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SKYRMIONS AND ANOMALIES

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"First law of physicists: *without experimentalists, theorists tend to drift*

Second law of physicists: *without theorists, experimentalists tend to falter*".

T.D. Lee

When I was first asked to summarize this meeting, my first impulse was to say no. I felt that I couldn't do it and in any event, I saw no need for it. But then the organizers assured me I could talk about anything -and in anyway- I like, so I accepted to do so, with the understanding that I don't have to do the ordinary kind of summary that nobody cares about and I could go about talking, instead, on something that I feel strongly about. I am going to do just that.

Within the last two and half days, we heard nothing but Skyrmions and variations thereof: how beautiful their theory is, how well (or badly) they reproduce Nature, how elegant their mathematics is, what fascinating connections there are to other discoveries in physics and where to go from there etc.

I am impressed by three remarkable things about this meeting.

The first is that both nuclear and particle theorists are gathered together talking about one common thing. This is an unusual event, though it is happening more often nowadays. The second is that although everyone had QCD in mind, nobody talked about quarks and gluons *explicitly*. If they appeared somehow, they were quickly "integrated out" as if they were unwelcome intruders. This is a drastic change from, say, seven years ago. It would have been considered scandalous or hopelessly outdated to talk about mesons while addressing such problems as the magnetic moment of the proton, the Δ-N mass difference and so on. In fact, at that time, many high-energy physicists were campaigning to bury Yukawa's meson theory of nuclear force and replace it by quark-gluon-only models. (I will later come back to tell you about a minority of physicists who refused to join this bandwagon). The third remarkable thing is that we could discuss whole-heartedly with such great enthusiasm on an overwhelmingly theoretical issue as Skyrmions and anomalies *without* having to make excuses to experimental colleagues (who I believe all happily managed to avoid coming here) and hence *without* the slightest obligation to Nature, i.e., to predict something that can be quickly killed. Now this is nice in some sense and I have learned a great deal that I would have been less eager to care about if it had been an ordinary meeting of mixed andience. But also there is a danger to this: as T.D. Lee puts it as a law of physicists, left uncontrolled by reality, theorists tend to drift off.

What did we learn in this meeting? To analyze what we learned, let me (quite arbitrarily) divide the principal issues of this meeting into five categories:

1) QCD effective Lagrangians; 2) chiral bags and the Cheshire Cat principle; 3) strangeness problem; 4) phenomenology; 5) mathematical structure. I won't follow these divisions in order, but it is useful to think in terms of these five items.

To start with, let me discuss the effective Lagrangian philosophy. Everybody will agree that it will be marvelous to derive, starting from the QCD action (Γ_{QCD}), an effective Γ_{eff} written in terms, wholly or partially, of low-energy effective fields that would predict low-energy hadron properties as accurately as one wishes. Many people have tried and developed some powerful techniques on making verious expansions. But I think the result is very meager indeed. In (1+1) dimensions, since fermion theories can be bosonized, $(QCD)_2$ could be written in terms of boson fields and detailed analyses on the spectra and static properties of "hadrons" could be made. Alas, we are not living in a (1+1) dimensional world and all the nice things in two dimensions are not much of help to the real world of (3+1) dimensions where no such bosonization scheme is in sight. So, most of the Γ_{eff} so far obtained for $(QCD)_4$, at least the part in which we can have some confidence are valid only in the limit of zero energy. In this limit, we are sure of two terms. One is the current algebra term

$$\frac{F_\pi^2}{4}\int d^4x \, \mathrm{Tr}\left(\partial_\mu U^\dagger \partial^\mu U\right)$$

and the other is the Wess-Zumino term

$$\frac{-i}{240\pi^2}\int_{M^5}\mathrm{Tr}(U^\dagger dU)^5$$

These two terms account correctly for soft-meson theorems and flavor anomalies. Can we get higher derivative terms and/or massive meson terms correctly? Here the situation is totally unclear. Some people integrated out the quark field ψ from a chiral quark σ-model, say,

$$\mathcal{L} = \bar{\psi} i \gamma . \partial \psi - M\left(\bar{\psi}_L U \psi_R + \bar{\psi}_R U^{\dagger} \psi_L\right),$$

$$U = \exp(i\ \lambda . \pi / F_{\pi})$$

and obtained, in addition to the above two terms, some quartic terms resembling Skyrme's quartic term and also higher derivative terms. It is not surprising that the σ-model gives the current algebra term: we have known it since the heydays of current algebra. Neither is it surprising that the (topological) Wess-Zumino term is correctly given by the model: it is in the Fujikawa measure. But it would have been most surprising (at least to me) if the higher derivative terms had come out right. And in fact they don't! It is easy to see why. The chiral quark σ-model is at best a caricature of QCD for length scale much larger than the chiral symmetry scale $\Lambda_{\chi}^{-1} \sim (1\text{GeV})^{-1}$ and cannot be trusted outside of that scale. Higher derivative terms reflect dynamics at a shorter distance scale for which the naive σ-model is not suitable. Hence one cannot address, with the σ-model, such issues as stability or instability of the QCD vacuum and excitations thereof (e.g. baryons). It may be an interesting mathematical question, but not much relevant to Nature.

