September 9, 2021

Some Essays and Thoughts

H. J. Frisch

There have been a number of occasions in which I've wanted to or had to write something. This is a collection for myself and my family, mostly.

In Memory of S. Courtenay Wright

There was a young man from Vancouverd

Who neutrons and protons maneuvred

and the π and the μ

were to him and to you

as pretty as art in the Louvred.

Pierre Piroué Remembered

Some Vignettes of Working with Pierre
March 16, 2020

I learned so much from Pierre. Pierre and Jim Cronin had proposed E100– the 100th proposal to Fermilab– in 1970. The physics was exciting, looking in the large-angle large-momentum-transfer region of proton-nucleon scattering where the prevailing prediction was there should be zero events, but for which Bjorken, Berman and Kogut had predicted the momentum spectrum would show the scattering of constituents of the proton. The Lab was still deep in construction– the ring for the accelerator magnets was not yet complete. The Proton Lab, where E100 would be built in below-ground-level pits lined by sheet-piling, was primitive– if one had to characterize the working conditions in a word, it would be mud. Bob Wilson's presence was everywhere- speed, new ideas, and boldness was the spirit. I was fresh from graduate school; working with Pierre and Jim was a high honor but a little scary as I knew I knew so little. Pierre taught me an immense amount on experimental technique and discipline, among other things. I admired him greatly.

Some vignettes as a young colleague working with him as a mentor appear below.

1. **Experimental Technique: Consistency Is Better Than Truth**

   Pierre's mantra for taking data and commissioning equipment was 'Consistency is better than truth'. By that he meant that a systematic approach of starting at a 'baseline' configuration, changing one parameter at a time, and going back a step if the result wasn't what one expected, recording carefully and neatly in the Log Book, and periodically (often, even) going back to the baseline to check the consistency and reproducibility of the measurement. He explained by telling the following story.

   Pierre was in the Swiss ski army. His platoon along with others was outfitted with half-a-dozen small mortars carried on the back of one of a 2-man team. The platoons were regularly in a competition that consisted of skiing somewhere, setting up the mortars, and zeroing in on a target located on a neighboring mountain or in a neighboring valley. The prize went to the first platoon for which every mortar hit the target.

   According to Pierre, his platoon always won. The soldiers in the other platoons independently trained their assigned gun on the target and started firing, adjusting their aim after each shot. Pierre instead focused on precisely aligning his guns in a row so that the target had the same (consistent) coordinates for every gun, paying little attention to the exact (truth) location of the target. Then, while his competition was working independently to get each of their guns on the target one-at-a-time, Pierre ordered his platoon to cycle down the row of guns in turn, with each shot providing the necessary adjustment for all the guns. Once one gun was on target all the guns were. Hence "Consistency is better than truth/"

2. **Two Pencils**

   Pierre would come to Fermilab to run shifts on E100. He would schedule himself for 4 am so that he would be there during most of the Day Shift; I often would be on the Owl Shift, so I would be the one on shift when he arrived in the early morning.

   Pierre wanted everything related to data-taking to be very focused, so before he would arrive I would clean off the shift desk in the Porta-Kamp so that there would be only the Log Book, 2 pencils, and the bottle of Rubber Cement (do not forget the rubber cement, Best Beloved[1]).

Pierre would come in, greet me, hang up his coat, and sit down carefully at the desk. He would then say " You are using two pencils?" I would say "No, Pierre, only one." Pierre would say "I put the other away, is it OK?", opening the desk drawer. And I would say "yes, of course". Then he would say "Now tell me what you are doing".

3. **Log Book Etiquette**

   An essential part of data-taking was taking pictures of displays or oscilloscope traces with a Polaroid camera. Pierre initiated any new person on shift in the essential skill of pasting the Polaroids into the Log Book. The assigned tool was his own large brown bottle of rubber cement with a thick brush that was part of the lid. Pierre showed each of us how one unscrewed the top, got the exact amount of rubber cement on the brush, and then brushed the back of the Polaroid in a prescribed pattern – first side-to-side and then vertically, to make a uniform layer over the whole back. We called this 'buttering', but it was a serious business.

   One early morning Pierre started on this essential ritual on a picture I had just taken; I looked over and he was buttering it very carefully in the prescribed pattern, but, when I looked, he looked down and realized he was buttering the picture side.

4. **Writing a Phys Rev Letter:**

   The first years of Fermilab operation was an exciting time; a new energy regime, and emerging ideas on the constituents of the proton. We made a number of pioneering measurements on the scattering and fragmentation of partons. We also discovered 'direct muons', which turned out later to be a signal of the yet-undiscovered Charm quark. Pierre took charge of writing one of our Phys Rev Letters; he gave us his draft with strict instructions on editing. We were not allowed to rewrite or add words, but only allowed to delete. (I still occasionally use his technique, but relaxed to include exceptions.)

5. **Two Experienced Professors and One Oscilloscope**

   A (very) minor story, but it gives a sense of the intensity of Pierre and Jim at work together.

   I drove in to Fermilab for my shift, and found Pierre and Jim in the Porta-Kamp shoulder-to-shoulder in front of a Tektronix oscilloscope (this was in the 70's- light blue small 700-series (I think) scope on a stand). They were seated very close to it, both pushing buttons and turning dials, trying to find the scope trace.

   I looked closely, and then plugged the scope into the wall, and left (I didn't know what to say other than 'it helps to plug it in', and my guess was that it was better to slide quietly out the door). Neither Pierre or Jim ever said anything about it.

6. **Management: E100 and The Hairy Arm**

   At the very outset of E100, with 'the world so new and all', the Lab required us to negotiate an Agreement that listed the equipment requests. The list was substantial,

[1]R. Kipling; Just So Stories

with two B2 (Main Ring) magnets, four Main Ring quadrupoles, the PortaKamp, electronics, etc.– a large sum of money. Pierre added the last item on the list: 'Two Brooms'.

John Peoples was Head of the Proton Lab, where E100 was to be installed in Proton East. Pierre, Mel Shochet and I met with John, and Pierre presented him with the list. John scanned down it quickly, and said "Why do you need two brooms?". Pierre said "Somebody may be using the other one". John said "That's ridiculous- you can't have two brooms". Pierre said "I must have two brooms". The situation escalated from there, ending up with them almost touching noses shouting at each other, Pierre threatening to cancel the experiment and just walk away, and John using unusual language for an administrator. It really sounded like it was all over.

