What I (think I) learned from Alvarez, Feynman, Segré, Bob Wilson, and my dad.

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Abstract

The science in High Energy Physics has seldom been more interesting than now. I argue that while the long-term future may not be clear, that is at least partly due to the opportunities that we have in hand. We, and in particular graduate students, should make this a golden era.

1 Introduction

This talk is not for everyone. It is intended for the graduate students, and is based on what I've learned from them in many discussions both at Fermilab and at my university. It's meant to be informal, and to incite discussion and perhaps even some action.

2 Questions

Ask yourself the following questions:

1. Is our field as interesting as it once was? Are there still big discoveries to be made? Are there new directions to be found and explored?

2. Where are the best opportunities for discovery in the next 5-10 years?

3. Are there good opportunities for young scientists?

4. Are there faculty jobs?

5. Am I learning what I want to learn? Are there people to learn from?

6. Am I having fun?

7. And, somewhat differently, is physics education stagnant?

My answers should become clear during the talk, but I can say that for all the questions (including the last one!) my answers are a very strong 'yes'.

3 Some Stories

I keep over my desk two quotes compiled by Lillian Hoddeson[1]:

> Strong American laboratory leaders, such Ernest Lawrence, Luis Alvarez, Edward Lofgren, Edwin McMillan, Wolfgang Panofsky, and Robert R. Wilson, "who imposed their rythm on world science", shared a characteristic "pragmatic and utilitarian approach notable for its clear stress on 'getting numbers out.' "

> Victor Weisskopf, CERN's fourth director-general, a veteran of wartime Los Alamos, where time was perhaps the most pressing constraint," reflected "It is no good in this field to be excellent and always late.

I thought I could illustrate this spirit with a few stories. Some of these I know well, having been there; others were told to me by 'reliable sources', and may or may not be apocryphal. I'm sure that at least some, if note most,of the facts are wrong, so caveat emptor.

3.1 Bob Wilson

1. Bob and extracting a beam from the Berkeley 40" cyclotron[2]. On the occasion of Bob Wilson's 80th birthday there was a big evening celebration. But I happened to run into him in the cafeteria at lunchtime, and he and Peter Limon and I had lunch together. He told the following story of being a first-year grad student at Berkeley:....

2. When the Proton Lab was being built, Bob wanted to build a building for a control room (this became the Pagoda). Proton was primitive in those days, to say the least– it was built with sheet piling, and that spring it was all mud and water. We_all wanted to have a bathroom with running (clean) water, and were much less interested in the building; we also wanted magnets and beam. Bob held a meeting to discuss the building, and when he was met with some opposition to the pagoda design, and an recurrent emphasis on the bathroom, his reaction was that he had kept lots of trees just south of Proton, and could easily use them instead. Moreover, he said, bathrooms were a pain- once you agreed to one, people immediately wanted another (men and women's)[3].

3. Bob had no patience for bureaucracy or fiefdoms. We all called it the 'Genghis Khan style of management', but admired it at the same time. Once Peter Limon was complaining, loudly, about the management of the Neutrino area. Bob happened to walk by, and Peter saw him glance at him. The next day

Peter got a memo making him Deputy Head of Neutrino[4]. Bob also had an effective way of dealing with the natural growth of protectiveness in leaders of a department. When he felt that two competing departments were getting too entrenched, he would switch the heads, making the competition now one in which each knew the true weaknesses of the other.

4. Bob believed in working fast and solving problems as they came up, with an adiabatic approach so that one was working on the real problem at all times. I have a friend and colleague who built a beautiful little device to solve a problem in the proton cooling rings. He is a quiet and thoughtful man, and so it was with some trepidation that he brought his device to Bob's office to show him. My friend very quietly but proudly said 'it worked the first time'. Whereupon Bob jumped out of his chair, leaned over his desk and said 'do that again and you're fired!'.

5. After the high-y anomally fiasco, Bob called Cline, Mann, and Rubbia into his office (this is more a story about them, but...), and really chewed them out, in one of his legendary chewings-out. In the process he called them 'a bunch of flying clowns'. The three of them left his office and walked across the mezzinine outside the directors office stunned. But it didn't take more than half-way across when Carlo brightened up, and said 'well, maybe it's not so bad to be a flying clown...'.

