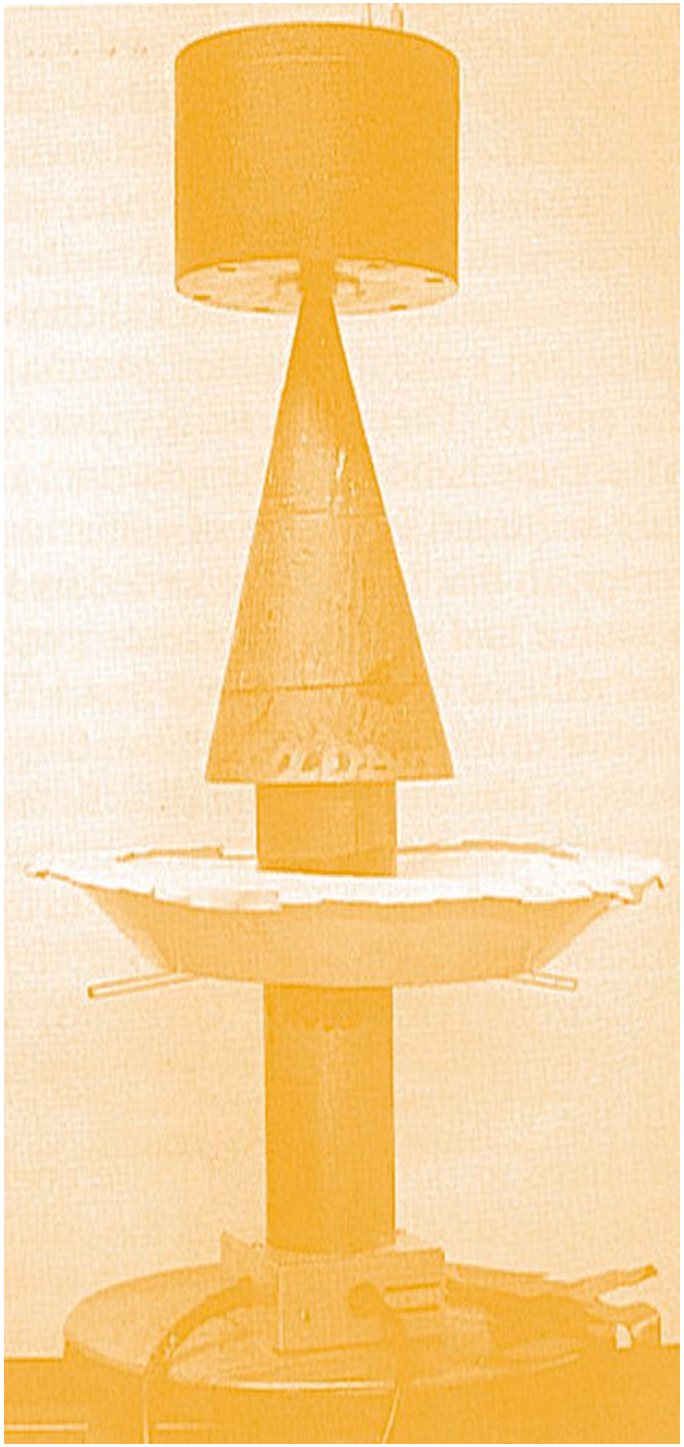

# Closing Remarks

Maurice Goldhaber

Neutrino Helicity at 50

BNL, May 2, 2008

Many things had to come together to make the proposal for the experiment that Dr. Grodzins described this morning. My student, Andy Sunyar and I, found that isomers systematically appear near closed shells of the single particle model of Maria Mayer and Jensen which pleased her.

PHYSICAL REVIEW VOLUME 83, NUMBER 5 SEPTEMBER 1, 1951

# Classification of Nuclear Isomers*†

M. GOLDHABER AND A. W. SUNYAR
*Brookhaven National Laboratory, Upton, New York*
(Received May 25, 1951)

The lifetime-energy relations of Axel and Dancoff and the $K/L$ ratios calculated by Hebb and Nelson are shown to yield spin differences $\Delta I$ which are one unit too high for long-lived electric transitions ($\Delta I \geq 3$). These transitions are also slower than expected from Weisskopf's one-particle model and have approximately the same lifetime as magnetic transitions of equal $\Delta I$. The lifetimes of magnetic transitions agree approximately with Weisskopf's formula. If the statistical weight of the initial state is introduced into the lifetime formula, the "scatter" of the square of the matrix elements is greatly reduced for these transitions. Most long-lived isomers show $M4$ transitions, in agreement with shell theory. Some isomeric transitions which were previously assumed to show no parity change are now interpreted as $E3$. Their occurrence in the $1g_{9/2}$ shell may be explained by assuming that for the configurations $(g_{9/2})^{3,5 \text{ or } 7}$, $7/2+$ and $g_{9/2}$ states are comparable in energy. The $7/2+$ state is lower in more than half of the cases. Empirical curves of $K/L$ ratios plotted against $Z^2/E$ are given. They are consistently lower than the existing theoretical curves based on nonrelativistic calculations of internal conversion coefficients. Spins of metastable and ground states are assigned for a number of nuclei. For even-even nuclei the following rule is found: the first excited state usually has spin 2 and even parity. The only mixed transitions found are $M1+E2$. Sufficiently many $E3$ transitions are established to permit the conclusion that electric transitions are slower for odd-neutron nuclei than for odd-proton nuclei. This gives strong support to a one-particle model. Among electric transitions only some $E2$ transitions are faster than expected on the one-particle model. This is interpreted as a cooperative phenomenon, related to the existence of large quadrupole moments.