Then Pierre said "OK, you bastard– you win– one broom'. John said "fine- one broom", and reached for the agreement to sign. Pierre signed, and we left. Once we were out of ear-shot we asked Pierre- "what was that all about?", and also "You mean we got everything else on the list?"

Pierre said "Let me explain. This is called 'The Hairy Arm' ". Once in Renaissance Italy there was a painter of portraits of society ladies. He was worn out by the constant complaining: 'my nose isn't that big'; my eyes are not that close together'; my chin doesn't have folds'; so one day after finishing a portrait of a particularly ugly lady he took a piece of charcoal and limned in black hair on her arms.

The lady came in, and immediately objected, saying 'my arms aren't that hairy'. They had a big fight, and eventually the painter reluctantly conceded, saying that he would delete the hair on her arms. The lady left victorious. The painter then wiped the charcoal from the portrait with a cloth. Her big nose, close-set eyes, and unpleasant chin remained untouched.

The remarkable thing about the technique is how well it works. Pierre understood people so well; his ability to push the boundaries came from a deep understanding of us individually. It was subtle, in that I didn't realize it at first, but it was one of the sources of my and others' deep fondness of him.

7. **Hardware: Is It Professor-Proof?**

Pierre designed the two differential Cherenkov counters for E100, critical elements of the experiment for determining the particle type (π,K, or p). They were works-of-art; 86-feet long stainless steel with formidable flanges and very precise optics for separating particle type into two channels by velocity. The gas handling system was complex with lots of valves and gauges.

Two of the Princeton technicians came to Fermilab to install the counters. The gas handling system controls were mounted on a beautiful rack-mounted panel, silk-screened on the front to show the connections behind the panel between the various valves, gauges, and relays. Howard and the other engineer spent several days installing the system controls in the pit and in a rack in the Porta-Kamp. At the end of the second day they announced to Pierre that they were done and it was all tested and working. Pierre asked "Is it Professor-proof?" They said absolutely– it was all tested and working. Pierre, elegantly lifting the knees of his grey suit pants, squatted down in front of the panel, adjusted his balance, and then started madly tossing all the switches and twirling valves in a whirlwind. He paused for a moment and leaned back. There was a sudden loud bang and black smoke and then flames came up from the panel. Pierre stood up, dusted his hands, and said 'Not Professor-proof'. He then went out the door, leaving us with the fire.

I went around behind the racks to unplug the AC cord, but the flames continued. I didn't know it, but the panel upstairs and the panel in the experimental pit shared AC power, so the panel was still powered.

I don't think we told anybody official. The panel was made Professor-proof; Pierre was a one-man Safety Review Panel.

I really loved and admired Pierre. This was a wonderful time at Fermilab, intellectually and professionally– very exploratory, with few boundaries. The intellectual landscape included such influential scientists such as Bob Wilson, Jim Cronin, Bj Bjorken, and Richard Feynman. Pierre provided a grounding and a clear philosophy for living and working. He meant a lot to me.

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Professor of Physics
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University of Chicago

Version 1.1
January 1, 2018
HJF

Fermi's Witty Response: A Blackboard with Two, Not One, 'Mistakes'?

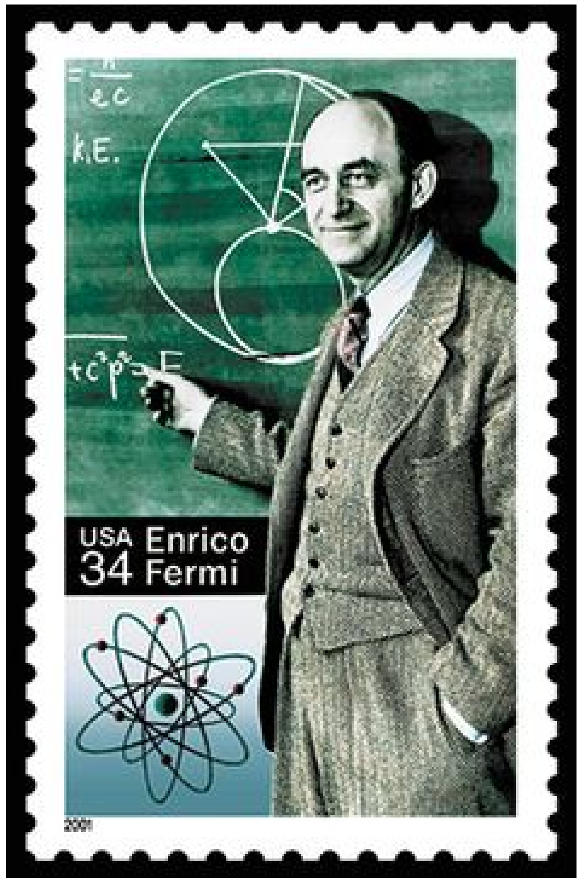


Figure 1: The famous stamp showing Fermi at the blackboard with several formulae and a diagram of circles and triangles.

1 Introduction

I am not the first to doubt that Enrico Fermi would make the much discussed 'mistake' on the blackboard shown in the 1991 US stamp (Figure 1). In a letter to Nature in 1992, Richard Garwin wrote "...it is difficult to believe that Fermi could have written it...", and then goes on to write "The most probably explanation is that Enrico Fermi, a great physicist, both in theory and experiment, and a man full of fun and humour, was having a little fun with the photographer."[2]. In a Symposium on Fermi in 2001, Jim Cronin suggested that '"He might have been pulling our leg"[1]. Garwin was Fermi's student, and Cronin had classes from Fermi; they knew him, in Garwin's case exceptionally well. However the idea that Fermi made a mistake has widely taken hold in the popular mind [3].

2 The Hypothesis: Fermi's Witty Response To An Unwanted Request

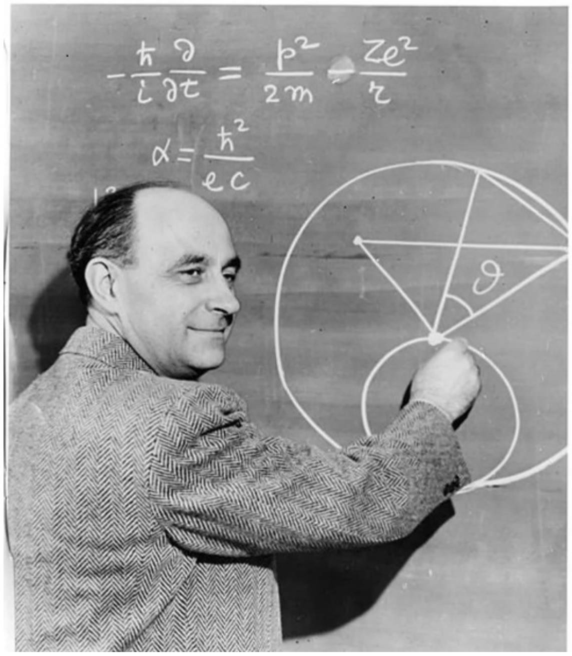


Figure 2: A photograph very similar – presumably from the same Public Relations session– to the photograph from which the stamp was derived. Note both the incorrect formula for alpha and the unusual diagram, which Fermi gives the impression to have just finished drawing.