3.2 Luis Alvarez

1. I was an undergrad working at SLAC for a summer, working on building the 40" rapid-cycling bubble chamber. This was the time when the big bubble chamber (the 80"?) was being moved from LBL to SLAC. The beam high at the Bevatron was 72" or so; at SLAC it was much less, being about waist high, if I remember correctly. I was in the office of Richard Blumberg, the engineer in charge, when Luis called to request that the beam high at SLAC be changed to 72". His proposal was to lower the grade of the whole SLAC experimental area by 3 feet or so (!). When Blumberg protested that taking 3 feet off of many many acres was impossibly expensive, the phone erupted so loudly he had to jerk it away from his ear: Luis shouted "It's small-minded people like you who are constantly getting in my way.".

2. There was a wine tasting at LBL in Berkeley when I was a grad student. I was on the terrace looking out over the crowd when Luis came up to me and put his arm around my shoulders. He said 'Henry, you should understand that there are two kinds of physicists: farmers, and explorers. Myself, I'm an explorer. Many of those others, they're farmers.'

3. The monopole story of Alvarez at the Lepton-Photon meeting at SLAC in 1974, and Feynman.

3.3 Richard Feynman

1. Feynman advocated 'active irresponsibility'- loosely translated as 'let George do it'. When he won the Nobel prize, Viki Weisskopf said 'Dick- it's really a shame that you've won it so young.' Feynman asked why, and Viki said 'You'll be put on every committee known to man- the committee for the starving orphans of Bosnia, the committee for.. and so on. You'll never do any creative work again.' Feynman thought the prize was irrelevant, and wouldn't change how he worked at all. The two consequently made a bet- for a substantial sum of money- that Feynman wouldn't hold a 'position of responsibility ' in the next 10 (0r 15?) years, where Feynman defined a 'position of responsibility' as one in which you told people who knew more about something than you did what to do. Giuseppe Cocconi was the keeper of the bet. At the appropriate time Weisskopf and Feynman met Cocconi in Geneva to decide the bet. Feynman had been on two committees in that time- the California State Board to select mathematics textbooks, and the Rose Bowl parade committee. With respect to the first he claimed he knew as much or more mathematics than anybody else on the Board; with respect to the second he claimed he knew as much or more about pretty women as anybody else in the Rose Bowl. Cocconi awarded him the bet.[5]

2. Mark Kislinger and myself at the Hawaii Summer School, and our trip to Kauai. 'You'll never amount to anything'.

3. 'Telegrams from the mainland', and the neutron total cross section versus energy. Feynman walked out.

3.4 Emilio Segre

I and some other grad students were waiting outside the door to the Building 50 auditorium at LBL for a meeting of the LBL senior physicists to end, so that we could go to the RPM (Research Progress Meeting- the big weekly LBL seminar). Segre' comes out the door, looks at all of us, and says 'I don't know what's wrong with you young people- one bomb- BOOM! Jobs for everybody'.. and walks off, leaving us just standing there staring at each other.

3.5 Enrico Fermi

There was a big meeting at Los Alamos of all the physicists in which Fermi announced the critical multiplication factor for neutrons on which the development of the bomb

depended. My dad was there - he hadn't yet finished his Ph.D when the war broke out, and so he followed Ray Herb to Los Alamos. He said that the front row of the meeting was filled with all the big-shots: Oppenheimer, Bohr, Teller, Ulam, Weisskopf, Von Neuman, Feynman, etc. Oppie started the meeting by saying that Enrico had made a major step, and would present the crucial number. Fermi then stood up, and gave the background, and then said 'the multiplciation factor is 2.3' (or some such number- I don't remember the number). Oppie then stood up again, and said 'We owe an enormous debt of gratitude to Enrico and his team for this critical work. However, Enrico, what is the uncertainty on this number?' Fermi stood up again, and said (I'm not really quoting- this is how I remember the story) 'I don't know the uncertainty, but it's good enough'. Oppie then stood up and very formally said "Enrico, I have been charged by the President of the United States with this project, and I feel that I cannot proceed without knowing the uncertainty on this number.' Fermi replied that he couldn't quote an uncertainty, as he didn't know it, but not to worry- 'it was good enough- the project would succeed'. Oppie then asked Fermi, if he couldn't quote an uncertainty, to at least set a limit on it. Whereupon Fermi stood up, grinned from ear-to-ear, and said (I'm making up the number), 'the uncertainty is not *smaller* than 0.2, and sat down.
Along these same lines, I asked Maurice Goldhaber, who had worked with Chadwick (and hence was around Rutherford) at the Cavendish when our current fetish about systematic errors took hold, and what Rutherford's attitude was about systematic uncertainties. He said (and again I paraphrase) 'He didn't put much stock in them. He would say 'If you don't believe your number, measure it again'. (Here's a guy who believed in getting results out fast!).
I was taught by my dad, and if I remember correctly, by Dave Jackson as well, that if you really trusted an experimenter you multiplied their quoted experimental uncertainty by π, and, if you didn't, you didn't pay any attention to the result at all.

