Fronsdal at Trieste. The $U(6) \times U(6)$ containing schemes described above were first suggested by Dothan, Gell-Mann and Ne'eman.

to stay. From the very elegant work which Peyrou showed us yesterday - from the purposeful and elegant use of the phase-shift analysis - it would appear that there need be no despondency regarding filling up these towers for the theoretician. Beneath the mound of every broad resonance, apparently, there may lie buried three or four more.

To summarize, SU(6) was the first breakthrough in the possible chain of dynamical symmetries; the realization that the structure must be widened to U(6) x U(6) was the second important step; the recognition that the infinite-dimensional unitary representations of non-compact symmetries may play a role is the third exciting idea of the past year. All such non-compact groups must contain U(6) x U(6) as a sub-group. If the non-compact symmetry groups do find a place in elementary particle physics, the subject would achieve a synthesis deeper and still deeper with the physics of forty years ago.

5. THE S-MATRIX

Let us henceforth assume what I called the liquid-drop model; i.e. we assume that the same force that produces the multiplets is also responsible for their scattering. What symmetries may one possibly expect for the relativistic S-matrix? Are any of these realized in nature?

Dealing with relativistic particles as we shall be, clearly the first essential was to make a Lorentz completion of the (spin-containing) rest-algebras. This is relatively and formally an easy step; one can carry the completion through for any specified symmetry; e.g. :-

	Compact		Non-compact
Rest symmetry (non-relativistic)	SU(6)	U(6) x U(6)	U(6) x U(6) x O(3,1)
Relativistic (completion)	SL(6,c)	$\tilde{U}(12)$	U(6) x U(6) x O(3,2)

The analogy is with three components of spin, σ_1 , σ_2 , σ_3 the relativistic completion needs in addition

$\gamma_0\gamma_1$, $\gamma_0\gamma_2$, $\gamma_0\gamma_3$; i.e. we pass from the rotation group O(3) to the Lorentz group O(3,1).

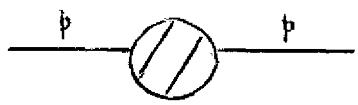
But what is one to do with these relativistic structures? Can any of these be considered as fundamental symmetries of a relativistic strong interaction Hamiltonian? From the view-point I have adopted throughout - that all dynamical symmetries are no more than symmetries pertaining to an idealized situation - the answer would be no. And this indeed was what was proved - by Michel, O'Raifeartaigh, Coleman and others in countless different ways. One could construct, for example, theories fully invariant for SL(6, c), (the relativistic completion of SU(6)) but only in a space-time of 35 dimensions. Every one of the SU(3) generators must be accompanied by their own space-time translations so that spin and unitary-spin could be truly on an equal footing. Since no-one knows how to pass from these 35 dimensions to the space-time we live in, it was clear once again that this was not, at least the immediate, way to progress. The survival of a spin-containing symmetry in a relativistic theory must be traced to a dynamical dominance of certain terms. Just as for the hydrogen atom a symmetry must owe its existence to a reasonable "accident".

But these "accidents" could in a large measure be systematized. As I said earlier, two distinct types of formulations have been tried; both give identical results in respect of what maximal symmetry one may expect for certain S-matrix elements. Professor Gell-Mann has already given a review of the current Algebras' approach; I shall briefly elaborate on the approach which starts with any rest-symmetry (compact or non-compact) and uses field equations to describe relativistic particles and their interactions.

To illustrate, consider the $\tilde{U}(12) = (M(12) = SU_{\mathcal{L}}(12))$ symmetry associated with a structure generated by 144 matrices, $\gamma_R \lambda^i$ where γ_R are the sixteen Dirac matrices and λ^i the nine 3x3 matrices of SU(3). The simplest realization of the Algebra is a 12-component Dirac quark, which - in virtue of Dirac's equation - exists in its rest-frame in six states (two states of spin for each member of the quark triplet) thus generating at rest a U(6) algebra. Likewise Dirac anti-quarks at rest generate another independent U(6) structure. All higher $\tilde{U}(12)$ multiplets (constructed formally from Dirac quarks and anti-quarks each satisfying a Dirac equation*) give therefore just the realization in motion of the rest-multiplets of U(6) x U(6).