Following Garwin's and Cronin's suggestions, one can guess what was the context for the 'mistake' in the equation for α. From the number of similar images it is clear that these are staged pictures in a photography session, probably for some article or public announcement. Fermi was most likely told by someone from the UofC Public Relations department to go to the blackboard and write several equations and make a diagram. As described below, in an interaction with Oppenheimer at Los Alamos, Fermi could not be talked into doing something he felt was false. My guess is that, much as in the story with Oppenheimer, after refusing several times, in this case to pretend to be teaching, Fermi decided it was easier to acquiesce than to resist, but would write something on the board that was a clear signal that this was not an authentic picture of him at work. What could be more concise and telling than for Fermi, the master of coupling constants (see Section 4), to write the electromagnetic coupling constant not proportional to the electron charge squared, but to the square of the fundamental unit of quantum mechanics, as far as we know a completely unrelated universal constant? It is so wrong in so many deep but obvious ways that any physicist would immediately pick up on it as a protest[1].

I should add that I was put in the same situation by a team of University of Chicago Public Relations staff, and was saved only by the fact that my young daughter had painted my fingernails white. When I pointed to the blackboard in a staged re-enactment of teaching, the team making the film turned white as well (my own unintended but effective protest).

[1]In 'natural units', both Planck's constant $\hbar$ and the velocity of light c are set $= 1$, so the formula as Fermi wrote it would be $\alpha = \frac{1}{e}$; painful even to contemplate.

3 A Family Story

My parents, David and Rose, came to Los Alamos at age 25 in early 1943 from Wisconsin, where they were in graduate school and Dave was studying cross-sections in Ray Herbst's Van De Graaff group. My dad told me the following story.[2]

Dave said Oppenheimer called what is now called an 'all-hands meeting'. The front row consisted of Bohr, Bethe, Weisskopf, Feynman, Rabi, Teller, and other luminaries. Oppie tells the assembly "Enrico has some wonderful news for us. Enrico, would you tell the group the news on the multiplication factor?". Whereupon Fermi stands up, faces the crowd, says "The number is 2.3", and sits down.

Oppenheimer says "This is wonderful news; this means the Gadget will work", and asks for a round of applause. "However, Enrico, there must be an uncertainty on the number– could you tell us the uncertainty?" Fermi stands up, and says "I don't know the uncertainty. But don't worry, it will work". And sits down again. My dad said that at this point Oppenheimer becomes very formal, and says words to the effect that he had been charged by the President of the United States with the success of the Project and that the future of the Free World hung on this number, and he had to have the uncertainty. Whereupon Fermi stands up again, and says "I don't know the uncertainty", and sits down. Oppenheimer, challenged in front of a large audience, says "Enrico, if you cannot quote a number, could you at least put a limit on the uncertainty." Whereupon Fermi stands up, faces the audience, grins, and says 'The uncertainty is not less than 0.1", and sits down.

The point is that "not less than" is the wrong limit. To be useful, it needs to be an upper limit and not a lower limit, as a lower limit allows uncertainties so large that the number itself is meaningless. Note also that Fermi, rather than argue, did literally what was demanded, in a way that seemed to satisfy the request but on further thought was in fact a dramatic protest. Note also the reference to the grin (See his grin in Figures 1 and 2).

