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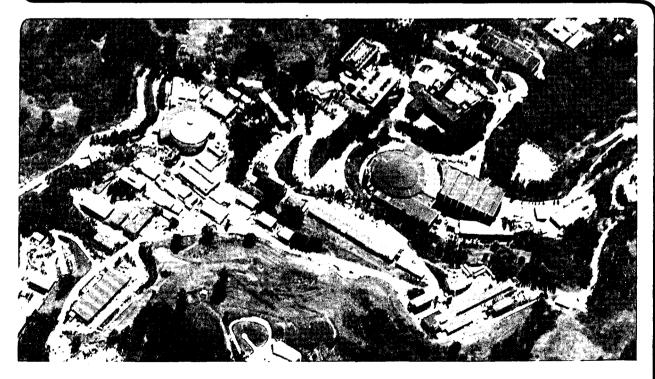Particle Physics, Fermilab, Batavia, IL,
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Luis Alvarez

Text of a talk given at the Fermilab Conference

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I have been out of high-energy physics for some 20 years and to get myself back into the mood of a particle physicist, I would like to quote some recent remarks by Carlo Rubbia. "Detectors are really the way to express yourself. To say somehow what you have in your guts. In the case of painters, it's painting. In the case of sculptors, it's sculpture. In the case of experimental physicists, it's detectors. The detector is the image of the guy who designed it." I've never heard it expressed so well, but I'd like to add that particle physics has always been done by a triad of equally important professionals — accelerator builders, experimental physicists, and theoretical physicists. I'll have some comments at the end on how I hope the members of the triad will interact in the future. I have been a member of the first two categories, but never of the third.

My ten years in the bubble chamber trenches (discussed also in Peter Galison's chapter in this volume), the most exciting period in my life, started at the 1953 Washington meeting of the APS, when I met Donald Glaser. He showed me his first cosmic-ray tracks in a tiny bubble chamber (1 cm $\times$ 2 cm), filled with ether. I had been unsuccessfully racking my brains to find an appropriate detector for the Bevatron, which was about to turn on. It was immediately clear to me that Don's chamber filled the bill exactly — if it could be made to work with liquid hydrogen, and if it would operate in large enough sizes. I wanted one big enough to see the production and decay of the strange particles that had first been observed in cosmic rays by George Rochester and Clifford Butler, in a Wilson cloud chamber (see chapter by Rochester in this volume), and had recently been seen by Ralph Shutt's group at the Brookhaven

Cosmotron, in a hydrogen diffusion cloud chamber (see chapters by William Fowler and William Chinowsky in this volume). The properties of these chambers were well suited to the discovery of the particles, and of their production mode, respectively, but not for systematic studies of their properties. I should add also that on the theoretical side, Abraham Pais had predicted the phenomenon of associated production, and Murray Gell-Mann had invented the strangeness rules that tied the few available experimental facts together, and that predicted many of the reactions we would later observe, a good fraction of them for the first time (see chapters by Pais and Gell-Mann in this volume).

As soon as I returned to Berkeley, my colleagues, Lynn Stevenson and Frank Crawford, started to repeat Glaser's experiments, with the explicit aim of seeing tracks in liquid hydrogen. They fired up two technicians in the synchrotron shop, where all were working, and these two men, John Wood and A.J. (Pete) Schwemin, collaborated in building the first hydrogen chamber to show tracks. Wood sent his letter to the Editor of *Physical Review*, with pictures of his first tracks. (Actually, I ghost-wrote the letter, since John had never published anything before.) John's pictures showed an unexpected effect that was the key to the successful operation of large bubble chambers. One could see bubbles forming at the glass walls, while sharp tracks were forming in the central region. This was contrary to Glaser's feeling that bubble chambers had to have such clean, smooth walls, that bubbles wouldn't form there, but only on the tracks. As soon as I pointed out the importance of John's discovery, Schwemin, together with Doug Parmentier, started to build a 2-inch diameter metal chamber with gasketted glass windows — the first purposely "dirty bubble chamber." They had it working very quickly, and at Schwemin's request, I ghost-wrote their article for the *Review of Scientific Instruments*. Schwemin and Parmentier then built a 4-inch diameter chamber, which was the first bubble chamber of any kind to be fitted with a magnetic field, and which saw its first V-particles in a short exposure to a negative pion beam at the Bevatron.

We now felt we were on the right track, and enlisted the help of Dick Blumberg, a mechanical engineer, to design a 10-inch diameter hydrogen chamber, to fit in the well of a

wonderful magnet that Wilson Powell very kindly let us use. Wilson had two nearly identical magnets for the beautiful cloud chambers he built and used, and he simply let us have one on an indefinite loan. The 10-inch chamber was the first of ours to be "designed;" the previous ones had been fashioned on a lathe by Pete, who would say to himself, "The flange should be about this wide, and it should have a groove about here, to take a solder wire gasket that I'll make to fit it."