Now given these moving U(6) x U(6) multiplets, what could be a possible relativistic strong interaction dynamics of which these multiplets are a manifestation? With the Lagrangian attitude this is not hard to specify. Using quarks (or the phenomenological multiplet field themselves) one could write $\tilde{U}(12)$-invariant interaction Lagrangians together, with free Lagrangians which (because of the spin-orbit coupling terms $\gamma_\mu p_\mu$ in Dirac's equations) break down the symmetry intrinsically. The question now arises; with this intrinsic symmetry breaking, can any vestige of the symmetry still possibly survive? An elegant answer was given by Gell-Mann and Dashen, Rühl, Harari and Lipkin. Write all S-matrix elements as sums of terms of two types $S = S_o + S_1$. The S_o terms represent either fully $\tilde{U}(12)$-invariant amplitudes or such amplitudes which include (following Oehme, Freund, Matthews and Rühl) Dirac combinations $\gamma_\mu p_\mu$ of all external momenta, S_1 are the remaining non-invariant symmetry-breaking terms. For reasons which will be clear later, I shall call S_1 terms the "unitarity terms".

Consider S_o terms first. Assume that for some reason these dominate. For S-matrix elements dependent on one 4-momentum



* The entire set of such equations is known as Bargmann-Wigner equations.

- and this includes Mass-matrices for all multiplets - we can pass to the rest frame. A general 4-vector p_μ has the transformation character of the Dirac 4-vector γ_μ. Starting with $\tilde{U}(12)$ in the rest frame (where $P = P_0,0,0,0$) clearly after inclusion of $\gamma_\mu p_\mu = \gamma_0 p_0$ terms the surviving symmetry group can be no more than that subgroup of $\tilde{U}(12)$ which commutes with γ_0 - the Dirac matrix corresponding to energy (P_0). This subgroup consists precisely of the 72 generators of $U(6) \times U(6)$. In grouptheoretic language it is the so-called "little group". The "little group" of $\tilde{U}(12)$ gives the rest-multiplets associated with the group.

Consider next processes involving two independent momenta (and this includes (a) vertex functions (b) $p\bar{p}$ annihilation at rest into two particles and (c) all forward and backward scattering). Using the same argument as above, the residual symmetry of S_0 terms is given by that sub-group of $\tilde{U}(12)$ which commutes both with γ_0 and γ_3 (in the frame where the two independent momenta can be written as $(P_0,0,0,0)$ and $(0,0,0,q)$). This is the so-called "collinear subgroup" $SU_W(6)$ (Lipkin and Meshkov) which consists of 36 generators*.

$$(1, \ \sigma_3, \ \gamma_0\sigma_1, \ \gamma_0\sigma_2) \times \lambda^i$$

For processes with three independent momenta the residual symmetry can easily be seen to be no larger than the 18-generator group $U(3) \times U(3)$.

One need not have started the analysis above with broken $\tilde{U}(12)$. The method is applicable equally to any non-compact rest-

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* Matthews and Charap have given $SU_W(6)$ the picturesque name of "the lesser group" and $SU(3) \times SU(3)$, the "least group".

symmetry; for example, starting from the Lorentz-complete structure U(6,6) x U(6,6), one successively gets the chain U(6,6) for the rest-symmetry, GL(6) for 2-momentum processes, U(3,3) for 3-momentum processes and so on*.

But this is not the end of the story. In addition to S_o terms, there are the S_1 terms. In general such terms will break the symmetry down to SU(3). For 1-momentum processes (the Mass-matrices) these terms will create mass splits among different spin particles in the same multiplet - something empirically very desirable. For collinear processes S_1 will destroy $SU_W(6)$. To see this one need only look at the unitarity equation for the T-matrix

$$\mathrm{Im}\, T = T \rho T^{+} \qquad (2)$$

Here ρ is the phase space for all intermediate states. Clearly the collinear $SU_W(6)$ could only be compatible with (2) if and only if all intermediate states were also collinear. Since this is impossible, the S_o approximation to the S-matrix must always be supplemented with

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* Contrary to what has been asserted sometimes, the use of infinite-dimensional unitary representations of non-compact groups like U(6,6) has no bearing whatever on the resolution of the so-called "unitarity" dilemmas of broken symmetries. The collinear symmetry, GL(6) for example, is compatible with unitarity only for collinear intermediate states in relation (2).