I don't mean to say that all chiral quark σ-models are totally useless for phenomenology. One can do a systematic chiral expansion in inverse power of the chiral scale parameter $\Lambda_{\chi} \sim 1\text{GeV}$ as Georgi does in his book "Weak interactions and modern particle theory" (in accordance with Weinberg's "theorem" of 1979), determine the coefficients phenomenologically and then calculate the baryon properties in mean field approximations. One shouldn't do any further loops, in the sense that the effective theory is defined. As Banerjee and his coworkers demonstrated, this scheme does not work badly for the nucleon and the Δ. I think this is also Truong's philosophy.

Is there any hope of ever "deriving" a Γ_{eff} with a great accuracy? Most probably *not* if one wants to approach a kinematic domain in which asymptotic freedom sets in. Even in the limit $N_c \longrightarrow \infty$, it will require an infinite number of meson fields to approach asymptotic freedom. Even if one had a systematic way of calculating further and further corrections, it would be too complicated to be of any use. So let's forget it or be a bit cleverer (or modest). If one, however, confines oneself to an energy regime ~ $\Lambda_\chi \approx 1$GeV, one can do quite a few things of great interest in low-energy hadron physics, i.e., nuclear physics. We should not expect a great accuracy (for which we should leave the task to future lattice gauge calculations), but aim more at an overall qualitative understanding of hadron dynamics. One can do this by "deriving" Γ'_{eff}s in several stages as Aitchison suggested in his talk. This resembles "faking" a marathon by several successive shorter runs with rests in between. It's clearly not a real stuff, but it's one way and better than nothing.

An initial step in this direction was made by Peter Simic three years ago and much progress has since been made by Richard Ball as we heard in this meeting. The first stage is to integrate out the gluon field from the QCD action. In doing this, Simic makes two important observations: bag formation and hidden chiral gauge symmetry. That a bag-like (or string-like) structure arises when one integrates out the gluons has been widely recognized and was studied in detail by Adler in 1979. For simplicity, Adler considered systems with static color sources (i.e., massive quarks). Simic discussed, albeit heuristically, how a bag (or more precisely a chiral bag) could arise in light-quark systems.

Another important point that Simic recognized but did not fully exploit is that when gluons are integrated out, there remains in the action a background term that is a chiral $U(N_f)\times U(N_f)$ (N_f = number of flavors) gauge invariant functional of bilinears in quark fields. What this implies is that there is a hidden local gauge invariance and in particular that the vector mesons observed in Nature, ρ, A_1, ω could be identified as hidden gauge particles, as suggested by Bando, Kugo, Uehara, Yamawaki, Tanagida and others. I couldn't quite figure out from the high-speed transparencymanship of Ball whether the conjecture of Bando et al comes true in his derivation. In any event, Ball has the vector mesons appearing in a natural way.

The next stage of construction of Γ_{eff} is to integrate out the quark field. Ball described how to do this with the incorporation of the hidden gauge bosons. Since he has ignored gluon radiative corrections (therefore no trace anomaly), he cannot obtain a bag formation, but it shouldn't be too difficult to do so. I hope he does.

This then suggests how one can climb up the ladder of energy scales: first zero energy X_χ with Goldstone bosons; next an energy scale of $\Lambda_\chi \approx M_V \sim 1\text{GeV}$ through a hidden gauge symmetry and so on. What comes next in the ladder and if one has one, what guiding principle is there for constructing an effective Lagrangian? We don't know at the moment. According to the recent work of Bando, Fujiwara and Yamawaki, if one wants to correctly incorporate the A_1, yet higher derivative terms and/or higher mass mesons seem to be needed to recover low-energy theorems some of which go to pot in the presence of the A_1. This could mean that there is yet another energy scale that must be taken into account for phenomenology at the A_1 mass scale.

We heard many talks on the phenomenology of Skyrmions with vector mesons. It appears quite satisfactory for the two-flavor (u,d) baryons, particularly for electromagnetic and weak form factors of the nucleon, as we heard from Meissner. (And in some cases, even for three-flavor (u,d,s) systems as Karliner told us). This is quite pleasing, specially for nuclear physicists since the results show clearly that the concept of "size" of the nucleon depends upon the probe, something that has been suspected since a long time. All is not well, however. While as we heard from Loiseau and Meissner the troublesome g_A is now approaching 1, thanks to an important role of the Wess-Zumino term (the rest 0.25 could easily come from fluctuation effects of higher order in N_c^{-1}, not yet calculated), the nucleon mass is still too high by more than 400-500MeV. I believe that this is a signal that more degrees of freedom than what the present Lagrangian contains are needed. This was also the conclusion of Truong. (The situation may be likened to the ground state of nuclei or nuclear matter. The energy of the nuclear matter has been found to be extremely difficult to calculate accurately, requiring all *ranges* of NN interactions, while excitation properties can be quantitatively understood). How do we go about incorporating more degrees of freedom? Quarks, we heard some say.