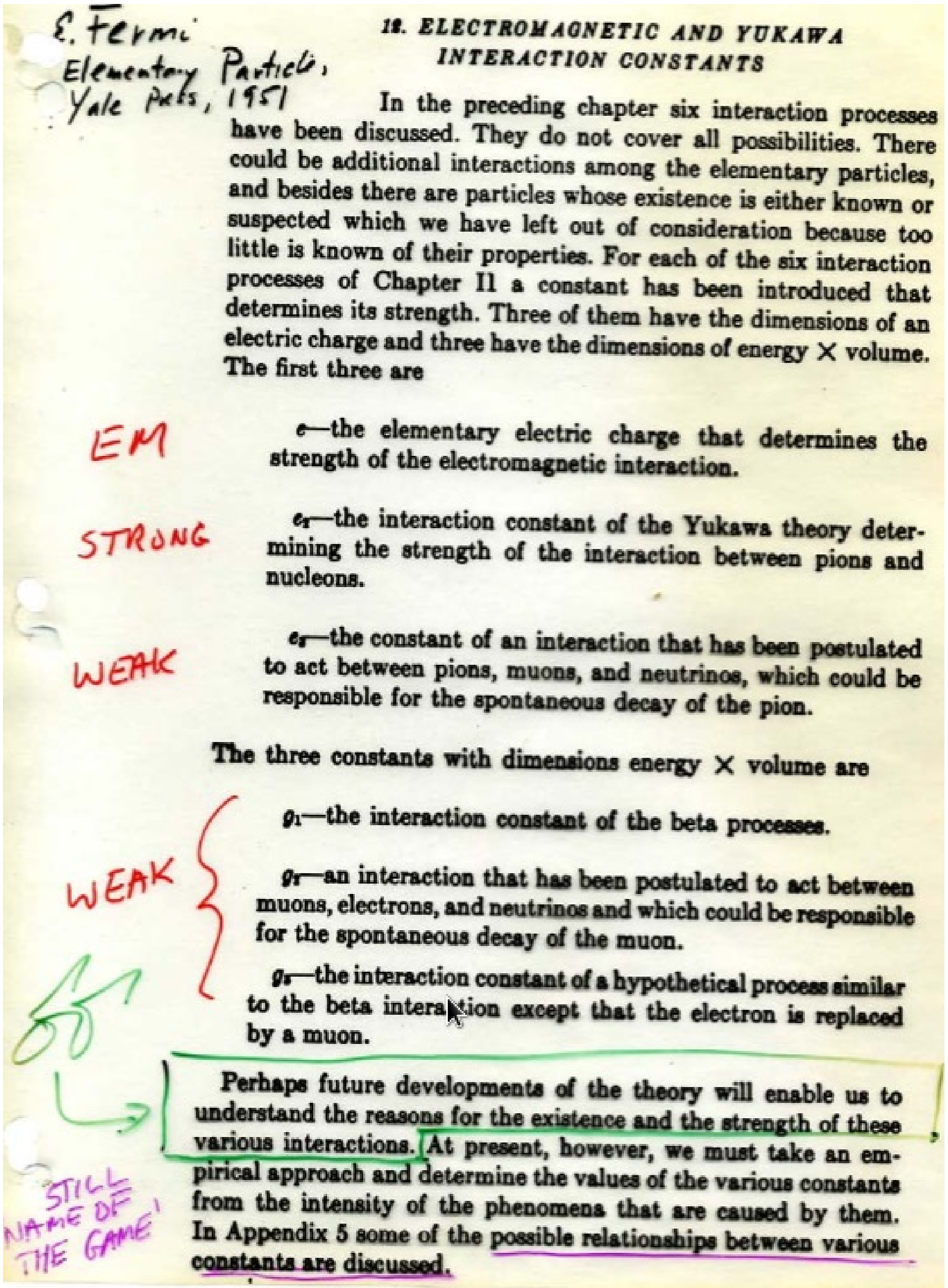
4 Fermi and Coupling Constants

The choice of $\alpha = e^2/\hbar c$ as the response to "Could you write an equation on the board?" is natural– it's a simple relationship, and the formula is physically transparent, being basically a change of units (α is just the square of the electric charge e). In Fermi's Yale lectures in 1950 he discussed the importance of the couplings in the larger picture of the fundamental forces. Figure 3 shows his thoughts on the couplings. To me there is no way that he would write alpha incorrectly, with the coupling inverse to the charge. This was a protest, much as the wrong limit presented to Oppie, and with a grin.

5 The 'Nonsense' Diagram: An Additional Clue?

I believe there is a definitive test of the hypothesis that the 'mistake' is instead a witty protest for sophisticated viewers. I have not seen discussed the diagram Fermi has drawn on the board. Like the equation for alpha, it's very elegant, consisting of two circles and two triangles. It is drawn well, in a clear and bold hand.

[2] This is as I remember it – quite possibly not as told. The number 2.3 is made up- I don't remember what Dave said it was.



E. Fermi
Elementary Particles,
Yale Pubs, 1951

18. ELECTROMAGNETIC AND YUKAWA INTERACTION CONSTANTS

In the preceding chapter six interaction processes have been discussed. They do not cover all possibilities. There could be additional interactions among the elementary particles, and besides there are particles whose existence is either known or suspected which we have left out of consideration because too little is known of their properties. For each of the six interaction processes of Chapter II a constant has been introduced that determines its strength. Three of them have the dimensions of an electric charge and three have the dimensions of energy × volume. The first three are

EM

e—the elementary electric charge that determines the strength of the electromagnetic interaction.

STRONG

e_1—the interaction constant of the Yukawa theory determining the strength of the interaction between pions and nucleons.

WEAK

e_2—the constant of an interaction that has been postulated to act between pions, muons, and neutrinos, which could be responsible for the spontaneous decay of the pion.

The three constants with dimensions energy × volume are

WEAK

g_1—the interaction constant of the beta processes.

g_2—an interaction that has been postulated to act between muons, electrons, and neutrinos and which could be responsible for the spontaneous decay of the muon.

g_3—the interaction constant of a hypothetical process similar to the beta interaction except that the electron is replaced by a muon.

Perhaps future developments of the theory will enable us to understand the reasons for the existence and the strength of these various interactions. At present, however, we must take an empirical approach and determine the values of the various constants from the intensity of the phenomena that are caused by them. In Appendix 5 some of the possible relationships between various constants are discussed.

STILL 'NAME OF THE GAME'

Figure 3: A page from Fermi's 1950 Yale Silliman lectures on the future unification of forces and the relationships of their coupling constants. (The scribblings are my own for a long-gone talk.)

However, I have not been able to think of a physical process or object that the diagram describes[3]. While striking and appealing, I think it too is a subtle but, with some thought obvious, 'mistake'. My hypothesis is that this too is a witty protest– Fermi was told to draw a diagram as well as write some equations on the board, and he drew a picture that has no basis in physics. I personally think it is a brilliant response to a spur-of-the-moment need in its clarity and appeal [5].

[3]I would welcome references for prior discussions of the diagram– it seems odd to me that it hasn't been discussed along with the equation for alpha.

In the spirit of 'a priori' testing of hypotheses, the diagram can serve as a test. If the picture has a well-defined physics context, the situation will remain as it is with alpha being the sole possible protest. However, if physicists across a wide array of fields cannot find a plausible explanation for what I think is a 'nonsense diagram', then the diagram is a second 'mistake', and one that cannot be accidental. In this case Fermi was sent to the board and arm-twisted into writing on it for public relations reasons, and both the 'wrong' equation for alpha and the meaningless diagram constituted a witty message of resistance for the cognoscenti.

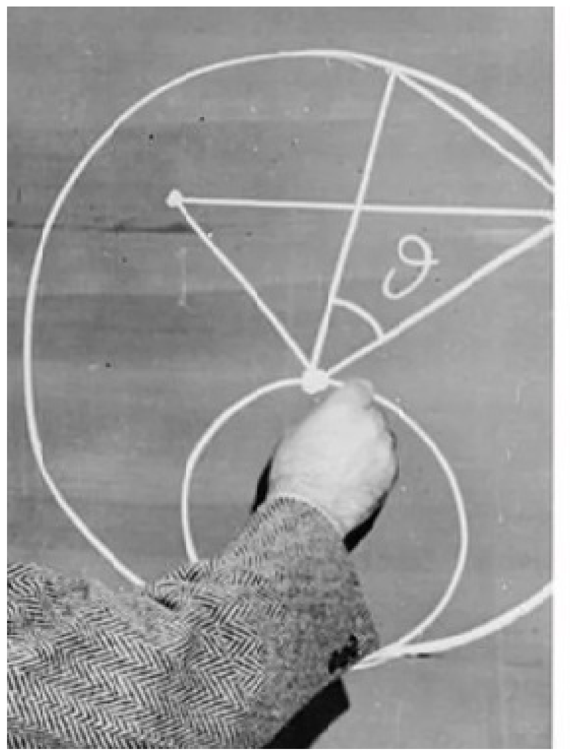


Figure 4: The mysterious diagram on the blackboard. Is there a physical process or object that it represents?