We spent a lot of time becoming familiar with Gell-Mann's strangeness rules, and I decided (after all, a group leader has to do something) that we would do our first experiment with stopping K⁻ mesons in hydrogen. From the theoretical and experimental standpoints, it appeared to be a potential gold mine, and from the sociological standpoint it was also a real winner.

Everyone else waited in line for high energy negative pions, kaons, or anti-protons, that came out of the one useful straight section of the Bevatron. But we were able to use a "private" target that could be flipped up in a curved section of the Bevatron, and that sent its sharply curved low-momentum pions and kaons between the outside iron return yokes, and into a very crude "mass spectrometer."

This separator consisted of a thin absorber that subtracted away almost all the momentum of the kaons, and much less of that of the pions. The cloud chamber magnet then bent the negative kaons into the active volume of hydrogen, where a reasonable fraction of them came to rest. No one had ever before seen K⁻ particles stopping in hydrogen, so we had the pleasure of seeing the copious production of all the hyperons with strangeness equal to -1: Λ, Σ^+, Σ^0, Σ^-. We very accurately measured the masses and the lifetimes of all these particles. We saw Σ^- hyperons interact in the hydrogen. (Anyone who wants to experience the impact of this experiment on the particle physics community should read the enthusiastic summary in the *Supplement to Nuovo Cimento*, *2*, 1957, pp. 773-5, with 3 photographs.)

We were fortunate that the separation efficiency of our crude "K⁻ beam" system was so poor that it let in large numbers of negative pions, as well as some negative muons. That permitted us to be the first to see the now well-known "muon-catalyzed fusion reactions." We thought at first that we had discovered a new particle that decayed into a muon, but it was soon apparent that the negative muon was forming a tightly-bound p-d-μ molecular ion, in which the p and d quickly fused to make ^{3}He plus energy, which internally converted to eject the muon, so the process could be repeated. (We found two cases in which successive fusions were catalyzed by the same muon.)

We were surprised to see the reaction happen so very often in our "pure" hydrogen with only one part deuterium per 5000 ordinary hydrogen atoms. The answer was that the reduced mass effect had the proper sign to make muons captured on protons become neutron-like objects which quickly transfer muons to deuterons at their first collision. The μ-d system then quickly captured another proton to form the molecular ion. We thought for an exciting hour that we might have solved the energy problems of the world by going to very low temperatures, where conventional wisdom said one had to go to many millions of degrees. Although we were quickly disabused of that motion, a recent resurgence in interest in catalyzed fusion has centered on experiments with d-t fusion, and several groups have found surprisingly high yields of 14 MeV neutrons per negative muon. Steve Jones, at Los Alamos, finds an average number of catalyzed reactions of the order of 150 per muon. Since each reaction yields 17 MeV, the energy released is thus about 2.5 GeV, which is within a factor of about 10 of what it takes to produce a muon. So we were originally over-optimistic, but not by such a large factor as we've thought for the past 29 years.

Before the 10-inch chamber was operated, I became convinced that if we were ever to do the kind of strange-particle physics that liquid hydrogen chambers should permit, we'd need a very large chamber. My first guess was 50 in. by 20 in. by 20 in., but I soon realized that because of the magnetic field, the particles would fan out more in the horizontal direction than the vertical. So we could exchange some unneeded depth for extra length, and the chamber

became the now well-known 72-inch; later, when it was moved to Stanford, it became the 82-inch chamber.

We now needed a special appropriation from the AEC, and after thinking about it for a few hours, the commissioners voted us 2.5 million dollars of the 1955 variety, or 10 million 1985 dollars. It couldn't have taken them very long, since I first briefed them one morning, and that same evening at a cocktail party, John Von Neumann told me that he and his four other Commissioners had given my proposal their stamp of approval. They didn't bother to ask for any peer review — that dismal procedure hadn't yet been invented! (I've now heard that it had been invented earlier, but I'd never heard of it at that time.) I didn't use much of the time in my presentation to remind them that the largest operating liquid hydrogen bubble chamber anywhere in the world was our 4-inch device.

Ernest Lawrence, from whom I learned how to make such a large extrapolation, thought I was sticking my neck out a bit too far, and one of my greatest disappointments is that he died a few months before the 72-inch chamber showed its first tracks. But I did have the pleasure of giving him some escorted tours of the bubble chamber and its new building, as the construction proceeded. Someone asked me why it took longer to build the building than it did to design and make the bubble chamber operational. My reply was that people had been putting up buildings for thousands of years, so there were long shelves of regulations that had to be met, but bubble chambers were too new to be so encumbered. In fact, the laboratory's safety department, that one might have thought would get involved in such a potentially dangerous project, left us totally alone, as we did our own tests on hydrogen safety.