Professor Havens (Columbia) asked me to supervise two students, Ruby and Rustad, sent by him and Professor Wu to use the fast neutrons of our graphite Reactor to produce $^6He$ from $^6Li$ to decide the nature of the β-interaction. I said if I supervise their students I have to be in charge which he refused and I said, then I am only a hole in the reactor, thank you!

About a year later Ruby and Rustad published their results, concluding that the interaction was scalar.

At about the same time Professor Allen and colleagues (U of Illinois) found in a different experiment the β-interaction to be Vector.

At that time the Am. Phys. Society still held a Christmas Meeting and Professor Leonard Schiff, the chairman of Physics at Stanford who was in charge of the meeting asked me to give an invited paper on "The present status of β-decay."

I stayed home on Friday morning to think of ways of deciding between S and V. People have asked me how I thought of the experiment on neutrino helicity. I said it is very simple:
You work for 20 years on isomers and think 20 minutes!

At one point I asked myself: How do we know that a proton does not decay? I estimated a lower limit for it's lifetime by using the fact that spontaneous fission was in Thorium; I estimated that decay of a nucleon (neutron or proton) would have looked like spontaneous fission, thus getting a limit for the proton or bound neutron lifetime of about $10^{21}$ years. I then convinced Reines and Cowan that a scintilation counter they had built at Los Alamos to study cosmic neutrinos, could also be used to study proton decay, and after an estimate of the cosmic ray background we obtained an approximate lower limit, about 10 times larger than my previous estimate.

I had given a talk at Michigan, discussing possible p decay, and as you have heard Jack van der Velde decided to look for p decay and hired Larry Sulak, whom I had promised that I would join him. After Reines joined Irvine our group became IMB.

The long lifetime of the proton showed the need for very massive detectors, for which water Cherenkov counters seemed the most practical and we built one at a salt mine near Cleveland.

One day somebody asked me: who at Brookhaven works on IMB? I modestly pointed at myself and said: I'm B!

# C.N. Yang's Comments

- Maurice recalls C. N. Yang commented that the neutrino helicity experiment was a 'devilishly clever' experiment.
- Maurice's recollection was indeed confirmed by C. N. Yang and his comment was recorded in his book, "Selected Papers, 1945-1980, With Commentary, p. 44"

> of the Feynman–Gell-Mann–Sudarshan–Marshak theory. However, this idea was rejected by Professor C. N. Yang on the ground that the $He^6$ recoil experiment is one of the more reliable experiments. C. N. Yang (private communication).[2]

He remembered correctly. I did communicate orally to him my conclusion when he visited Brookhaven that summer.

Although the proposal of $V$ and $A$ coupling was theoretically very attractive, especially the conserved vector-current idea of Feynman and Gell-Mann, experimental confirmation was necessary since the mistake in the $He^6$ experiment had not yet been found. In a devilishly ingenious experiment, M. Goldhaber, L. Grodzins, and A. W. Sunyar resolved this question by measuring the helicity of the neutrino, which they found to be $-1$. That was conclusive evidence of the $V$ and $A$ coupling, and a new chapter in our knowledge of $\beta$ decay began. This experiment was finished in early December, 1957, and I remember Goldhaber calling me about the result a couple of days before Chih Li and I took off for Stockholm.

The Institute had invited Lee to visit for the academic year 1957–1958. He and his family moved to Princeton in the fall of 1957 and settled in the new housing project. Our collaboration was now more convenient, and we launched into an investigation that resulted in [[58a]]. It was an elegant paper, useful for experimentally measuring the spins of hyperons.

[2] J. J. Sakurai, *Il Nuovo Cimento* 7, 649 (1958).

---

[[58d]]

Article begins page 252

## Low-Temperature Behavior of a Dilute Bose System of Hard Spheres. I. Equilibrium Properties

*The Physical Review* 112, 1419 (1958)

*T. D. Lee and C. N. Yang*

*Commentary* This paper and its sequel, [59a], are follow-ups of [[57i]]. The same method is used as in the earlier paper. Now considering incomplete zero-momentum-state occupation, we treated the degenerate phase of a dilute Bose system of hard spheres at temperatures greater than zero degrees Kelvin.

As explained in the Commentary on [[57h]], [[57i]], and [[57q]], Lee and I had spent a lot of time on the many-body problem in the fall of 1956. There were many fruitful directions to be followed, and the formulation of the binary collision expansion method was still to be written up, when news about C. S. Wu's experiment came in late 1956. We immediately turned our full attention to elementary particles, and that state of affairs lasted about one year. Later, when we could again afford to spend

Although the proposal of $V$ and $A$ coupling was theoretically very attractive, especially the conserved vector-current idea of Feynman and Gell-Mann, experimental confirmation was necessary since the mistake in the $He^6$ experiment had not yet been found. In a devilishly ingenious experiment, M. Goldhaber, L. Grodzins, and A. W. Sunyar resolved this question by measuring the helicity of the neutrino, which they found to be $-1$. That was conclusive evidence of the $V$ and $A$ coupling, and a new chapter in our knowledge of $\beta$ decay began. This experiment was finished in early December, 1957, and I remember Goldhaber calling me about the result a couple of days before Chih Li and I took off for Stockholm.

– C. N. Yang
Selected Papers, 1945–1980, With Commentary, p. 44

極 ← Extremely

妙 ← Ingenious / subtle

的，意想不到的 ← Unimaginable

實驗。 ← Experiment

prepared by c. k. jung for Maurice Goldhaber