6 Epilogue– Note on Uncertainties in HEP

To be hard-nosed about the Los Alamos story, I believe that it is quite likely that Fermi did not 'know' a number for the uncertainty on the multiplication factor, but had other evidence from the experiments making him confident that the chain reaction would work. Thus he was being precise and responsible, rather than perverse, in not quoting a number to Oppenheimer. In both cases the 'protest' was not subtle when expressed in the language of Physics.

References

[1] J. W. Cronin, as quoted by Richard Mertens, University of Chicago Magazine, Vol. 94, #2. Mertens writes: "Cronin says Fermi may have been asked to put some scientific-

looking stuff on the board as background for the photo session: 'He might have been pulling our leg'."

[2] R. Garwin, Nature Vol 355, 20 Feb. 668 1992;
https://www.nature.com/articles/355668d0.pdf

[3] See, for example, G. Huber, *Postage Stamp Poses a Fermi Problem.* Science Vol 294, Oct 2001.

[4] http://fermi.lib.uchicago.edu/fermicommemoration.htm

[5] I thank Andrey Elagin and Carla Grosso-Pilcher for pointing out that drawing geometric diagrams, often with circles and inscribed triangles, was a basic part of the mathematics curriculum in elementary school when Fermi was young. The drawing mastery thus would be from practice.

Jim, Hard Scattering, and the Development of the Parton Model

We have become used to the idea that matter has a structure smaller than protons- it wasn’t so in 1970…

1. Introduction: Partons & Hadrons, and Hadrons & Partons
2. Context: a new national lab, new energy reach, challenges
3. 1970: Jim and Pierre propose Fermilab Experiment E100
4. `Discoveries’ (or almost)- parton-like particle production, direct muons, the ‘Cronin-Effect’ in nuclei
5. 1976: Jim heads up the Colliding Beam Experiments Dept. (the seeds of the Collider Detector at Fermilab (CDF))
6. 1984: If Wishes Were Horses: The pbar-p SSC option: Jim’s vision of a more careful and more real approach to the SSC
7. Working with Jim…
8. Taking stock- high-Pt parton production, charm, RHIC/Alice

Probing a New Energy Region

Fermilab was coming on the air. Wilson's vision was it would be a national lab rather than 'in-house', and so an opportunity to propose new ideas with strong technical support ($, talent). It was exciting.

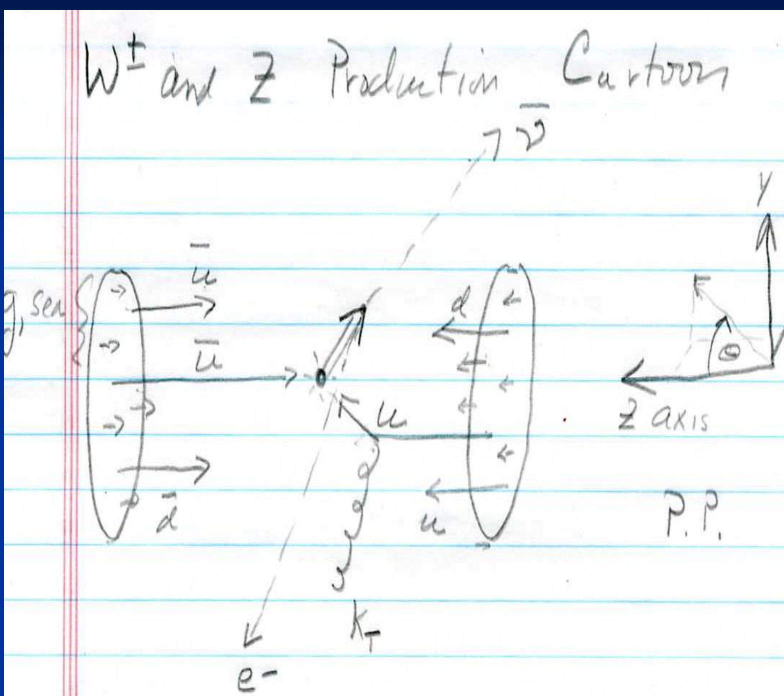
Going from 30 GeV to 200 GeV seemed like an enormous jump, opening up a huge energy region for the discovery of what was really going on at short distances. It was a simpler time, and the opportunity to explore was so clear…

But where to look? Jim and Pierre proposed looking at particle production in a region where conventional wisdom (sic) predicted there wouldn't be particles- large momentum perpendicular to the beam direction. The rule-of-thumb was the spectrum was exponential and very very steep (e^{-6P_T}), where P_T is 'transverse mom.'.

And in Jim's style, the apparatus was simple and could be built by a small group- 6 people. `Single-arm' magnetic spectrometer at 90^o

However, the scale was new- 90^o in the c.m. transforms into a long spectrometer at a small angle in the laboratory frame.

Hard Parton Scattering-Introduction



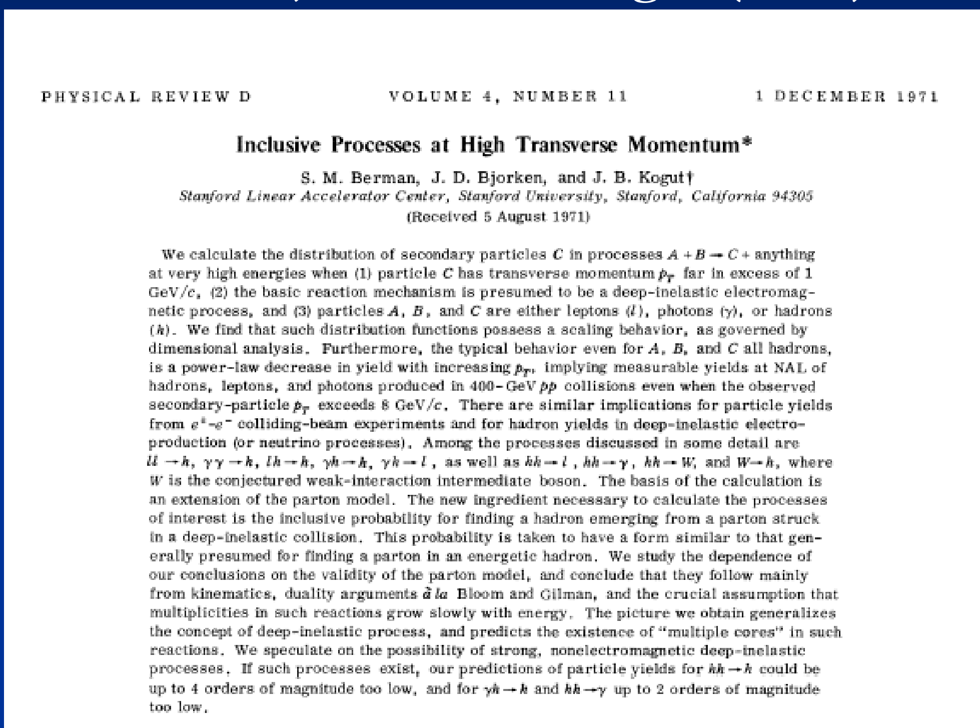
A parton is a quark or gluon-
carry color, and so aren't `free