The 72-inch chamber was a major engineering effort, and we assembled a very strong design team, under the leadership of Paul Hernandez, and an equally strong operational group under the direction of my closest associate, J. Donald Gow. We decided to test several novel features of the 72-inch design in a smaller 15-inch chamber. One new feature was the single window design, which increased the safety and more importantly the strength of the magnetic

field. We went full speed ahead with the single window design, even though we didn't know how to illuminate the bubbles and photograph them through the same window, until shortly before the 15-inch chamber became operational. The 72-inch chamber worked for the first time on 24 March 1959, and it had a long and very useful life.

I'm pleased that we decided, early in our bubble chamber program, to share all our technical information with anyone interested in hydrogen chambers. As physicists, we thought of ourselves as competitors, trying to do the best experiments before our friends in other parts of the world could get around to them. But as engineers, we considered ourselves as "members of a club," and custodians of a lot of government-funded development work, so we sent copies of our voluminous unpublished "engineering notes" and "physics notes" to everyone else in the club. Very quickly, all of our potential physics competitors knew everything we did about how to build chambers and how to use our rapidly increasing volume of software, with which to analyze the bubble chamber pictures. The leaders in this important phase of our work were Frank Solmitz and Art Rosenfeld.

In my 1955 proposal to the AEC for money to build the big chamber, I pointed out that unless we could greatly increase our ability to analyze bubble chamber film — compared to cloud chamber film — the big chamber would simply be a very expensive toy, that would produce enough "interesting events" in a single day, to keep all of the world's cloud chamber experts busy for a year. Cloud chamber events usually were "solved" by reprojecting the two stereo views of each track onto tiltable and rotatable "space tables" until the two images coincided everywhere. Then the orientation of the tracks in space could be read from angular scales, and the curvature of the tracks could be measured, using sets of circles with varying diameters. It was very time consuming — one might solve two events per day, but it fitted the production rate in the low density gas. But in going to liquid density, plus very long path lengths, the event rate would rise by about three orders of magnitude.

I proposed that we use what later became known as Franckensteins (for their designer, Jack Franck), which would quickly measure the track coordinates on the film itself, in a semi-automatic track following mode. These coordinates would then be subjected to computer analysis, which would give us the coordinates in real space. That was a very successful program, involving several generations of more and more automatic devices, culminating in the Spiral Readers, of which we had two, each capable of measuring very nearly one million events per year. Our group's scanning and measuring department eventually employed about 100 people, most of whom were undergraduate students, working part time. From the earliest days, we always measured more events per year than any other group, and we (almost) always had the largest hydrogen bubble chamber from our first one, in 1953, until the Brookhaven 80-inch came on in 1964. (The one exception was a period of a few months, when Jack Steinberger's 12-inch chamber came on shortly before our 15-inch was operational.)

I'll now take off my detector designer's hat, and exchange it for my "user's" hat. The first experiment we did with the 15-inch chamber was designed as a test of the Gell-Mann-Nishijima strangeness rules, that predicted the existence of a neutral cascade particle, the Ξ°. We set out to measure the mass of a new neutral particle, the Ξ°, which should decay into two other neutral particles, the K° and the Λ. (Victor Weisskopf had recently given a humorous Rochester Conference banquet talk, the high point of which had him showing a perfectly blank cloud chamber picture, and claiming that it showed the existence of a new neutral particle decaying into two other neutral particles. So that was the unlikely task we set for ourselves.) We found one excellent event that let us measure the mass of the Ξ°, thereby proving it existed.

But the most important result of that first 15-inch bubble chamber exposure to a medium energy well-separated K⁻ beam was the discovery of the first three "strange resonances," that set off the "population explosion" of what were for a time called fundamental particles, but which couldn't hold that title for long, in view of their rapidly increasing number. We have all been brought up with Eddington's concept of an "experimental fish-net." A fisherman who throws out a net will catch no fish with dimensions less than that of his mesh. But our discovery of

the strange resonances violated Eddington's rule by an enormous factor. We had designed our chambers with a fish net to match the decay lengths of the strange particles, in the range from millimeters to perhaps 20 centimeters. But the most important "fish" we caught had decay lengths shorter by factors of about 10^{12} — just the factor by which the lifetimes of the strange particles had been increased over typical "nuclear times" to make physicists call their behavior "strange."

