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UNRESOLVED THEORETICAL PROBLEMS

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Perhaps as much as any recent conference on nuclear physics this Telluride conference has brought together a number of very important ideas in nuclear physics and has pointed to future directions of the whole field. In this summary I want to give some personal impressions of the ideas about which we heard and also to try to put them into a perspective within which we can look at unresolved theoretical problems - questions which could have importance in the future. We have had the kind of vigorous discussions which are appropriate to a frontier town like Telluride. There were some moments of glory and some heinous crimes. I want to provide, as appropriate, some quick accolades and some summary frontier justice. The first such quick and harsh justice should probably be dispensed to the previous summary speaker who has, in his usual style, stolen most of the prize nuggets unearthed at this conference.

It has been the history of nuclear physics that quantum jumps forward have occurred when the right experiments and the right ideas came forward at the right time. Usually the right time came not as the result of any vigorous theoretical prediction but rather when some experimental evidence - for example, some enormous resonance behaviour - hit us on the head at a time when a climate of ideas existed in terms of which the resonance behaviour could be understood. Before looking at the Gamow-Teller resonance and the climate of ideas in which it has flourished let's take a brief look at earlier similar revolutions of our field.

For the nuclear shell model individual physicists made predictions from the mid 1930's onward. However, it wasn't until about 1950 that the resonance and the climate of ideas quite suddenly came together. The resonances, of course, were the nuclear size resonance

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The first clearly seen in the Wisconsin neutron cross sections. The ideas pertained to spin-orbit coupling but even more to the beginning of the phenomenological understanding of the shell model for systems of nucleons and its basis in the appropriate many-body theory.

Next, in the late 1950's, came the understanding of nuclear collective motion in terms of individual nucleons in single-particle orbits with a residual interaction. This breakthrough was heralded as much as anything by the data for giant dipole resonances observed in photonuclear reactions. The climate of ideas arose from the experience of handling interacting nucleons and nucleon holes in single-particle configurations. The ideas for coherent single-particle motion in the giant dipole state came from David Brink and Gerry Brown and have led to two decades of very great progress on collective motion of nuclear many-body systems.

Cluster degrees of freedom came into focus when, in 1960, heavy ion accelerators first achieved high resolution and the strong molecular resonances of the ^{12}C and ^{16}O ions were discovered. The climate of ideas again arose from the shell model. Sorting out true cluster correlations from these correlations imposed on the nuclear system by Pauli principle and by attractive residual interactions among the nucleons of a single shell has taken a long time.

In the mid 1960's isobaric analogic resonances were a somewhat startling experimental discovery which led to a much deeper understanding of nuclear symmetry laws and how little they were violated by Coulomb forces.

In each of these cases there were some prior theoretical speculations. In each there were beautiful experiments perceived with a large element of surprise in spite of the earlier speculations. In each there was a climate of ideas within which the significance of the results was quite quickly comprehended. In each there were, suddenly, many questions and unresolved problems - one knew where one was and what had to be done. In each case there was an important conference when the experiments and the ideas came together. All of these conditions appear to be present now about the experiments pertaining to the Gamow-Teller resonance and the perspective of ideas within which it exists. It is my opinion that this conference will be judged to have been the important one for this conjunction.

I want to summarize the nuclear physics ideas which form the perspective within which the spin excitations in nuclei should be considered and to give some indications of the directions in which we must head, as elucidated in the many interesting review talks and discussions which we have heard.

$$\sigma_{pn}(\theta_n = 0^\circ, T_p \approx 200 \text{ MeV})$$

Fig. 1. A qualitative description of the collective GT resonance observed in σ_{pn} at 0° . The primary interest here is neither in the level energy, E_{GT} , nor in its width, Γ_{GT} , but rather in the wings of the resonance.

Let me begin by recognizing some of the individuals who predicted some of the phenomena which we have discussed but whose ideas came before the time was ripe - before the Telluride epoch. In the early 1960's Anderson, Wong and McClure anticipated the existence of the collective Gamow-Teller resonance and correctly predicted its energy. Also Fujita and his colleagues in Japan discussed the collective Gamow-Teller resonance.

A little later Migdal foresaw the basic ideas of pion condensates although he did not have the insights provided by QCD. Much credit should be given to Magda Ericson and her collaborators (echoed by the Paris group - fortunately for Rho and his collaborators echoing is not a crime in the San Juan mountains of Telluride) for predicting in the 1970's the quenching ideas related to the role of delta-hole interactions. Some early experimental work at low energy discovered the Gamow-Teller resonance. However, the full experimental exploration required higher energies and came about, four years ago, in the beautiful Indiana work of Goodman and his collaborators.