A hadron is a strongly interacting particle made of partons- e.g. the proton, neutron, pion, kaon, c- and b mesons, s,c,and b containing baryons

A "Cartoon" of a hard parton `scattering' producing a W boson in pbarp collisions

Hard Parton Scattering

Berman, Bjorken, and Kogut (BBK)- 1971



PHYSICAL REVIEW D VOLUME 4, NUMBER 11 1 DECEMBER 1971

Inclusive Processes at High Transverse Momentum*

S. M. Berman, J. D. Bjorken, and J. B. Kogut†
Stanford Linear Accelerator Center, Stanford University, Stanford, California 94305

(Received 5 August 1971)

We calculate the distribution of secondary particles C in processes $A + B \to C +$ anything at very high energies when (1) particle C has transverse momentum p_T far in excess of 1 GeV/c, (2) the basic reaction mechanism is presumed to be a deep-inelastic electromagnetic process, and (3) particles A, B, and C are either leptons (l), photons (γ), or hadrons (h). We find that such distribution functions possess a scaling behavior, as governed by dimensional analysis. Furthermore, the typical behavior even for A, B, and C all hadrons, is a power-law decrease in yield with increasing p_T, implying measurable yields at NAL of hadrons, leptons, and photons produced in 400-GeV pp collisions even when the observed secondary-particle p_T exceeds 8 GeV/c. There are similar implications for particle yields from e^+-e^- colliding-beam experiments and for hadron yields in deep-inelastic electroproduction (or neutrino processes). Among the processes discussed in some detail are $l\bar{l} \to h$, $\gamma\gamma \to h$, $lh \to h$, $\gamma h \to h$, $\gamma h \to l$, as well as $hh \to l$, $hh \to \gamma$, $hh \to W$, and $W \to h$, where W is the conjectured weak-interaction intermediate boson. The basis of the calculation is an extension of the parton model. The new ingredient necessary to calculate the processes of interest is the inclusive probability for finding a hadron emerging from a parton struck in a deep-inelastic collision. This probability is taken to have a form similar to that generally presumed for finding a parton in an energetic hadron. We study the dependence of our conclusions on the validity of the parton model, and conclude that they follow mainly from kinematics, duality arguments *à la* Bloom and Gilman, and the crucial assumption that multiplicities in such reactions grow slowly with energy. The picture we obtain generalizes the concept of deep-inelastic process, and predicts the existence of "multiple cores" in such reactions. We speculate on the possibility of strong, nonelectromagnetic deep-inelastic processes. If such processes exist, our predictions of particle yields for $hh \to h$ could be up to 4 orders of magnitude too low, and for $\gamma h \to h$ and $hh \to \gamma$ up to 2 orders of magnitude too low.

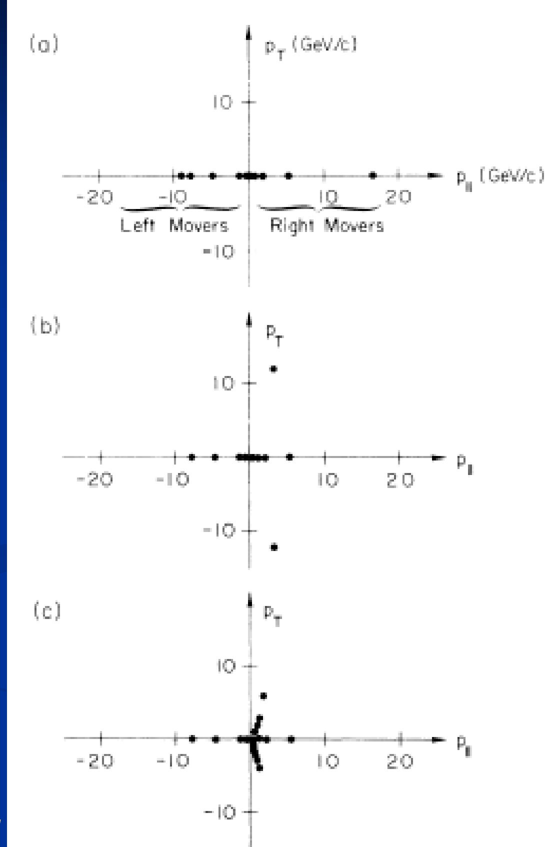


FIG. 4. A momentum-space visualization of hadron-hadron deep-inelastic scattering occurring in three steps.

Momentum space- $P_{longitudinal}$ along the beams; P_T Transverse

Dots are partons; scales are in GeV.