My impression of what the effective Lagrangian enthusiasts have been looking for is that they eventually want explicit quark degrees of freedom to appear in the background of the chiral soliton. They don't say it, but what they mean, I interpret, is a chiral bag structure, to which it seems to me they are heading.

The version of the chiral bag discussed in this meeting was invented by nuclear physicists for reasons completely unrelated to the intriguing mathematical properties that have been recently discovered.

I will tell you more about them later. The most fascinating aspect of the chiral bag is the possible realization of the Cheshire Cat Principle, as discussed by H.B. Nielsen (who coined this name). As shown by Nadkarni, Nielsen, Zahed, Perry and others, the Cheshire Cat Principle (CCP) holds exactly in (1+1) dimensions: one may divide the space occupied by the hadron into as many subspaces as one wants and in any configuration one wants, letting some spaces occupied by "quarks" and other spaces by "mesons". If one imposes an appropriate chiral (bag) boundary condition at each boundary and takes into account proper Casimir effects (or anomaly effects), then the physics must not depend at all where and how many boundaries are set up. It has not been demonstrated explicitly but it is believed that the CCP does not depend upon interactions either. This phenomenon occurs because in (1+1) dimensions, a fermion field can be exactly bosonized (and vice versa) and the bag boundary condition -and the anomaly effects- just reflects the bosonization conditions

$$\partial_\mu \phi = \sqrt{\pi}\, \bar{\psi}\, \gamma_\mu \gamma_5 \psi$$

This is all very nice and clever in (1+1) dimensions but what about (3+1) dimensions? As mensioned before, no bosonization rule has been discovered -and it's not known whether it exists or not- in four dimensions, so demanding that the CCP holds is tantamout to requiring that all the radiative corrections in the quark-gluon sector and an infinite number of meson fields and their fluctuations in the boson sector be calculated, which is of course impossible in practice. Remarkably enough though, Nature is kind enough to let us have an *approximate* Cheshire Cat conditions with the minimum number of degrees of freedom within a reasonable range of bag radius, as demonstrated by the work of Andy Jackson and his coworkers. Consider the simplest chiral bag configuration of a spherical bag of radius R in which nearly massless quarks interact weakly via gluon exchanges, surrounded by hedgehog pion fields subject to a bag boundary condition analogous to the (1+1) dimensional bosonization condition. (If you don't like this configuration, try a swiss cheese configuration. I don't think it would matter). This model (customarily called *chiral bag model)*, the Stony Brook group finds, supplies a fairly respectable Cheshire Cat property for $0 \lesssim R \lesssim \frac{1}{2}$ fm, the range nowadays favored by phenomenology. As often the case in the calculations of Casimir effects, there are singularities in the theory: there are controversies as to what they are and how to tame them, as we heard from Vepstas, Jezabek and Heller. But to paraphrase Einstein, I believe that "God does not care about our mathematical difficulties; He integrates empirically". I am not very worried. In fact, I take the phenomenologically motivated calculations of the StonyBrook group to indicate that one is on the right track.

The support for an approximate Cheshire Cat picture comes not just from theoretical exercises, but more compellingly from Nature. I will show you later some amazing things in nuclear physics that beg to be given a Cheshire Cat interpretation. As a whole, the evidence is overwhelming that a dual description -that in terms of QCD variables, namely quarks and gluons and that in terms of effective variables, namely mesons and baryons- must exist: perhaps best known to particle physicists is the EMC effect which has recently stirred up a great deal of excitement among both particle and nuclear physicists. The Cheshire Cat Principle frustrates the search for a "smoking gun signature" for quarks in nuclei, but provides us with an elegant solution of many puzzles in nuclear physics.

Skeptics still ask whether there is any solid evidence for a *chiral bag* (or in that matter, for a *bag* of any type). The answer is probably not: we have only an overall qualitative support for it. But I think this is a wrong question to ask. The right question is: are the assumptions that go into the model consistent with the known premises of QCD and is the model predictive? To me, the chiral bag is the only model that has the potential to encompass a wide range of applicability, from long to short-distance regimes. Some people seem to be bothered by the boundarly conditions, particularly by the sharp ones people use in solving the model. They should not be. After all, the boundary conditions are mere artifacts of simplification and one can do without them (if one wishes) as in monopole-fermion systems. (Incidentally, in a suitable limit, the Banerjee-Broniowski model that we heard yesterday will also go over to the chiral bag model and to an approximate Cheshire Cat once anomaly effects are taken into account). Some people are allergic to the notion of bag. Let me just remind them to make sure to incorporate in their favorite model the QCD trace anomaly.

Now what sorts of great things can one do with the chiral bag if the CCP makes sense in a prescribed way? So far nothing much. But as Nielsen suggests, we should at least be able to calculate the parameters of effective QCD Lagrangians. In particular, we should be able to calculate F_π, the $\rho_{\pi\pi}$ coupling g, etc and "derive" the KSRF relation, the Weinberg spectral sum rules and so on. This would be wonderful. Also we should be able to describe with chiral bags what happens in the dense interior of neutron stars, in relativistic heavy-ion collisions and other short-distance processes.