Let's use the Indiana experiment to epitomize the physics ideas confronted here at Telluride. I have sketched in Fig. 1 the idealized (p,n) double differential cross section (at 0° and as a function of excitation energy in the residual nucleus), for an incident proton energy of about 200 MeV. We know that for such proton

energies the cross section at 0° is dominated by the magnetic isovector spin-flip process. The Gamow-Teller collective giant resonance is shown (any fine structure is assumed to be averaged over) and we enquire about its properties.

Generally the most important properties of any resonance are its energy and its width. With regard to these the Gamow-Teller resonance is garden variety. We have heard at this conference about some very fine and sophisticated calculations of the energy and width, but frankly, in 1982, there is no reason to be enormously excited about these. The resonance position is not far from the place immediately above the isobaric analogue resonance where Anderson, Wong and McClure predicted it to be in 1962. There are some modest contributions to the energy shift arising from current ideas.

Let me spend a little more time on the width of the GTR which is, as I said, also garden variety and not nearly as startling as those for the resonances on which our previous revolutions in nuclear physics were based.

The width of any giant resonance is the sum of the natural width of the state and its spreading or damping width. In the situations to which I shall refer - including the GTR resonance - it is the spreading width, $\Gamma^\dagger$, which dominates. The spreading width was discussed at this conference by Wambach and by Bortignon, and it pertains to the way in which a residual interaction distributes the simple wave function of a model Hamiltonian among the eigenstates of the full Hamiltonian (or among the eigenstates of a doorway Hamiltonian). The amount of the spreading is, phenomenologically, equal to twice the imaginary part of the optical potential, $\Gamma^\dagger = 2\langle W \rangle$, of the model state or, at a deeper level,

$$\Gamma^\dagger \approx \sqrt{M_2} = 2 \sqrt{\langle v_{res}^2 \rangle}$$

where M_2 is the second moment.

If, for example, one considers the possible single-particle motion of nucleons and if one takes the residual interaction, V_{res} , to be the sum of all the two-body interactions with neighbouring nucleons, it was already known before 1940 that one obtained an answer of about 40 MeV for $\Gamma^\dagger$. This came simply from the strength (depth ≈ 40 MeV, range ≈ 1 fm) of the nucleon-nucleon interaction needed to bind the deuteron. Because this spreading width of 40 MeV was roughly equal to the spacing of single-particle levels in a potential well of nuclear size, it seemed impossible to have a viable shell model. This was, perhaps, the strongest reason why no reasonable physicist entertained seriously the thought of a shell model until overwhelming experimental evidence established it. Why were

the theorists wrong? Why did the spreading width - as measured by the low energy neutron and proton optical potential ($W \approx 4$ MeV, $V \approx 8$ MeV) - turn out to be so much smaller? The answer lay in the correlations imposed on the system of nucleons by the Pauli principle and by short-range repulsion.

The narrow width of the giant dipole resonance (about 4 MeV in carbon) was not as much of a surprise since one already understood the comparable widths of nuclear single-particle levels.

The situation for molecular levels of heavy ion systems was, however, totally astonishing. In $^{12}\text{C}+^{12}\text{C}$ these states - probably alpha cluster doorway states - have widths of about 100 keV. Since doorway states are spread by the same strong nuclear interaction as single particle and collective giant resonance, there was no reason why they should be much narrower - let alone two orders of magnitude narrower. Two decades later we have no real understanding of the long lifetime of molecular levels, even though a whole industry has flourished to elucidate their spectroscopy. Spreading width problems are difficult and have been notoriously neglected, but surely some very deep information about nuclear structure resides in understanding the physics of the various factors which conspire to produce such narrow doorway states.

The isobaric analogue resonances presented a similar surprise. The Coulomb force in heavy nuclei is, on the average, only three times smaller than the nuclear force so the widths were expected to be about three times smaller than those of nucleon size resonances - or about 3 MeV. No one anticipated the 50 keV widths found experimentally. The spreading of such states by Coulomb forces into states of lower isotopic spin is reasonably understood in terms of the long range of the Coulomb interaction.

For a Gamow-Teller resonance we would have expected spreading widths similar to those for other states (nucleon size-resonances, giant dipole resonance, etc.) of about 5 MeV or so and that is what is found. So we have no enormous excitement caused by the width but some good physics. At first sight it might seem surprising that such collective particle-hole states should be narrower than the square root of the sum of the squares of the spreading widths of the particle and hole states on which they are based. Recently George Bertsch and his collaborators have studied the spreading of simple low energy nuclear excitations into doorway states made up of surface vibrations. Bortignon reported on how the moderate narrowing of the GTR was accomplished by this mechanism. Similar results were achieved by Wambach et al. with beautiful RPA calculations including coupling to Δ -hole states. However, this is not where the current revolution lies.