Hard Parton Scattering

BBK Predictions on hard parton scattering, annhilation to the W and Z, direct leptons,…

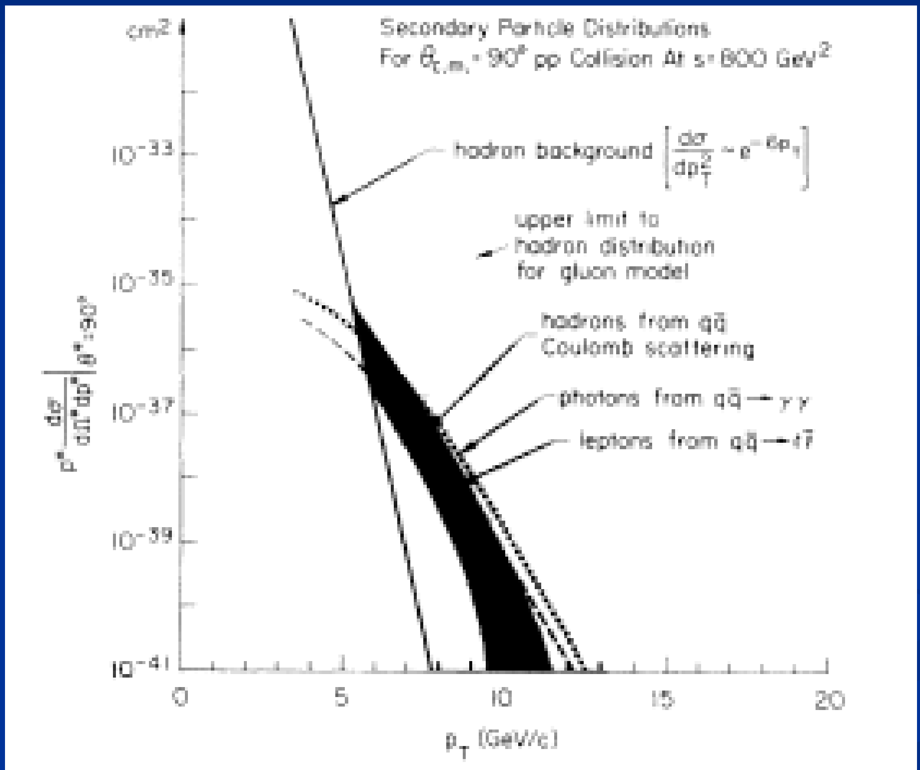


FIG. 1. Secondary-particle distributions as calculated in the parton model and compared to diffractive backgrounds for typical NAL conditions.

High-PT Particle Production: E100 at Fermilab: 1970-77

"A PROPOSAL TO STUDY PARTICLE PRODUCTION AT HIGH TRANSVERSE MOMENTA"

J. W. Cronin and P. A. Piroué

Princeton University

ABSTRACT

We propose to study the particle constituents of a beam produced at 80 mrad lab angle ($\sim 90^{\circ}$ in the p-p c.m. system) by 200-500 GeV protons striking a target. Such an exploratory investigation would provide information on

1) hadron production at high transverse momentum.
2) the possible existence of the weak intermediate boson, heavy photons, and heavy leptons by searching for leptons with high transverse momentum.
3) the possible existence of long-lived particles (with or without fractional charge). In addition, with slight modifications of the apparatus, we could search for short-lived particles and also direct photon production.

December 1, 1970

Correspondent: J. W. Cronin

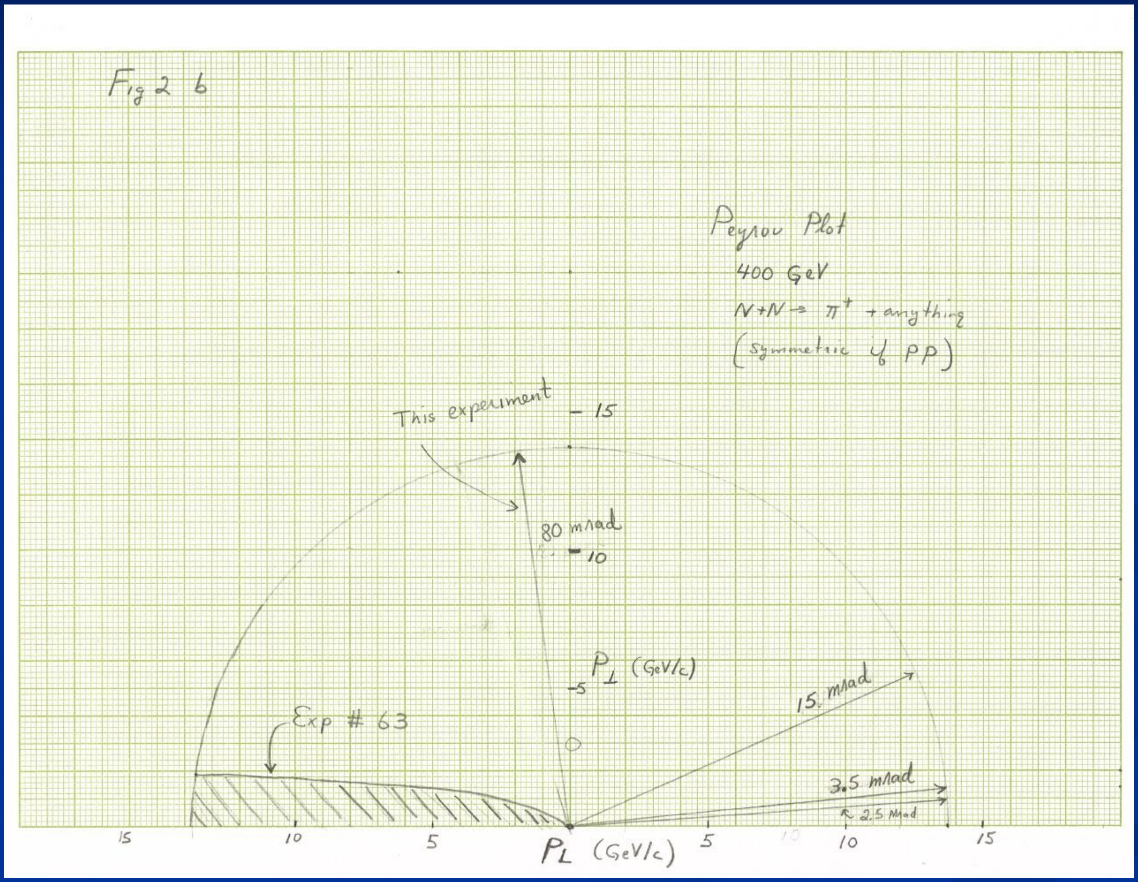
Jim and Pierre: Fermilab Proposal, Expt100, 1970:

"…an Exploratory Investigation…"

1. **High Pt Hadron Production**
2. **The W boson**
3. **The Z boson (`heavy photon')**
4. **Charm, beauty (`Short-lived particles')**