Some fascinating mathematical problems arise in connection with attempts to couple three infinite-dimensional towers. If only the symmetries were not so badly broken, what fun it would be to have myriads of couplings expressed as (known) functions of just one parameter - fun surely for both the theorist as well as for the experimental physicist. The formal mathematical problems of writing down such couplings have been solved by Fronsdal, Gell-Mann, Delbourgo, Strathdee and Salam.

(the unitarity terms) S_1. This, in the literature, has been called the "unitarity dilemma" of approximate symmetry theories. Unitarity is an intrinsic symmetry breaker for relativistic spin-containing symmetries. The important, the unresolved question is: when do the symmetry-exhibiting terms S_o dominate, for what situations and for what dynamical models are the S_1 terms relatively unimportant in magnitude?*

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* It is not completely impossible to invent such models where $\tilde{U}(12)$-invariant terms could dominate. I am reminded of some recent work of Yang and Byers who analyze forward and backward scattering (collinear process) both elastic and inelastic for momenta 5BeV/c and up. They note - as indeed was done so forcefully by Lindenbaum at this Conference - that there exist at small angles enormous peaks, rising above the small value of the large angle differential cross-sections, irrespective of the quantum numbers exchanged. They interpret known data to indicate a great difficulty to transfer large momenta - shades of invariant interaction terms in SL(6,c) or $\tilde{U}(12)$ - but relative ease in coherently transferring quantum numbers like charge, spin, strangeness and nucleon number. They picture elastic and exchange processes as very much similar to the passage of a particle through an absorptive medium with or without its coherent excitation; they invoke a "droplet" model of elementary particle structure though they do not attempt to relate this with any ideas of quark matter.

Concluding then; I believe that in order to determine when dynamical symmetries will survive in an S-matrix situation, it is not enough to enumerate the chain of maximal possible symmetry sub-groups. One must go further and investigate how and when it is possible that the symmetry survives the unitarity corrections. The hope that one can go on circumventing dynamical considerations cannot for ever endure, though with the symmetry method one has made a fair go at this.*

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* "One wonders how Herodotus could believe in the Oracle of Delphi, in his time, as he was an intelligent man. What really happens is that each of the predictions of the oracle are in vague language and they become particularly clear when the event occurs afterwards ... The high priests of Babylon used to predict events by looking at the liver of sheep. And why? Because in the complexity of the arrangements of the veins interpreted correctly, they could tell what the future may be. It is that complexity, and the possibility of reinterpreting later .... that permits the power of the priests to be maintained." - Feynman on the predictive role of dynamical theory, Aix-en-Provence (1961).

6. THE EXPERIMENTAL SITUATION

What is the experimental situation? How far, for example, does the collinear symmetry (i.e., the S_o part of $\tilde{U}(12)$ - survive? The experimental situation is indeed most tantalising. As I said earlier, three types of tests have been attempted.

(1) The Vertex Function:

These include:

(a) baryon-meson decay processes,

(b) meson-meson decay processes.

There is a host of predictions here*; by and large all agree well (within 5-10% or so) with experiment.

(c) the electromagnetic form factor.

Electromagnetic form factors are the joy and pride of the $\tilde{U}(12)$ symmetry physicist. In fact it was these that started some of us off on the search for a symmetry larger than SL(6,C) in the first place. Let me state the argument in a somewhat simplified manner.

Ever since 1962, when Barnes at Imperial College first noticed this from empirical data, one had worried about the astounding experimental fact that there appeared just one (Sachs) form factor for the proton (electric and magnetic) as well as for the neutron (magnetic) for all known momentum transfers. In Figure 3,I reproduce the beautiful slide shown by Pipkin which

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* These include predictions in respect of the decay of 2^+ mesons also as shown by Gatto, Costa and Delbourgo.

summarizes all known data. There could but be just one explanation; some very powerful symmetry principle must be at work.