The drama resides in the area under the resonance and in the shape of its wings. It is interesting that the question of line shape has been totally unimportant in all of the earlier nuclear revolutions to which I referred, and even the question of area under the resonance - generally quantified in terms of sum rules - has been of only moderate interest. Here, because of the remarkable spin and isospin selectivity of the (p,n) reaction at intermediate energies one has been able to probe the resonance shape over a reasonably wide energy interval - and one has been able to quantify the area under the resonance accurately by normalization to established β -decays. Both the quenching of the GTR area and the shape of its wings involve exciting physics. It is that excitement which has brought us together. Let's look at the ideas which are the source of this excitement.

About half the strength has gone from the peak. Where it goes tells us something immediately about the nucleon. If the missing strength corresponds to the dissipation of the simple excitation into other shell model configurations - such as $2p-2h$ or $4p-4h$ - then it should be found in the "near wings" of the resonance. If it involves short distances and very high energy states - such as the Δ -hole excitations lying 300 MeV higher - then one might expect the missing strength to go into the "far wings". Osterfeld's analysis as reported at this conference shows that microscopic particle-hole calculations may give some strong energy dependence to the non-collective background in the vicinity of the Gamow-Teller resonance so that some care must be taken in interpreting the "near wings". Even allowing for this there appears to be a real quenching effect - a substantial missing portion of the collective resonance.

Extracting tangible physics from analysis of line shapes is not easy. We would not have been drawn to the main questions of our Telluride conference if the recent development of nuclear and particle physics had not led us there. Quantum chromodynamics (QCD) should matter for atomic nuclei. There are compelling reasons why QCD effects might show up in the GT resonance. We want it to be there.

It is the compelling conjunction of particle physics (QCD) with nuclear physics which constitutes the climate of ideas within which the GT resonance is discussed and which may possibly herald another revolutionary change in our thinking about nuclear physics. Almost everything that transpired at the conference can be put in terms of a series of questions, few of which have any solid answers at present. It is the fact that so many questions have suddenly presented themselves with such urgency that should make us optimistic for the next decade of nuclear physics.

We are looking for ways in which QCD effects can augment our standard single-particle picture of atomic nuclei which has proved

so fertile for three decades. In the single-particle picture, Fig. 2(a), protons and neutrons in single-particle orbits interact through meson exchange ($\pi, \rho, \dots$) with the presence of meson-exchange currents (MEC). Although so much has been accomplished phenomenologically with this model we are only now learning how to properly handle systems of mixed fermions, to treat their mean fields, residual interactions, etc. while Pauli is probably smiling benignly at our efforts to handle the effects of his exclusion principle on neutrons and protons with path integrals and Grassman algebras.

In the QCD nucleus, Fig. 2(b), each nucleon is a colour-neutral bag of quarks embedded in a medium. Goldstone bosons (pions) exist in the medium but not inside the bags. Efforts are made to conserve chiral symmetry at the bag surface which is the interface between the bag and the medium. The very fact that we talk about a bag - a sharp interface - highlights the fact that at present we know so little about the physics of confinement.

Although QCD tells us about quark interactions at small distance in a nucleus we must confront the little-known physics associated with confinement. The average internucleon spacings (~ 2 fm) are only little larger than the bag radius (~ 0.8 fm - ρ and Brown may, in the future, retreat from their smaller bag radius or face frontier justice). There are, then, immediately a host of questions, in addition to the size of the bag radius. Do the bags coalesce or

Fig. 2. (a) The familiar single-particle picture of the nucleus in which protons (large solid circles) and neutrons (open circles) interact through meson (small closed circles) exchange; (b) the QCD nucleus in very qualitative terms; nucleons are considered as colour-neutral bags of quarks with some of the quark bags overlapping.

percolate? What are the consequences of percolation? Is the medium in between the bags polarized? If so, how are the pions affected? What are the effects of bag deformation? Can one bring "real" deltas into the nucleus? Can one introduce ρ -mesons into the interactions? [Introducing ρ -mesons into effective interactions now appears to require more larceny than is acceptable in a frontier town. Possibly because of the fear of summary justice Gerry Brown did not appear in person in Telluride to give his talk on effective interactions which, instead, was given by another member of the Brown gang who was not seen subsequently.] At what densities or temperatures do phase transitions occur from normal nuclear matter to quark matter?

Although all of these questions were raised at the conference the real interest was, of course, the search for any nuclear properties which might possibly have some bearing on QCD effects. ρ gave an analysis of nuclear response functions some of which might act as filters in being especially sensitive to such effects. In his opening address Teller presented the simple picture why the magnetic isovector spin-flip transition should involve Δ -nucleon excitations. The stage was set then for considering the Gamow-Teller strength function and other detailed nuclear properties which might provide clues to the elusive quark degrees of freedom.