4 My Concerns, for what they're worth (which may not be much).

The field has changed a lot as apparati and groups have gotten bigger, and as software and hardware have gotten more complex. I see adiabatic changes in directions that bother me, and thought it might be useful to talk about them, so that grad students can at least know that it hasn't always been so, and, if you want, doesn't have to be so. These may be places where you want to make changes (BOOM!?). I go from small to large...

1. The pace and importance of publishing have diminished. We are slow to publish (CDF is particularly poor at this), and do not put enough emphasis on getting the results out.

2. Authorship- I think it's lost its meaning. Grad students should get more credit for their papers, for example, rather than begin lost in the crowd. Papers are published with names of folks who don't even know that the paper exists, much less defend the science in it. Every author on a paper should be able to defend the science in it, I believe, at the minimum.

3. Complexity we now have the tools to make experiments fantastically complex. Processors are much faster, and yet analysis code links and runs slower. Memories are much bigger (Cronin and the rest of us shared 8K of core (24 bit words) when I arrived at UC)- and yet codes suffer from lack of memory. In many cases we're doing exactly the same kinds of tasks as before- e.g. cluster finding in an array of counters- but now we have lost flexibility and simplicity. Are we really tightly focussed on getting the results out fast?

4. Sid Drell and Viki Weisskopf always emphasized avoiding 'The Last Accelerator' syndrome– the idea that this may be the last accelerator, and so we have to make it big enough so that it will do the job (whatever that is at present), no matter what. This is not how science proceeds, and it carries a self-defeating element. The next machine is not the last accelerator- technology moves forward, and so does the science.

5. Time scales- We need to keep expertise in the field; this means having projects on time scales that are not long, with the scale set by a graduate student tenure. For example, in accelerator physics if we wait 10 years for starting something new, we won't have young bright flexible accelerator physicists. Adiabatic is important. The SSC is a good case studey- there were many reasons it didn't succeed, but one can ask where we would be now if we had sited it at Fermilab, and had started with pbarp at luminosities of up to 10^{33} at 40 TeV in a single ring. Much of the initial costs would have been charged to operating rather than to construction, and, I believe, the path from the Tevatron Collider to higher energies would be much easier for students and postdocs.

6. The Big One- looking elsewhere when there are big opportunities at hand. We have the possibility (not assured), that with additional manpower and money small on the scale of an entirely new machine we can discover the Higgs in the next 6 years (there are lots of other opportunities, in neutrino physics, cosmology, astrophysics, accelerator physics- I emphasize the one I think is most important). In addition there is a high likelihood, given our present knowledge, that we will find new physics, be it supersymmetry, new gauge bosons, etc., in that time. It's all in the luminosity- given 30 fb^{-1}S per detector on tape we can do it. We mustn't let this one slip through our fingers- looking toward the future is necessary and important, but the big and fun opportunity is now, and it's yours.

5 Conclusions

1. Go for it– speed matters. Don't go for bells and whistles- go for the physics.
2. Publish – it's the long-lasting output of what we do.
3. Be flexible – we train experimentalists, not just high energy physicists. You may end up in accelerator physics, biophysics, technology (e.g., inventing medical instruments), management (e.g. running a division of Microsoft), astrophysics, or cosmology, for example. Or, you may want to move into public policy (e.g. Sid Drell, Dick Garwin, Kurt Gottfried), education leadership, or politics.
4. Contribute to society– we are blessed to be able to do what we want. You can have a very big impact on science education, for example, with a rather small investment of time.
5. The Big One- we at Fermilab have the opportunity to make an enormous impact on science if we can get and use an integrated luminosity of 30 fb-1 or so. We should focus on this opportunity with the same intensity that LEP did boosting the machine energy over the past few years- this is our chance to really make a difference.

References

[1] Lillian Hoddeson

[2] Told to me and Peter Limon at lunch in the Fermilab cafeteria a day before or a day after Bob's 80th birthday.

[3] This must have been in 1971, when the Director's Office was in the Village.

[4] This is how I remember it. It may or may not be so.

[5] I'm not sure where I heard this. Again, it may or may not be so. I will check it.