Calculating Casimir effects in the chiral bag is a painful process. On the other hand, ignoring them leads to a nonsense. So some people invoking the CCP, shrink the bag all the way to a point and work with a Skyrmion, suitably generalized with vector mesons. The results, as we know from the work of Jackson, Vepstas, and coworkers, are just as good as any other models, as long as we restrict to $0 \lesssim R \lesssim \frac{1}{2}$ fm, and to SU(2) flavors. However something goes wrong, obviously, somewhere in the CCP when a strange quark s is added (when we have the SU(3) flavor symmetry). We were told by Nowak and Praszalowicz that the Skyrmion for the hyperons does not work. Since quark models have no problem with the hyperons, this means that the Cheshire Cat does not work. We do not know what the real cause is, but a good guess may be that the strange quark mass is, at the same time, too big *and* too small. Too big compared with the up and down quark masses (which are negligible compared with the QCD scale $\Lambda_{QCD} \sim 200\text{MeV}$), so the concept of SU(3)×SU(3) chiral symmetry becomes a suspect. Too small, because it does *not invalidate* the notion of chiral symmetry as the charm and beauty quarks do, as is indicated by the approximate SU(3) symmetry in the vacuum condensates $\langle 0|\bar{u}u|0\rangle \simeq \langle 0|\bar{d}d|0\rangle \simeq \langle 0|\bar{s}s|0\rangle$ (the latter deviating at most by 20%). So while soft-kaon theorems work rather well the phenomenology of SU(3) Skyrmions is miserable. This suggests that if we are to treat all three quarks (u,d,s) in terms of a chiral symmetry, we better treat the chiral symmetry breaking term $H_{SB} = m_u\bar{u}u + m_d\bar{d}d + m_s\bar{s}s$ non perturbatively. This is the message of the works by Yabu and Ando and by Nowak and Praszalowicz.

Suppose the s quark mass is so "big" that we can treat the strangeness differently from the u and d. Then one can do what Callan and Klebanov did, namely bind kaons to an SU(2) soliton to construct the hyperons. As Nowak told us, if one does this, the Wess-Zumino term which does miraculous things in the usual collective coordinate quantization no longer plays the magical role and actually becomes quite obscure. Callan and Klebanov made a mistake of a factor 5/2 in their Wess-Zumino term and so when this is corrected, all the good things they obained go into drain. So far nobody has remedied this sad situation, but we all feel that when the dust settles, the "wrong" results of Callan and Klebanov will turn out to be the "right" ones.

The upshot of all these is that it is unclear what role the Wess-Zumino term must play in the Callan-Klebanov scheme. One thing is certain, however. If the Callan-Klebanov scheme is close to reality, then as argued by Nowak and his coworkers, the hyperons must look *bigger* than non-strange baryons. This is because the baryon density for strangeness s (B_s) has an additional term δB_s due to the kaon field (adding to the non-strange baryon density $B_0(r)$) in the form

$$B_s(r) = B_0(r)+\delta B_s(r)$$

and $\delta B_s(r) \geqslant 0$. That the strange baryons have bigger baryon sizes than the non-strange ones does not appear to be supported (at least in the crude interpretation so far given) by proton-baryon scattering data, but makes Gerry Brown and his colleagues at StonyBrook happy since it seems consistent with the magnetic moments of hyperons and the spectra of excited hyperons. All these are, however, open questions to be settled once we understand the strangeness better.

There is an interesting phenomenon associated with the large H_{SB}, which was not discussed in this meeting, but is thought to be connected to this mysterious feature of the strangeness. It is the strangeness condensation in dense nuclear matter discussed recently by Kaplan and Nelson at Harvard and Gerry Brown, Kubodera et al at Stony Brook.

The idea is simple and goes as follows. If the pion-nucleon sigma term is as large as $\Sigma^{\pi N} \simeq 57$MeV (presently quoted values are 60±10MeV) indicated by recent analyses, then as discussed by Donoghue and Nappi, there must be a considerable amount of strange quark condensates in the proton and hence the KN sigma term can be very big, $\Sigma^{KN} \simeq 10\Sigma^{\pi N}$. This means that there is a source of considerable attraction that can be gained by "clearing" condensates from the vacuum by, say, increasing nuclear density. In fact a nuclear matter, when compressed enough, could be found in a phase different from that of a nuclear matter, provided that kaons condense into the system. The critical density is roughly found to be

$$\rho_c \simeq F_K^2 m_K^2/\Sigma^{KN} \simeq 3\rho_0$$

where $\rho_0 \simeq 0.17\ \mathrm{fm}^{-3}$ is the ordinary nuclear matter density. A kaon condensation would lead to an S/B ratio of $\sim O(1)$ (where S is the strangeness and B is the baryon number) and hence it would not occur in relativistic heavy-ion collisions since there is not enough time to violate strangeness. But it could occur in neutron stars; Gerry Brown told me that he and Hans Bethe are studying how the kaon condensation could affect the cooling of neutron stars.