Now SU(6) (or rather its rather relativistic version SL(6,c)) gave a ready explanation of the equality of the proton and neutron magnetic form factors. It did not, however, relate the electric form factor to the magnetic. Nor did it account for the additional remarkable circumstances of the neutron's electric form factor being essentially zero. Some extension of the group seemed absolutely essential.

In terms of group algebras what was needed was clear. Simplifying the argument one essentially wanted an equality of the γ_μ and $\sigma_{\mu\nu}$ form factors. From the well-known Goldberger-Treiman relations one also knew that the pion form factor γ_5 was closely related to the axial vector form factor $\gamma_\mu\gamma_5$. Empirically, therefore, one wanted a symmetry principle which should assert (in a rough manner of speaking):

$$\gamma_\mu \approx \sigma_{\mu\nu} \quad , \quad \gamma_5 \approx \gamma_\mu \gamma_5 \,.$$

The spark provided by SU(6) was that vector (1^-) and p.s. (0^-) particles form parts of a single multiplet - again crudely speaking - from SU(6);

$$\gamma_\mu \approx \gamma_5$$

Clearly one needed a generalization giving:

$$\gamma_5 \approx \gamma_\mu \approx \sigma_{\mu\nu} \approx \gamma_\mu \gamma_5$$

i.e., a generalization which treated all sixteen Dirac matrices on a footing of equality - in fact the symmetry $\tilde{U}(4)$. Combining with SU(3) this is the structure $\tilde{U}(12)$.

So far so good. But why does the symmetry persist, why are the unitarity corrections S_1 empirically so small? I believe the answer could lie in that for space-like momenta - and these are the momenta accessible for electron scattering - the form factors are purely real. From all one's work with dispersion theory one knows it is Im T which is more directly sensitive to unitarity. Thus for time-like momenta, where Im T is very much alive, it would indeed be an agreeable surprise if the form factors did exhibit the same lively traces of the symmetry.

(2) Consider next $p\bar{p}$ annihilation at rest into 2 bosons. Groups at Columbia and CERN have presented somewhat complete experimental data for the various channels. For $\pi^+\pi^-$: K^+K^- : $K^oK^{\bar{o}}$ the experimental ratios are:

$$3 : 1 : \tfrac{1}{4}$$

The straight S_o terms give:

$$1 : 2 : 1 \qquad (3)$$

Clearly the symmetry-breaking terms S_1 are exceedingly important. This is sad - undoubtedly so. But we have met precisely this situation before - and for that well-established part of classical physics, the SU(3) symmetry. I had the privilege last year of reviewing evidence for SU(3) at the Dubna Conference. From the then available data one could conclude that whereas the SU(3) symmetry had a number of remarkable successes in predicting the existence of multiplets ($\underset{\sim}{8}$'s and $\underset{\sim}{10}$'s), and in correlating vertex function predictions (the same region where $\tilde{U}(12)$ seems to succeed), one would never have given much credence to the symmetry if the only evidence for it came from scattering processes. Figure 4, from a slide shown by Meshkov, Yodh and Snow at Dubna, sets out one comparison of theory with experiment; the prediction $\frac{1}{3}\sigma_a = \sigma_b = \sigma_c = \sigma_d$ (4)

is clearly strongly contradicted by experiment.

More recently, Harari at Trieste made a further exhaustive analysis of SU(3) predictions. His conclusions are the following:

(i) The predictions of (phase-space corrected) SU(3) are in numerous cases incompatible with experiment.

(ii) In reactions with non-strange initial particles, the production rate of strange particles is experimentally smaller by one order of magnitude than, say, charge-exchange cross section.

