Working (And Sparring) with Luis: Some Personal Recollections

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I would like to express my appreciation to the Forum on the History of Physics (FHP) for arranging this session to honor the memory and career of Luis Alvarez, one of the most remarkable experimental physicists of the 20th century. It is particularly meaningful for those of us who had the opportunity of working with him, and a privilege for me to be one of the speakers.

First, as a caveat-emptor, let me offer the following observation: In today's session, my fellow speakers, Rich Muller and Art Rosenfeld, and I overlapped as members of the Alvarez bubble chamber group at Berkeley, which would probably lead to some overlap in our accounts and could provide a "Rashomon-like" experience, following the Japanese tale of several witnesses to the same event who then recounted very different versions of what they observed. Nonetheless, while our memories of that era and our interactions with Luie may differ somewhat, on the whole they are quite consistent, if not identical.

Luie's career was especially noteworthy not only because of the importance and sheer number of his innovations and discoveries, but also by the unusually wide spectrum of topics covered, from cosmic rays to nuclear physics to particle physics, with a significant detour into radar work during world war II, resulting in his development of the Ground Controlled Approach (GCA) system which had a major impact in aviation and for which he won the Collier Trophy. Impressive as all this was, he then followed with his last great work on the extinction of the dinosaurs. In his discussions with members of the group he often prided himself on persistently having changed research careers every few years.

He had an uncanny intuition about physics and technology, coupled with an insatiable curiosity about the world around him. He is justly renowned as a member of the Inventors Hall of Fame for his myriad inventions and as a Nobel Laureate in physics for his contributions to particle physics through his development of the hydrogen bubble chamber technique, leading to the discovery of a large number of resonance states, described by Art Rosenfeld

earlier. Parenthetically, I should also add that our session chairman, Stan Wojcicki, played a seminal role in those early discoveries while a graduate student in the group. However, it was Luie's wide-ranging curiosity, as Rich Muller indicated earlier, which led him to one of his finest achievements, while working with his son Walter -- developing the asteroid impact theory as the explanation of the extinction of the dinosaurs.

So what was it like working with Luie? The short, and incomplete, answer is: very stimulating, challenging and sometimes frustrating.

Because Luie was who he was, the longer answer embedded in my recollections of him also embrace three themes, namely, the role of the individual scientist in this era of big science and team research, the sometimes skeptical reaction to breakthrough ideas introduced by scientists outside the specialty affected – the so-called "not invented here" syndrome, and lastly, the challenge to provide flexible support to scientists if they should have excellent curiosity-driven ideas outside their specialty.

When I was a graduate student in high energy physics at Berkeley, starting in the late 1950s, I spent a few months in the Alvarez bubble chamber group at the Radiation (or "Rad") Lab, as it was known then, before switching to the Moyer-Helmholz group to do experiments using fast-electronic counter techniques. Among my fellow graduate students at the time were Stan Wojcicki in the Alvarez group and Barry Barish, the present APS President, in the latter group. After I got my Ph.D. I drifted back immediately to bubble chamber physics, first with Philippe Eberhard at the College de France in Paris for a year and then at the University of Illinois with Bob Hulsizer and his group who were collaborating with the Alvarez group. After two years of commuting between Urbana and Berkeley, I was invited by Luie to join his group at the Lab as a research physicist. I arrived in the Fall of 1965 and stayed for the next 40 years, including a stint as leader of the group a few years after Luie stepped down as group leader and following Art Rosenfeld in that capacity. I don't know about Art's experience but as group leader I interacted with Luie almost on a daily basis, sometimes several times a day, which made for a stimulating, if sometimes challenging, experience. But more about that later.

The period from the mid- 1950s through the 1960s represented the heyday of the bubble chamber program developed by Luie and the group and so it was a most exciting time to be there. The coming together of the technological marvels of

the various bubble chambers his team designed and built, culminating in the 72-inch chamber, and the simultaneous development of the analysis systems, resulted in a veritable explosion of new particles and resonances discovered, starting with the Y^*_1 from the 15-inch chamber data by a team including our chair today, Stan Wojcicki, when he was still a graduate student. It seemed like not a week went by before yet another discovery was made.