It is obvious that we understand very little of strangeness when viewed from the Skyrmion side. Does this suggest an interesting physics? Many people think so and there is big experimental effort to unravel the mysteries of strangeness. There are several proposals for kaon factories devoted to such questions. An urgent task for theorists is to understand whether and how strangeness differs from the other light-quark flavors.

The last few years have witnessed a great success of mathematics in unifying diverse phenomena into one universal structure. Topology and differential geometry became a common language between particle physicists (e.g. superstrings), condensed matter physicists and now nuclear physicists. Most of us understood little of them before 1984; now we are all struggling with mathematics text books on them. We heard from Niemi and Balachaudran how to describe anomalies (e.g. the Wess-Zumino term) in terms of Berry's phase. We learned in particular that our favorite Skyrmion is nothing but a "magnetic monopole" in the parameter space, with the Wess-Zumino term being the non-trivial adiabatic phase; that topology is generated in the parameter space by degeneracies. This is amazing and beautiful; and the Berry phase is universal: it can be understood as a topological classification of respective (induced) "gauge" bundles, e.g., gauge bundles over spheres.

It appears physically in the quantum Hall effect, the optical activity of a helical optical fiber, molecular dynamics, the sequence of states in the Jahn-Teller effect, gauge field theories etc. Experimentalists go into laboratories, do measurements and confirm the phase directly, in all other fields, it seems, than in strong interactions. One wonders: is there any hadronic observable that relates directly to a Berry's phase? We know, of course, how it figures crucially in the quantization of a Skyrmion. We also know that there are various tests of the Wess-Zumino term. But we have so far nothing equivalent to what one finds in other areas of physics. I think we should look for it.

I do not know whether one can make a precise interpretation in terms of a Berry's phase, but there is one recent development in nuclear physics which is closely linked to the presence of the

Wess-Zumino term in the Skyrmion action. It has to do with the resolution by Nyman and Riska of a long-standing difficulty in understanding the exchange-current contributions to isoscalar electromagnetic form factors of nuclei. I will return to this matter later.

Let me now play a role of a devil's advocate. (I could have done this at the beginning but I postponed it till now so as to postpone provoking anger among some of you).

Why are you here? What is the goal of this meeting? What is the ultimate goal of working on Skyrmions, chiral bags and their variations?

Some will say: "for the sake of curiosity". After all, the Skyrmions are curious objects. However I fear that as some already do they will most certainly drift off into all kinds of esoteric stuff having nothing to do with Nature. Some will say: "this is a warm-up excercise for superstrings". And you know many former Skyrmion enthusiasts are now doing superstrings.

Some will seriously say: "it is to test QCD, particularly at low energy". In fact, this sounds most respectable and many people use it to get the contract money. (And some experimental colleagues are more impressed by it than anything else). I think this is a nonsense. If you want to test QCD at low energy, I suggest to turn to lattice calculations. When a lattice calculation someday gives the proton magnetic moment to the experimental accuracy, QCD will have been "confirmed" magnificiently at low energy. But testing QCD? Here s. Weinberg says much more forcibly than I can: "an analogy may be useful between quantum chromodynamics and another science that I think it will increasingly come to resemble, hydrodynamics. In both cases we think we know the underlying equation: the Navier-Stokes equations in hydrodynamics, the SU(3) Yang-Mills equations for quantum chromodynamics. In both cases fascinating, important hard problems have not been solved. In hydrodynamics there are problems involving flow at high Reynolds numbers-phenomena such as turbulence and chaos. In quantum chromodynamics there is everything having to do with low energies and long distances: glueballs, confinement, phase transitions. Turbulence is a fascinating subject, and will go on interesting physicists for many years, but we do not study turbulence to test the Navier-Stokes equations. In the same way I think that all the really interesting problems of quantum chromodynamics have nothing to do with testing quantum chromodynamics. Critical tests of quantum chromodynamics will come at high energy; my guess is that the critical tests will be found most beautifully in high-energy annihilation of electron-positron pairs into jets, where we will be able to avoid the "interesting" aspects of quantum chromodynamics".

And I agree with him. So why do Skyrmions with all the "song and danse" about anomalies, painful Casimir effects, Cheshire Cats and hedgehogs and what other animals? To get 20% accuracy in static properties? Or reach 10% eventually? Big deal. As crude a quark model as Glashow's in 1979 Schwinger Festschrift can do just as well and in fact some sophisticated quark potential models can do much better with less work. So why are you wasting your time solving Skyrmion equations or manufacturing another model with another name? Did someone say "Quo vadis?" Sorry, I cannot answer for him. But I can answer for myself why I am excited about this meeting and about the things I heard here. I claim that it is in nuclear physics that the concept of Skyrmions and anomalies will make a major impact. To highlight what I mean, let me tell you how we, nuclear physicists, came upon this business.