Is this alarming? No-one thinks so. For in all fairness to the symmetry, one must include in any such comparison also amplitudes which arise from the strong symmetry breaking. It is fair and consistent because the Gell-Mann-Okubo mass differences* arise from the same source. Harari did precisely this; he included in reactions like (4) symmetry-breaking spurions to take account of symmetry breaking to the lowest order (spurions (S) are 0^+ object of zero momentum and energy);

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* Why the lowest order corrections work so well for the mass formulae is of course another (dynamical) mystery on which one has thrown a cloak of silence.

i.e., consider not just the process

$$M + B \rightarrow M + B$$

but instead

$$M + B \rightarrow M + B + S \ .$$

The number of amplitudes now increases; equalities of the type (4) disappear; one is left in most cases with inequalities to compare with experiment. All such inequalities are satisfied.

Likewise, for $SU_w(6)$, it is imperative that S_1 corrections are made. From the work of Pais, Bég and Singh and others, one knows that the S_1 corrections for mass splitting can be taken into account group-theoretically by inclusion of spurions $\underset{\sim}{35}^{(8)}$, $\underset{\sim}{405}^{(1)}$, $\underset{\sim}{405}^{(8)}$. Just this has recently been done by C.S. Lai for $p\bar{p}$ annihilation; he derives instead of (3) a sum rule

$$A(\pi^+\pi^-) + A(K^o\ \overline{K^o}) - A(K^+\ K^-) = 0$$

in good agreement with experiment.

(3) Forward and Backward Scattering.

Here again one has an anomolous situation. As is well known, the original relations of Johnson and Treiman

$$\frac{1}{2}\left[\sigma(K^+\ p) - \sigma(K^-\ p)\right] = \sigma(K^o\ p) - \sigma(\overline{K^o}\ p)$$

$$= \sigma(\pi^+\ p) - \sigma(\pi^-\ p)$$

are fairly well obeyed (see Lindenbaum's contribution to the Conference). There are in addition a host of other predictions which were derived by Carter, Coyne and others from

$SU_W(6)$ symmetry for

$$M + B \rightarrow M' + B'$$

where initial and final particles are not the same. Dr. Jackson has analyzed some of these during the Conference; he concludes that most of these predictions disagree badly. Clearly the predictions of the symmetry are highly sensitive to the mass differences; one must make a re-analysis with spurions included, an analysis of the type made by Lai for $p\bar{p}$ annihilation before a last word may be pronounced on the prospects of the symmetry.

Summarizing then, starting with any (broken) relativistic symmetry, we can identify for the S_0 terms, a chain of maximal symmetry sub-groups. The largest is the sub-group of rest-symmetries. This predicts the multiplet structure. The next largest is the collinear group, the next coplanar, and so on. The S_1 terms (the unitarity corrections) are expected to break this chain. For (dynamical) reasons we only vaguely comprehend, all symmetry-breaking terms - and this includes the empirical situation for SU(3) as well - affect multiplet sequences and vertex function predictions the least. It is an open question whether starting just with the predicted multiplets and their vertices as input, one can use dispersion techniques to compute reliably other S-matrix elements. I believe Gatto and Wali have started on this ambitious programme. In the coming years I hope we shall finally know the answer.

7. CONCLUSIONS

Where do we go from here? Do quarks exist? Or - as the bootstrap physicist has always believed - is SU(3) also a dynamical symmetry? What higher approximate symmetries are likely to emerge in the future? Would the "universal" couplings of non-compact towers - or at any rate the towers themselves - ever be manifest? One must confess that the viewpoint I have expressed - and which seems fairly generally shared among theorists - about the role of symmetries and the symmetry method divests the subject of part of its romance. The thirty-five-dimensional space-time structure with its promise of geometrising strong interaction physics may perhaps have been more exciting, if only we could survive with it. As it is, our next efforts will inevitably bend more and more towards marrying symmetries and dynamics, symmetries and dispersion theory - if only to estimate more reliably possible deviations from the symmetry magnitudes. One wonders if a more powerful formulation of the unitarity relation is in the offing with the use of non-compact groups or if that is a forlorn hope, as forlorn as the use of the multiple dimensions.