At that time, the group was the largest high energy physics group in the world. So, some of the concerns about joining the group (amplified many fold by social scientists of the day, lamenting the impact of big science) were the possible anonymity of the individual scientist and the dangers of conformity of thinking within large groups. Luie aggressively laid those concerns to rest. He was an ardent foe of group-think which he characterized as "intellectual phase-lock". For that matter he was just as much against intellectual phase-lock from individuals as from groups, by regularly challenging our assumptions and pronouncements. This was also his way of learning. To that end, he interacted regularly with individuals in the group, from grad students and postdocs to senior physicists, to keep abreast of what we were doing, while providing the additional benefit of sharpening our thinking (sometimes painfully) and challenging any sense of anonymity. Another factor was structural. Because there were so many fundamental topics and issues to be addressed, we naturally divided into small subgroups to tackle different but complementary research problems, with the result that we did not feel as anonymous cogs in a bigger wheel. Finally, one of the most important factors which contributed to the feeling that we were an intimate research group and also provided the most stimulating learning experience for me, was the institution of the weekly Monday Night Seminar at Luie's home. This was a memorable seminar series, made especially so by Luie's role. Because of its impact on many of us I would like to describe it briefly.

The ground rules were simple: The meetings were every Monday night, except for Christmas and (all?) holidays that fell on that day of the week; Luie and Jan, his wife, provided the refreshments of coffee, cookies, pretzels and beer; the first 15 minutes were devoted to newsbriefs (ie science news and gossip from around the world) diligently prepared during the week and usually delivered by a postdoc or graduate student in the group; this was then followed by about an hour or so of presentation by the seminar speaker. The audience was encouraged to interrupt with questions or comments at any time, subject to Luie's intervention if it got out of hand, which was ironic since he was often the instigator. The two most

important rules that Luie insisted on were: (1), do not spill drinks on the carpet, and (2), the name of the speaker must never be allowed to be known in advance. The latter rule was stringently enforced by Luie as a matter of principle because he didn't want this to be a popularity contest in general, and to ensure the usual full audience for the graduate student speakers. He also felt, that the stimulation from just the give-and-take discussion among the audience would make it worthwhile, regardless, and he was definitely right in that assessment.

Luie sat in his arm chair in the front row, always in the same place, to the side of the speaker, perhaps as a conscious courtesy to avoid being in the speaker's direct line of vision. The speakers included grad students and physicists from the group, as well as from other groups at the lab, and visiting scientists. While the topics were mostly related to high energy physics then, there were also many talks on subjects far removed. The seminars and the attendant discussions, often instigated by Luie's questions, were usually spirited and almost always very stimulating. We looked forward to these meetings, even after being sleep-deprived from taking shifts at the Bevatron.

Visitors included theorists such as Richard Dalitz, Edward Teller and Freeman Dyson. Charles Wohl reminded me that Luie introduced Dyson as the scientist who talked himself out of a Nobel Prize by recommending someone else.

Typical of seminars on topics remote from particle physics was the spell-binding talk on the first successful human-powered aircraft flight across the English Channel, by the leader of the team, Paul MacCready, who described the evolution of the project from conception to the flight of the Gossamer Albatross, with a cyclist pedaling to drive a propeller. Another riveting speaker was the designer of the first Hewlett-Packard pocket calculator, the HP-35, who described the history and design of the device. Since Luie was on the board of directors of HP at the time, he suggested this person as a worthwhile speaker for the seminar organizers to pursue and he spoke to us soon after the product came to market, as I recall . Unfortunately, I don't remember his name. Yet another seminar which I found intriguing was the one given by Gerry O'Neill from Princeton who was a high energy physicist but had shifted his interest to space science. He had recently created a private company called Geostar and was planning to launch a small communication satellite to test a position-determination system he was developing to track aircraft. As O'Neill was describing his Geostar activities, Luie had what I would characterize as a "Eureka moment". Interrupting the speaker, he turned to the group and exclaimed something to the effect, " This idea is