By 1978, QCD was accepted by the majority of nuclear physicists (not to mention particle physicists) to be the one and only theory of the strong interactions and it was concluded that nuclear physics should be re-examined, if not completely re-derived, in terms of quarks and gluons. This sounded exciting, because that meant that nuclear physics was a wide-open new field, starting with throwing away the old Yukawa theory of nuclear force and re-expressing everything in terms of QCD variables. This fashion, however, brought about the "size crisis". In a qualitative way, QCD has a length scale of ~ 1 fm and so the nucleon would have a quark-core "radius" ~ 1 fm. This size can be arrived at by various different ways; in particular, the popular MIT bag model required that the nucleon bag have the radius $R \gtrsim 1$ fm. But this is simply too large in nuclei. Naively, this size would imply that the nucleons in, say ^{208}Pb, are close packed. How does shell model work at all? One can think of all kinds of ingenious mechanisms to squeeze out of this dilemma, but as Maris and his coworkers discussed, it would be truly difficult to understand single-particle properties in heavy nuclei if R were as large as ~ 1 fm. Some people thought of extremely clever mechanisms which (they hoped) could "derive" shell model from quark-gluon dynamics, but so far none has succeeded and none shows any promise. Short of miracles, it looks out of reach, even though nobody would contest that ^{208}Pb could eventually be described in terms of QCD (just as nobody doubts that a crystal could be derived from QED; it's just hard).

The "Little Bag" was created in 1979 to resolve this size crisis. The bag is small with $R \lesssim \frac{1}{2}$ fm, surrounded by a pion cloud, in conformity with spontaneously broken chiral symmetry. This automatically the resolved size problem; it also accounted for all the processes involving pion emission and absorption, which made up the bulk of nuclear physics. In the original version, however, the pion field was considered to be purely fluctuating but soon after it was realized that for nucleon structure, the mean-field pion played a key role. To account for this, the hedgehog configuration was brought in and when this was done, two effects were discovered: first for small R, the energy of the system went proportional to F_π^2 and hence linear in N_c, so according to Witten's 1979 argument, it behaved like a soliton or a "magnetic monopole". The second observation was that at the magic chiral angle $\theta = \pi/2$ for which the bag radius is $\sim \frac{1}{2}$ fm, only half of the baryon charge was found inside the bag. (This was reached at by an argument borrowed from Jackiw and Rebbi's seminal paper on fermion number fractionization). One other observation, very disturbing at that time, was that the "little bag" system with the hedgehog field outside collapsed. This took place around 1980, quite a few years before Skyrme was rediscovered.

In subsequent years, many of the puzzles in the little bag were quickly resolved. Vento realized that coupling the ω field to the bag stabilized the bag-hedgehog system. (After Skyrme was rediscovered (in 1983), the Skyrme quartic term was used to stabilize the system, but as we heard from Kunz and others, it is really the ω field that seems to be responsible for the stability of the soliton). The missing baryon charge was subsequently recovered in the pion cloud, now known as Skyrmion. This was first shown for the chiral angle $\theta = \pi/2$, but later extended by Goldstone and Jaffe to an arbitrary angle. The mechanism of "leaking" baryon charge (leaking out of the bag into the pion cloud) was then identified to be due to a chiral anomaly, a subject that became fashionable in all other fields of physics. Jackson and collaborators, and Goldstone and Jaffe, and Mulders and others simultaneously discussed the possibility of shrinking the bag "adiabatically" all the way to a point; this led to the birth of the Cheshire Cat idea. And that brings us up to today. As you can see, the rediscovery of Skyrme came just at a crucial moment, but isn't it a real shame that SKyrme's work was not known to nuclear physicists before Balachandran, Pak, Tze, Witten and others rediscovered it for other purposes?

Now let me come to my punch line which would have been my talk here if I had not been asked to "summarize" the conference. I claim that the recent development on Skyrmions and anomalies *does* and *will* provide long-awaited answers on some fundamental questions in nuclear physics. I have yet no fully satisfactory explanation of the phenomena, but I am certain that we are on the way to it. This is what I would call exciting problems in QCD which, however, have very little to do with "testing" of it. It is a subject of the intriguing ways QCD manifests itself at low energy though it will never "show" us what QCD really is.

One of the most intriguing questions in nuclear physics is: when does the conventional nuclear physics picture break down and when do the degrees of freedom of quarks and gluons have to be explicitly invoked? I mentioned early on some of the things that have occurred in this connection, but what happens in electron scattering from nuclei is even more surprising and intriguing.