The other major topic which has engrossed our Conference is CP - or even C - violation. I have purposely refrained from mentioning this, for after the complete and elegant discourses of Prentki, Steinberger and Bell, and the coup achieved by T.D. Lee in drawing a distinction of baryon, lepton and electric charge conjugations, there is very little more that one can say. I have confined myself to strong interactions; I hope I have conveyed the sense of achievement made, tinged always with the realization of how far we must yet travel. There is the prospect that we stand on the threshold of a new chapter in the subject; there is today, as ever, an exhilarating vitality on the frontier it is our privilege to explore. I, for one, look very much forward to the years to come.

Tentative predictions for higher boson resonances

Kinetic supermultiplet U(6) x U(6) x O(3) = $(6,\bar{6},1)$

J^{PC}	T = 0	T = 1	T = ½
2^{++}	(1560 ± 50)← f°(1253 ± 20)	A_2 (1310 ± 15)	K^* (1430 ± 15)
1^{+-}	(1270 ± 30)← (1215 ± 15)←	B (1215 ± 15)	Two possibilities: (i) K' = K^*(1175) K" (1100 ± 40)
1^{++}	(1180 ± 60)← (990 ± 70)←	A_1 (1090 ± 15)	(ii) K' = C (1215) K" (1050 ± 40)
0^{++}	σ^0(390) ? ϵ^0(730) ?	(970 ± 50)←	K (725)

The predictions in this table contain an additional assumption: that the mixing between the two T = 0, 2^{++} mesons is maximal (like φ-ω). The masses are in MeV. The input data are underlined. Arrows indicate the predicted masses. A possible completition of the 0^{++} nonet with σ^0 and ϵ^0 is also indicated.