The present evidence for the effect of Δ -hole interactions or other manifestations of quark degrees of freedom is sufficiently oblique that it is clearly important to be sure how far the standard model without explicit quark effects can go in describing nuclear properties. At our conference the conservative approach - the role of honest broker - was presented by Arima. In the single-particle approach of the Tokyo school one uses truncated shell model spaces, effective operators and residual interactions (Hamada-Johnston) with strong tensor pieces. As Arima showed, this approach is generally very successful. Further, Towner and Khanna presented very explicit calculations for the various corrections - including a number of meson-exchange current corrections - to isovector magnetic moments. Still the conservative approach raises many questions. How realistic is the residual interaction? (Is one, in effect, augmenting the realistic interaction to mimic some aspects of the Δ -hole interaction, etc.?) Alternatively, as Weise asked, can the Δ -hole interaction be incorporated into a phenomenological interaction? Can one calculate reasonably all of the many corrections to magnetic moments, transition rates, etc.? Does one avoid double-counting problems (for renormalized MI and GT operators)? These questions about the standard model are not necessarily new but they have now gained a new urgency.

Focusing, as our conference did, on the Δ -hole interaction as an example of nuclear QCD effects there are a number of questions which remain open. Is the Δ -hole interaction justified by QCD? Is

the coupling constant, g' , the same for the Δ as for the nucleon? Is the Δ -hole interaction reduced by exchange? Is there a good experimental signature for the Δ -hole interaction which distinguish its effects from those of the N-hole and the tensor force? (Here possible candidates are the quenching in low and high spin states, magnetic moment corrections, etc.)

Turning to the quenching itself - the physical phenomenon which was the central interest of our Telluride conference - there remain a host of open problems. How well do we really understand the Δ -hole interaction? Considering the background subtraction problems, what is the magnitude of the quenching of the Gamow-Teller resonance? (The answer for the amount of strength, relative to the sum rule limit, found under the resonance and its near wings appeared to increase as the conference progressed from about 33% to well over 50%.) What does QCD (Adler-Weissburger) say about the sum rule? What is the energy dependence and total angular momentum dependence of quenching? Has any GT strength been found (or should any such strength be expected to manifest itself) at 300 MeV, for example in the very new ($^3\text{He}, t$) work?

In turn, the GT-quenching and Δ -hole interaction problems give rise to questions about real Δ 's in the nucleus which were addressed by Lenz and others. How good is the Δ -dominance picture for the pion-nucleus optical potential? How does the propagating Δ interact with the nucleus? What is its mean free path? Does the $^{12}\text{C}(\pi, \pi')^{12}\text{C}^*$ (15.11 MeV) excitation function tell us something about real Δ 's in the final state? (Probably not!) What is the Δ -N interaction? What reactions might inform us about real Δ 's in the nucleus? (One candidate might be recoilless Δ -production in (p,d) reactions.)

Perhaps one of the most important results of Telluride is the impetus it has given to the general study of nuclear response functions with a variety of probes. The response function has a large number of different components varying in their multipolarity and isospin. Unscrambling the cross section data with various probes to obtain the individual components or strength functions is not a trivial task. Walker, Love, Heisenberg and others showed how it might be done and at what energies it is possible and desirable. Clearly one of the most important tasks for nuclear physics in the next decade is to unravel the individual strength functions from the inelastic scattering and reactions of electrons, nucleons and pions at intermediate energies.

In the analysis of nuclear strength functions a number of important questions remain. What is the difference between the longitudinal and transverse particle-hole interaction and their effect on the corresponding nuclear response function? How good a testing ground are the stretched states, especially considering the importance of initial and final state correlations?

Although the whole GT business began with the study of (p,n) reactions - at zero degrees and rather low energy - there is clearly now a need to explore (p,p'). Is this a very rich field? How good is DWIA at intermediate energies? [In particular, what is the validity of the one-step assumption and of a local t-matrix at energies as low as 150 MeV?] Can one sort out the longitudinal modes at large q? Is there a window of visibility for spin-excitations?

Similar questions face inelastic pion field which is just emerging with important data from the EPICS spectrometer of LANL. Here one can begin with the questions raised above. Is the π -A (or the Δ -A) interaction known well enough? Can one look at T=1 final states? Do inelastic excitation functions identify $\Delta S=1$? Is the angular distribution sensitive to spin transfer? How does one sort out convection current contributions?

It is undoubtedly the long list of questions which is the lasting legacy of Telluride. The GT quenching effect is, at best, an oblique and somewhat illusory manifestation of QCD effects in nuclei, but the phenomenon itself survived the searching analysis of the conference. Accepting the evidence then the open problems point to the direction in which nuclear physics will now turn.