potentially a license to print money!", and then went on to speculate how this could be extended well beyond aircraft to commercial fleets of cars, buses or trucks, outfitted with transponders where the owners could eventually track the locations of their vehicles to within at least a few hundred yards, possibly better. He ended by saying that he thought that depending on the technology development and price, such transponders could also eventually be used to track private vehicles and even people, with greater accuracy. Talk about prescience! And now years later we have GPS, albeit without Geostar which subsequently went bankrupt. Anyway, despite the excitement of that moment, I don't know if anyone rushed out to invest. Others among this varied list of speakers were the likes of the eminent Egyptologist, Ahmed Fakhry from the University of Cairo and Marian Diamond, the neuroscientist from the Berkeley campus who described some of her research on the brain and according to Rich Muller, illustrated some points by dissecting a brain as part of the presentation.

The spirit and stimulation of these seminars was largely influenced by Luie's curiosity to learn more and his encouragement to the rest of us in the audience to speak out with comments or questions to clarify points of discussion, if necessary. To illustrate, during one seminar where the speaker continued with a theory-jargon-filled exposition despite polite requests to reduce the jargon usage, Luie lost patience and barked to the audience, "are you going to let him get away with it?" While not the most polite of interventions, the talk improved considerably from that moment on. He was more considerate of the graduate student speakers but not to the point of giving them a free pass on their presentations. After the fact, most of the students I talked to felt the experience to be of "significant benefit". I believe they were sincere in their assessment. Finally, as a measure of Luie's impact on these seminars, one should note that after he eventually stopped hosting them, others valiantly tried to continue the series, keeping to the same format, but interest petered out soon after, primarily because of the missing Luie factor.

So, with regard to concerns about working in a big research group, Luie strongly supported the role of the individual and vigorously confronted group-think conformity. On the contrary, I derived much benefit from the stimulation and vitality of diverse views of my colleagues which also provided a nurturing environment for new, unorthodox ideas.

As an another example of Luie's wide-ranging and spontaneous curiosity was his sudden interest in the Egyptian pyramids of Giza after he saw them for the first

time on a plane flight from Africa to a high energy physics conference in Geneva in 1962. He marveled at their sheer size and wondered how they were built , especially 4,500 years ago! The two largest pyramids, Cheops (the largest of the two by a little) and Chephren, were approximately 145 meters high and 230 meters on a side. To get a feeling of how remarkable these structures are, one need only consider they were the tallest man-made structures until the Washington Monument, at 169 meters, was completed in 1884 but with it having only about 1% of their bulk. This was followed 5 years later by the Eiffel Tower at a height of 324 meters.

After some initial thinking about this subject at the time, Luie then set it aside because of the press of other matters, but came back to it a couple of years later. As he read more of the literature of the pyramids, he became intrigued by the fact that of the three large pyramids, the one by the father Sneferu, had two chambers, while his son Cheops had three, but that of the grandson, Chephren had none. Despite reading that the conventional wisdom among modern archaeologists was that this was due to a change in tradition emphasizing simplicity by the time of Chephren, Luie felt this belied human nature which would suggest that the grandson would have tried to outdo his father and grandfather and build at least four chambers, regardless. Parenthically, one may be tempted to speculate on the origins of Luie's views of human nature—but I won't.

Another fact which stimulated Luie's interest was that all the chambers had been discovered only thousands of years after the pyramids were built, which suggested that the pyramid architects took great efforts to hide the existence of these chambers, possibly from future grave robbers, and that perhaps the architects of Chephren's pyramid were just more successful in hiding his chambers, filled with great treasures yet to be discovered.