What made the discovery possible was of course our extensive set of computer software that came from the Solmitz-Rosenfeld collaboration and the many talented associates they had recruited to work with them. And it also took the perseverance of two dedicated graduate students, Stan Wojcicki and Bill Graziano. We all know that Jocelyn Bell discovered the pulsars for which her graduate advisor, Antony Hewish was subsequently honored. In the resonance business, Stan and Bill were my two Jocelyn Bells, and I'm pleased to acknowledge their discovery. Bill is now doing other things, but Stan is well known to all of you as the leader in the plan to build the SSC at the earliest possible moment. Stan and Bill accidentally discovered the now standard method of finding new particles by looking for bumps in invariant mass plots. (For many years, I thought of myself as a "professional bump-hunter," and I've found that that is still a pretty good job description, now that I'm working in geology and paleontology.)

The discoveries of the first three strange resonances were published by a group of seven of us, known collectively as (Margaret) Alston et al. The first one is now known as the Σ 1385, the second as the Λ 1405, and the third as the K^* 892. Bogdan Maglich soon found the ω meson in a 72-inch exposure, using the bump-hunting technique; Harold Ticho led a group at UCLA that used 72-inch film to find the Ξ 1530, and Aihud Pevsner led a group at Johns Hopkins that found the η meson in 72-inch film. By giving our precious film to other laboratories, we were following the example set by Lawrence. (The first of the four "missing elements," technetium, was discovered in Palermo, Sicily, by Emilio Segre and C. Perrier, in a molybdenum deflector strip that Lawrence sent them from the 28-inch cyclotron that had been

bombarded by 6 MeV deuterons for two years.) Bump-hunting soon became a popular activity, and physicists with access to other bubble chamber film reported the discovery of other resonances, or particles as they now are called; the most important of these was the ρ meson, discovered by William Walker, at Wisconsin.

Until the population explosion started in the 15-inch chamber, there was only one resonance known in particle physics, and that was Enrico Fermi's famous "3-3 resonance" in the pion-nucleon system. I went to all the Rochester Conferences in this period, and I never heard anyone call the 3-3 resonance a particle; it was always thought of simply as a resonance or bump in a production cross-section curve as a function of energy. But it was clear that the objects we found as bumps in mass plots were really particles; they stayed together long enough for other particles to recoil against them, and then they came apart in times of the order of 10^{-22} seconds, as one could measure from the energy widths of the bumps, using the uncertainty principle. It was soon apparent that the 3-3 resonance was the first of the "new particles," and Rosenfeld started a new cottage industry to keep everyone abreast of the best values of masses, lifetimes, spins, etc. of *all* the particles that gave our profession its name. If the proton had been found to decay, the lifetime range would now span 60 decades!

It wouldn't be fair to say that as soon as all the new particles were found, the theorists came into the picture, and explained their taxonomy — the theorists had various frameworks to codify the particles known before the population explosion started, most notably the "8-fold way" of Gell-Mann and Yuval Ne'eman. They extended these ideas to embrace the newly discovered very short-lived particles. Their most famous prediction was that the Ω^- hyperon should exist, with an accurately predicted mass. That prediction came from the equality of the mass spacings of the 3-3 resonance (now known as the Δ), the Σ 1385, and the Σ 1530. The Bevatron didn't have enough energy to make Ω^- hyperons, which was a big disappointment to my group; we had the right detector, but the wrong accelerator. So we had to wait a few years until the 80-inch chamber came into operation, when we sent our congratulations to the Brookhaven group. But we did have the satisfaction of knowing that the important equality in

the mass spacings came out of measurements made in our hydrogen chambers, plus, of course, Fermi and Anderson's old mass value for the Δ. So we could feel, in the language of the official baseball scorer, that we had "an assist" in the important discovery of the Ω^-; Gell-Mann used our data to tell the Brookhaven group "where to look" for the Ω^-.

I would like to close by distinguishing between two classes of discoveries. I have said that if I had been born a few hundred years ago, I would probably have been an explorer. My heroes in the world of exploration are James Cook, and Roald Amundsen (and of course his unlucky rival, Robert Scott). They made great geographical discoveries that are correctly acclaimed by everyone. But just think how different they were; Amundsen found the South Pole, whose existence could be questioned only by members of the flat earth society, while Cook found the Hawaiian Islands whose existence was a surprise to everyone. One can't decide which discovery was more praiseworthy; the point I am trying to make is that we need both kinds. I have described our satisfaction in finding the predicted Ξ^0, and in watching our friends at Brookhaven find the Ω^-. We all have enormous admiration for the discovery of the W's and the Z, which are in the same category, but much harder. But I wonder if in the future, anyone will be able to find something completely unexpected, such as the J/Ψ, or the strange resonances. As I look ahead in particle physics, I see support only for the enormously expensive detectors to find particles whose existence has been predicted by theorists. I think that is a very unhealthy situation, and I hope that those who are pressing for a quick construction of the SSC will turn some of their attention to this dilemma; if one is only allowed to look for things that are predicted, from earlier knowledge, both experimental and theoretical, how can we use our powerful new accelerator to find something *really* new, such as the examples I've just mentioned, both in geography and in particle physics?

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