Consider electron scattering from the simplest nucleus, the deuteron,

$$e + d \longrightarrow e' + n + p,$$

$$e + d \longrightarrow e + d.$$

The first process, known as electrodisintegration of the deuteron, involves an *isovector* electromagnetic (e.m.) current (dominated by magnetic dipole), and the second -elastic scattering of the deuteron- involves an *isoscalar* e.m. current, so the two processes probe the response of the nucleus to different components of the e.m. current. The two nucleons undergo strong interactions; therefore as the virtual photon probes the system, it will see not only the two individual nucleons, but also other degrees of freedom virtually excited at short internucleon distances. Thus the total current of the system (responding to the photon) J^a_μ (μ = Lorentz index, a = isospin index, 1,2,3 for isovector, 0 for isoscalar) is of the form

$$J^a_\mu = \sum_i J^a_\mu(i) + \sum_{i<j} \Delta J^a_\mu(i,j)$$

where $J_\mu(i)$ is a one-body current, and $\Delta J_\mu(i,j)$ a two-body current. Suitably defined, the former is referred to as an "impulse current", the latter as an "exchange current".

It has been a subject of intense research since Yukawa's theory of nuclear force was first proposed to "see" experimentally how the meson exchanges, responsible for nuclear force, contribute to ΔJ_μ. Incredibly enough, it was only a few years ago that the isovector exchange current was unambiguously confirmed and only last year that the isoscalar exchange current was given a reliable theoretical treatment and confirmed experimentally.

Naively one would think that ΔJ_μ would be hopelessly complicated, involving many meson exchanges and lots of nucleon excitations (such as Δ, N^* etc). To our great surprise, however, it turned out to be *extremely* simple: $\Delta J^{a=1,2,3}_\mu$ is entirely (!) given by the current algebra soft pions and ΔJ^0_μ is entirely (!) given by the Noether current coming from the Wess-Zumino term. Put differently, the two nucleons in interaction "filter off" *all* other degrees of freedom than *soft pions* in response to the isovector e.m. current and "filter off" all current algebra pions, letting through only those degrees of freedom associated with the anomalous Wess-Zumino action, in response to the isoscalar e.m. current. Such a possibility was discussed some years ago in connection with various intriguing phenomena in nuclei such as the effective g_A in nuclear matter quenched to $g^*_A \simeq 1$ etc and given the name "the chiral filter hypothesis". This hypothesis seems now more relevant than thought before.

The results in Fig.1 illustrate my points. In Fig.1a is plotted the differential cross section of the deuteron electrodisintegration for small energy transfer (ω) vs. momentum transfer q. The theoretical curves are from J.F. Mathiot: "N" corresponds to taking into account only one-body current, "soft pions" to a full current including, however, only soft-pion exchange current in ΔJ_μ (this is essentially what Riska and his co-workers calculated in 1973, a true *prediction*) and "full" to a full current including pion exchange, vector meson exchange and appropriate form factors in ΔJ_μ. The data points are from the Saclay experiment of 1985. The soft-pion prediction is remarkably well supported by the experiment and by the full calculation, to large momentum transfers. Is this just a freak accident in the deuteron?

Fig.1b says it is not. In Fig.1b is given the magnetic form factor of ^{3}He (more complicated than the deuteron); the soft-pion calculation is from Peter Sauer, who has also done a "full" calculation which gives results close to that of "soft pions". Again it is only the soft pions that are "seen" by the isovector magnetic probe.