So, the challenge was how to find especially well-hidden chambers in such a truly massive structure. One could dig tunnels forever, while destroying much of the pyramid along the way, and still miss the chambers. Here then is where Luie came up with the idea that was ingenious in its simplicity, namely, to use highly penetrating cosmic-ray muons to "X-Ray" the pyramid. (Perhaps "Mu-Ray "would be a more precise term). The principle is similar. By measuring the flux of muons penetrating the pyramid with great precision as a function of azimuth and zenith angles over a large sensitive area, one might notice the existence of a hidden chamber by the local increase in flux due to the absence of mass from the

chamber void. Two factors facilitated the use of this technique: one is the existence of a large chamber (called the Belzoni chamber after Giovanni Belzoni who first explored it in 1818) at the base of the Chephren pyramid near the center to provide a good point of detection and second, was a recent invention in high energy physics particle detectors of spark chambers with digital read-out, which when used in conjunction with scintillation counters as triggers for the spark chambers, created an effective muon cosmic ray telescope to measure the arrival angle of the muons.

After deciding on the general idea, Luie then organized the project quickly in his inimitable fashion. He made contact with Prof. Fathy El Bedewi, head of the physics department at Ein Shams University in Cairo and the aforementioned Egyptologist, Ahmed Fakhry, and with them set up a U.S.- United Arab Republic collaboration. At the same time, he organized a team from the Alvarez group to design and build the equipment and develop the data-collection and analysis systems, with collaboration from scientists of Ein Shams. The team leader was Jerry Anderson who had been a graduate student in the group, and the analysis effort was headed by Gerry Lynch, one of outstanding analysis experts in the group. Luie then got support from the Atomic Energy Commission, as well as some contributions from private sources such as Hewlett-Packard, IBM and the National Geographic Society.

The apparatus was shipped to Cairo in early 1967 and the experiment was ready for first operation on June 5, one day before the outbreak of the six-day war, which led to a cessation of the operation as well as to some interesting adventures for the crew. The project was reactivated by the Ein Shams group a few months later, with some American participation a few months after that. First results were reported by Luie at the Spring 1969 APS meeting, indicating that there were no chambers in the 19% of the pyramid they had probed. In subsequent runs, with the equipment tilted and rotated to different positions, they were able to scan the rest of the pyramid with similar null results. No chambers, no treasures. The pyramid was solid throughout. Nonetheless, a remarkable effort. Afterwards, Luie often commented in response to statements that he didn't find a chamber, saying, " It wasn't that we didn't find a chamber. We found that there wasn't any chamber."

I would now like to offer some personal observations about Luie during some events already discussed by Rich and Art. Some of my most vivid images of him were from the period during his work on the mass-extinction theory. My office

was close to his so that I could see what he was doing. I remember him sitting at his desk, a solitary figure surrounded by pictures of his physics heroes, and pounding away, hour after hour, week after week, on his tiny Hewlett-Packard pocket calculator. I don't remember the precise model but it could have been the HP-65 by then. He was trying to calculate the effect of a 10-km asteroid striking the earth with enough energy to vaporize itself and many times its weight of rock. As a guide, he used the written accounts of the 1883 Krakatoa volcano eruption and the resulting migration of atmospheric dust particles around the world. What was amazing to me in retrospect was that this one person with a pocket calculator was anticipating the studies of a "nuclear winter" caused by the detonation of many nuclear bombs, done by large teams of scientists using state-of-the-art powerful computers. Admittedly, they came up with much more accurate detailed models but the gross features were similar!

Some of the initial reaction to the mass-extinction hypothesis, together with those from some of Luie's earlier cross-over initiatives, illustrate an issue, which rises from time to time, namely, the additional skepticism towards important ideas when proposed by scientists outside the specialty affected. I guess I am more sensitized to the issue since I became a program officer or petit fonctionaire in Washington, first at DOE and now at NSF. While he was very careful in his recounting to some of us of his experiences interacting with scientists whose specialties embraced dinosaur studies, I got the impression that some of the resistance to the Berkeley extinction theory stemmed from their suspicion that he wasn't expert enough to be credible in this area. Let me emphasize that this is my interpretation of Luie's accounts, as he tended to be circumspect on this topic. Nonetheless, when challenged along these lines, Luie often responded by saying that at least the Berkeley theory made specific predictions that could be tested. While the implication was clear I thought the reply was quite diplomatic . Other possible examples of the "not invented here" syndrome that I recall were, initial reactions to the group's first foray into development of adaptive optics with their "rubber mirror" or adjustable mirror concept for optical telescopes, and to particle-physics expatriate Jerry Nelson's attempts to promote his concept of segmented parabolic mirrors which resulted in the Keck Telescope.