Figure 1

MESON RESONANCES

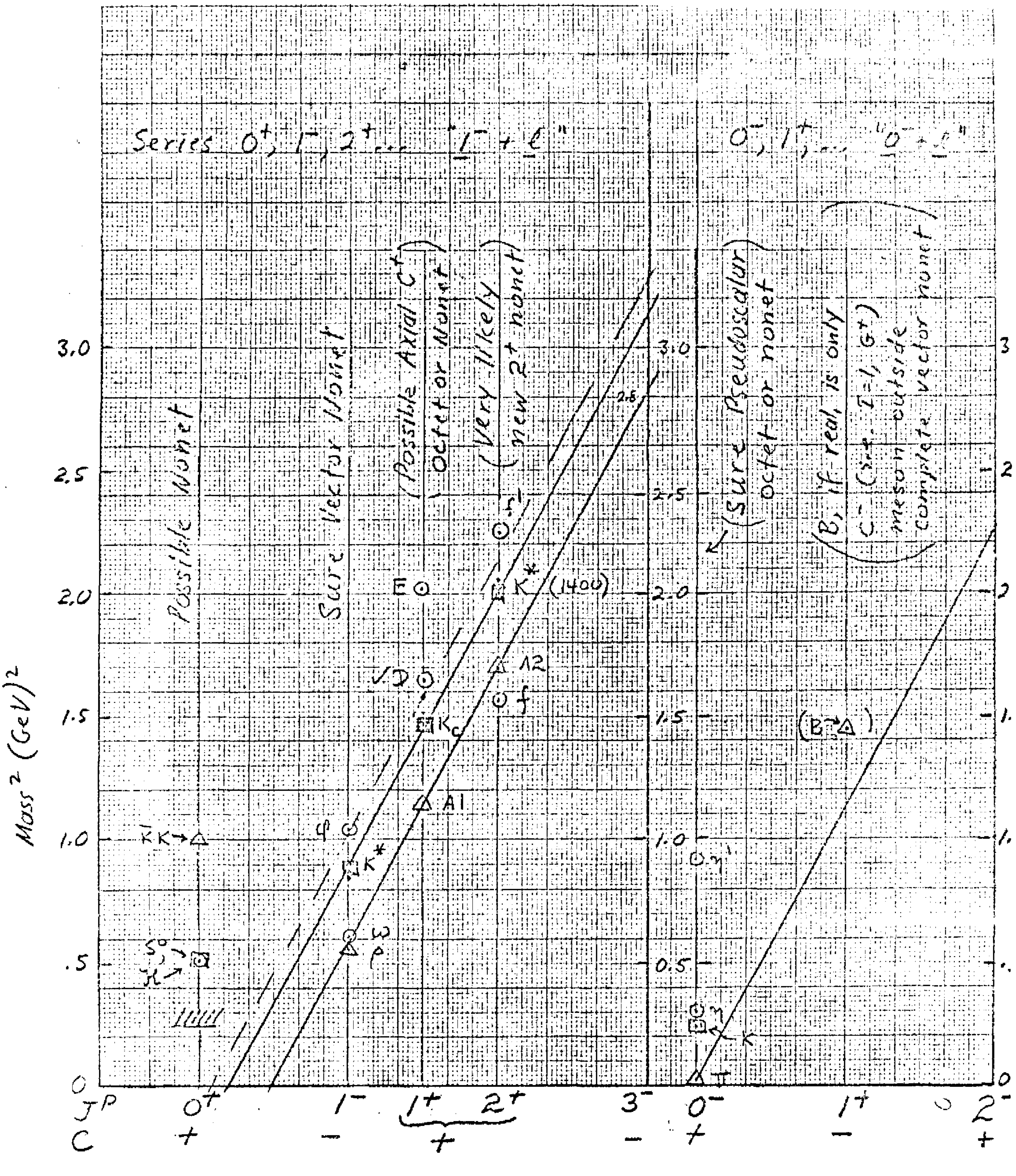


Figure 2

ELECTROMAGNETIC FORM FACTORS

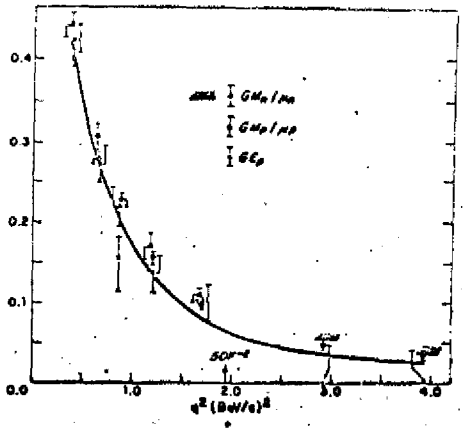
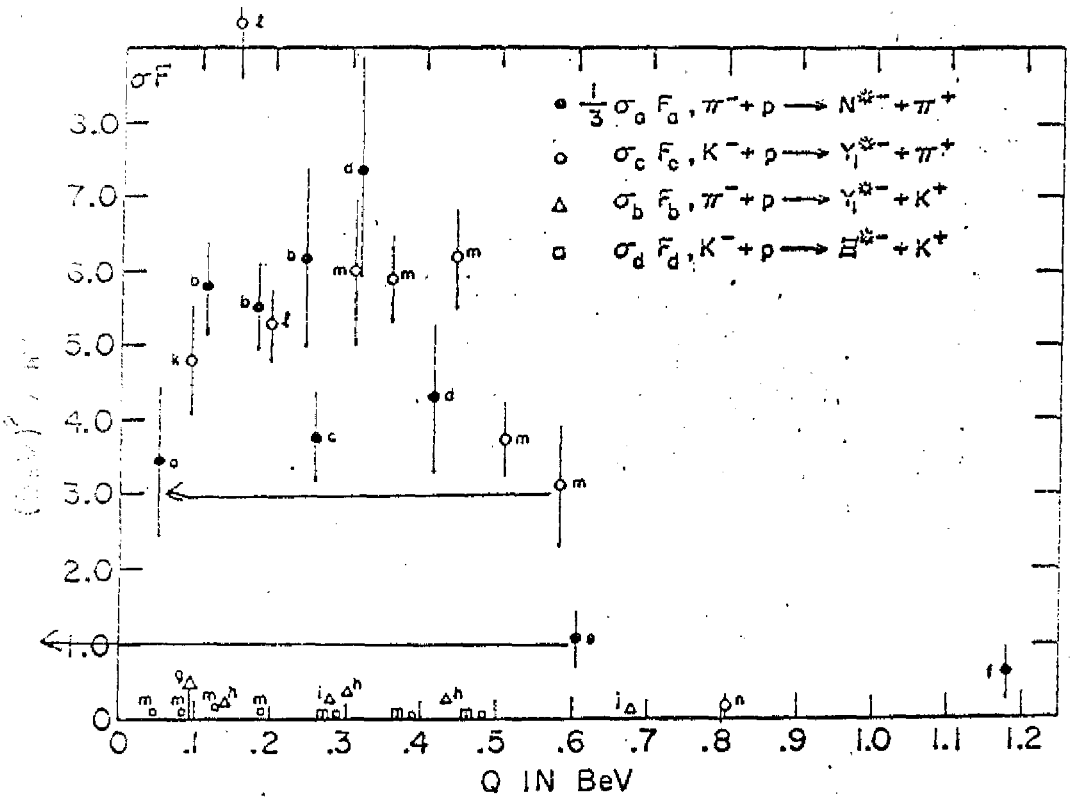