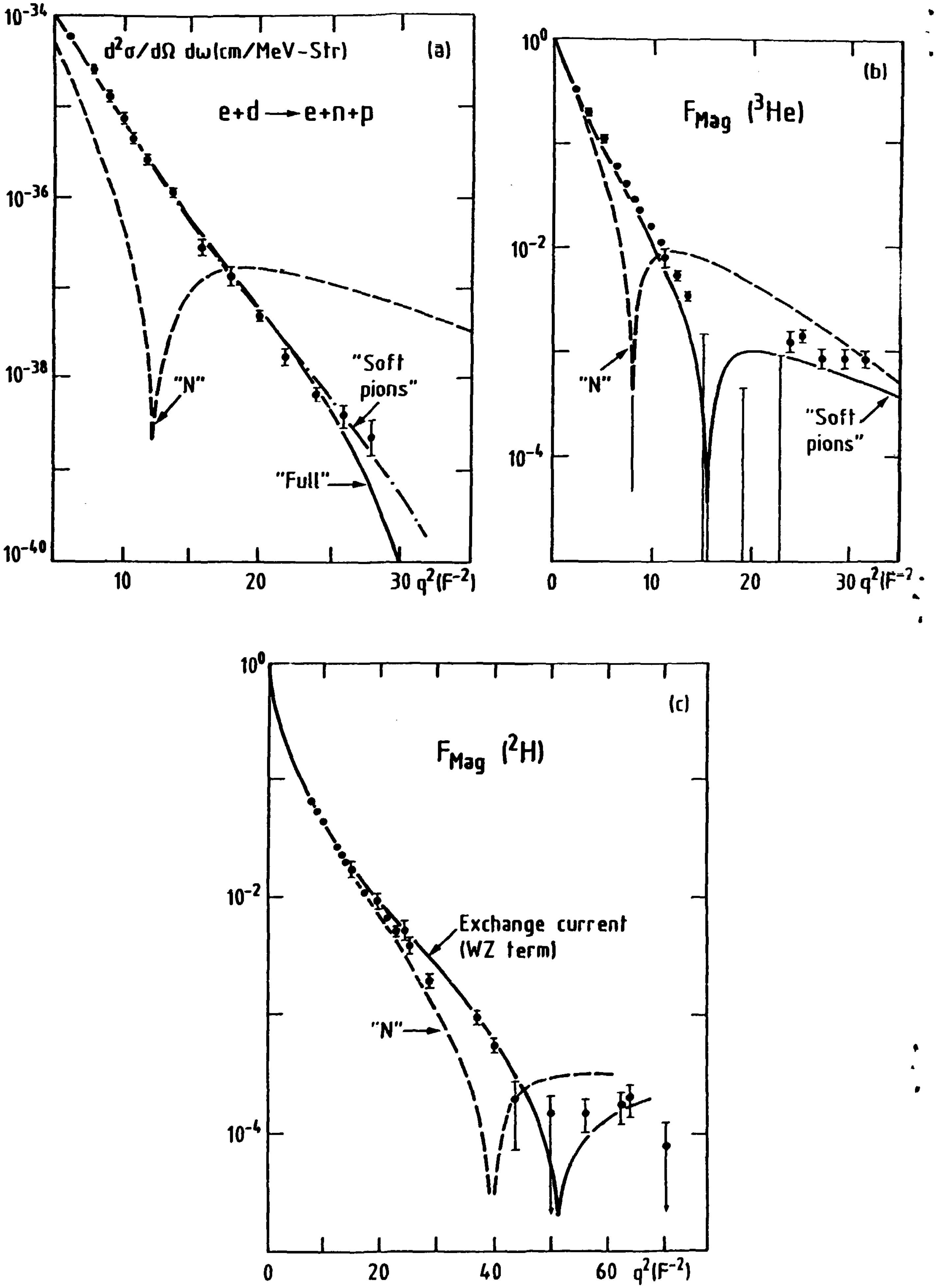
Some of us argued as early as 1970 that current algebra soft-pion theorems should be relevant for meson-exchange currents but the arguments were made for very low momentum transfers. It now seems that current algebra works to much larger momentum transfers, say $q^2 \sim 30\ \mathrm{fm}^{-2}$. Why the simple mechanisms work at such momentum transfers must have a simple and elegant explanation!

Since soft pions were invisible to an isoscalar photon, the "chiral filter hypothesis" went on to say, the isoscalar exchange current would be swamped with many hard pions, massive vector mesons and all the horrible things happening at short distance that we don't know how to handle. In short, we had no theory to work with until the Wess-Zumino term appeared. Last year, Nyman and Riska showed that the crucial information on the isoscalar exchange current is entirely encoded in the Wess-Zumino action. They derived, using the product ansatz for the Skyrmion field U for $B = 2$, the appropriate exchange current and predicted what would be expected in the magnetic form factors of the deuteron for $q^2 > 40\ \mathrm{fm}^{-2}$. The results are shown in Fig.1c. At the time of the calculation, the data were available only for $q^2 < 40\ \mathrm{fm}^{-2}$; the Stanford data for $q^2 > 40\ \mathrm{fm}^{-2}$ came *after* the prediction. Again the simple theory works remarkably well to such a large momentum transfer as $q^2 \sim 60\ \mathrm{fm}^{-2}$, in perfect accordance with the "chiral filter hypothesis". (A caveat: as Verbarschot discussed today, there may be some problem in the product ansatz, particularly at short distances. On the other hand, in (1+1) dimensions, the product ansatz seems to be accurate over all distances. Furthermore the applicability of the product ansatz may depend upon the channels one looks at. This is an important issue that has to be resolved, before one reaches any firm conclusion on the marvelous result of Nyman and Riska). A truly fascinating possibility, if the Nyman-Riska result is viable, is that the topological effect associated with the Wess-Zumino term or maybe with a Berry's phase may have been seen in nuclear physics.

But where are the quarks and gluons?

These are only a few examples of some remarkable happenings in nuclear systems that call for a simple and convincing explanation. I think that Skyrmions and anomalies are a good starting point towards a resolution.

Let me conclude this "summary" by thanking all the organizers of this conference for the enormous effort they have made to make the meeting exciting and for all the hospitality shown to us to make our stay enjoyable. For myself, let me add a particularly personal note. I carry a passport which unfortunately does not open all the doors of the world and this part of the world had been closed to me until I came here. This meeting is thus an important first event for me, for which I must sincerely thank the organizers -and of course the Skyrmions. As someone from the audience said, mine is a "boundary" problem and a "Cheshire Cat principle" is making the boundary obsolete!


10^-34
10^-36
10^-38
10^-40
d²σ/dΩ dω(cm/MeV-Str)
(a)
e+d → e+n+p
"N"
"Soft pions"
"Full"
10
20
30 q²(F⁻²)
10^0
10^-2
10^-4
(b)
F_Mag (³He)
"N"
"Soft pions"
0
10
20
30 q²(F⁻²)
10^0
10^-2
10^-4
(c)
F_Mag (²H)
Exchange current (WZ term)
"N"
0
20
40
60 q²(F⁻²)