Where Luie could be less than diplomatic-- to say the least-- was occasionally while he was competing for resources support for his bubble chamber program and also when he met resistance from Radiation Lab management as he eventually tried to shift his interest from accelerator-based particle physics to

space-based studies of high energy particle interactions and cosmic rays, particularly looking for anti-matter in primary cosmic rays. (This was ~40 years before the imminent launch of the Endeavor shuttle, carrying Sam Ting's AMS spectrometer to install in the Space Station to do the same thing). As he tried to get support for his proposed balloon experiment called HAPPE (for High Altitude Particle Physics Experiment) I remember him being frustrated by the argument that this wasn't part of the mission of the lab or of the AEC agency, at the time. His response, delivered undiplomatically at times, was that it should be! In the end he prevailed and this eventually led to an outstanding and diverse astrophysics program at the lab, with the likes of former particle physicists Rich Muller, and George Smoot, an eventual joint winner of the Nobel prize for the anisotropy of the cosmic microwave background radiation, as well as the team which co-discovered the acceleration of the expansion of the universe, considered an indication of dark energy. In any case, apart from his own experience, Luie was generally concerned about tendencies of intellectual and fiscal inertia in the scientific community and the funding agencies, respectively, even with their peer-review panels, which could inhibit the incubation of new cross-disciplinary ideas and programs. Needless to say, this is an ongoing challenge.

My regular one-on-one interactions with Luie were always stimulating and often challenging, especially when he disagreed with one of my decisions. I remember one instance where he was critical of my decision declining to shift support to a new effort being proposed. I responded by saying that I found his criticism ironic since years before he had actively discouraged members of his group when they proposed a brief experiment using Wilson Powell's propane bubble chamber at the Bevatron. He replied that his action was guided not by principle, such as being against new ideas, nor the merits of the proposal but rather by time-dependent strategic needs of mobilizing the full resources of the group towards exploiting the development of the hydrogen bubble chamber program. To that, I said it sounded reasonable and I would use a similar explanation to justify my decision in the issue at hand. He laughed and the argument ended on an amicable note.

To summarize what it was like to work with Luie, the following true anecdote may say it best. I often had to go back to the lab after hours in the evening and on the weekends to briefly check on some activity, and on many such occasions I would bring my two young sons with me in the car. Because it was after regular lab hours, I had to stop at the entrance gate and give my employee badge to the

guard for him to approve entry. I never thought anything of all this, until several years later I learned from my sons that they thought I was paying the guard each time to let me in to work! I was stunned and delighted, because even though they got the facts wrong they did get the sentiment correctly. I would have paid to go to work in that environment and I think that says it all. (Fortunately I didn't have to).

To conclude, I would like to quote from Richard Feynman's book,'What do you care what other people think?', which describes his experience on the Rogers Commission investigating the Space Shuttle Challenger disaster. I believe it captures an essential component of Luie. In this excerpt, Feynman is ruminating the night before whether to go through with the O-ring in ice-water experiment during the actual hearing the following day:

"I think, 'I could do this tomorrow while we're all sittin' around, listening to this [Richard] Cook crap we heard today. We always get ice water in those meetings; that's something I can do to save time.' Then I think, 'No, that would be gauche.' But then I think of Luis Alvarez, the physicist. He's a guy I admire for his gutsiness and sense of humor, and I think, if Alvarez was on this commission, he would do it, and that's good enough for me." The rest as they say is history.

Thank you for your attention.