Figure 3

COMPARISON OF SU(3) PREDICTIONS WITH EXPERIMENT

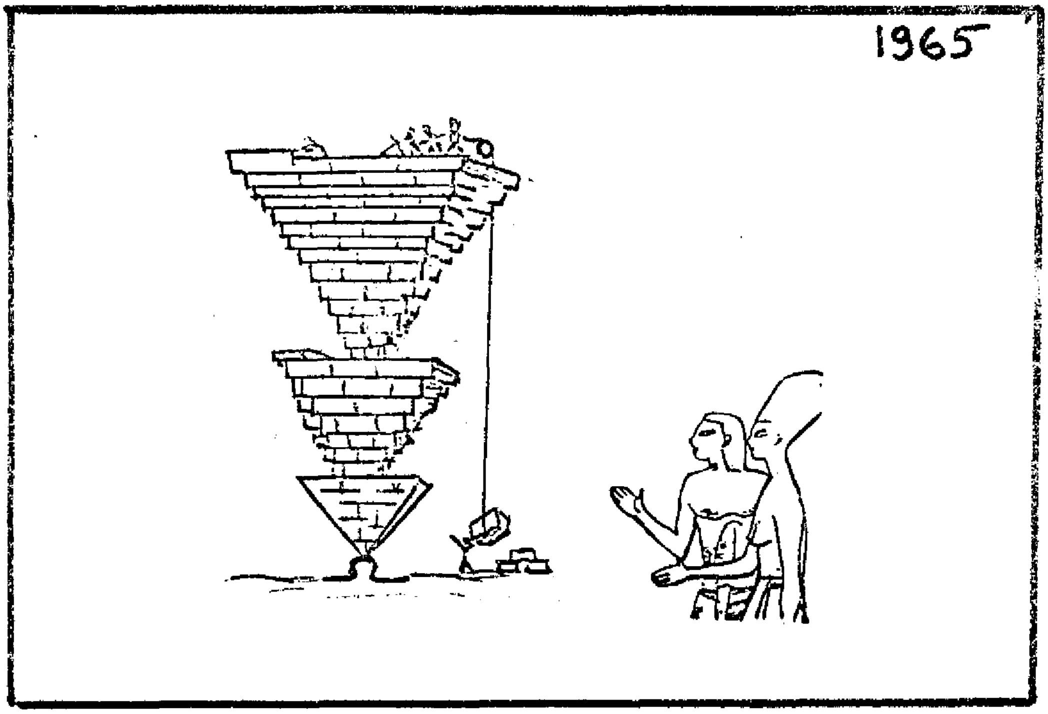


SU(3) Prediction: $\frac{1}{3}\sigma_a = \sigma_b = \sigma_c = \sigma_d$

Figure 4

APPENDIX

Ever since I was asked to speak at Oxford, I have been haunted by thoughts of pyramids and the awesome fate of trying to continue the series started in 1962. With infinite-dimensional representations in the offing, and with all that went before, I believe Figure 5 of the pyramid sequence may well convey the spirit of Physics of 1965. For the idea I am indebted to Professor Okun.



"Now this will stand for centuries!"

Figure 5