E100 at Fermilab: 1970-77

Figure 1 of the E100 Proposal – the "Peyrou Plot" at NAL



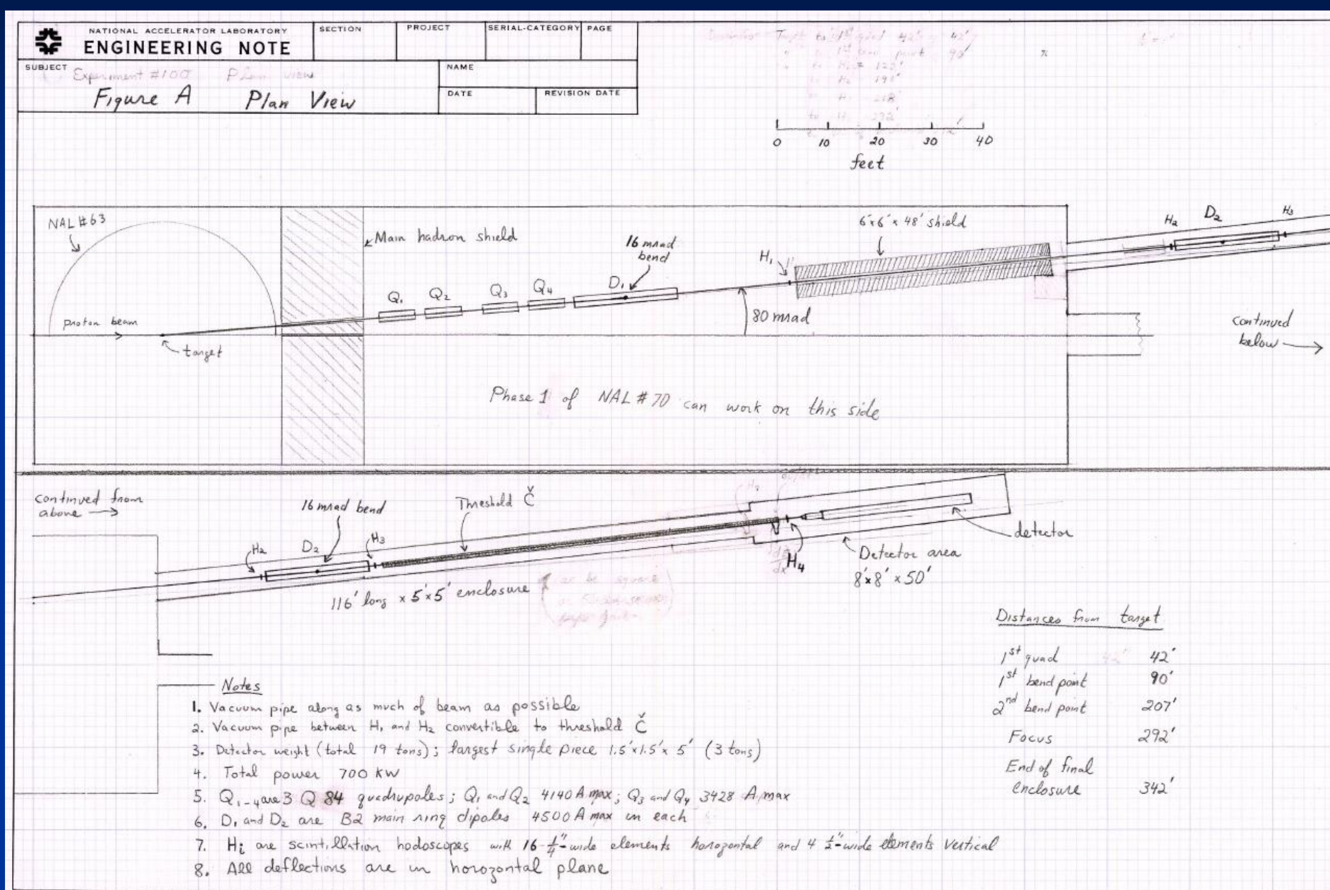
(JWC hand-drawn original)

$P_{transverse}$

The transverse direction is perpendicular to the beam- looking at collisions that scatter at 90^0

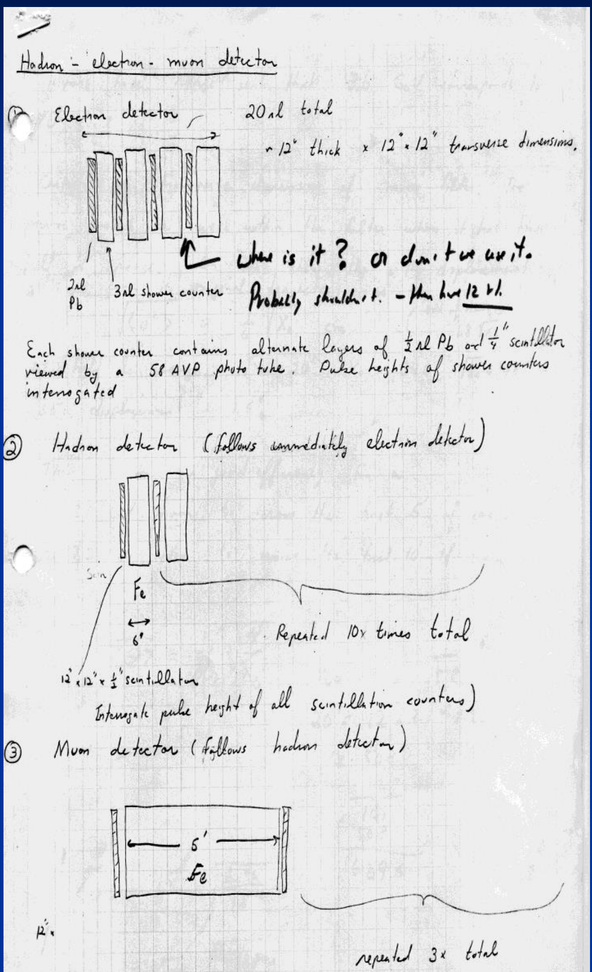
$P_{longitudinal}$ (along the initial beam direction)

E100 at Fermilab: 1970-77



Jim's hand-drawn layout of the E100 spectrometer- 100 yards long…

E100 at Fermilab: 1970-77


Hadron - electron - muon detector

① Electron detector 20 X_0 total

~12" thick x 12" x 12" transverse dimensions.

← where is it? or don't we use it.

Probably shouldn't. - then have 12 X_0.

2 X_0 Pb 3 X_0 shower counter

Each shower counter contains alternate layers of $\frac{1}{4}$ X_0 Pb and $\frac{1}{4}$" scintillator viewed by a 58 AVP photo tube. Pulse heights of shower counters interrogated

② Hadron detector (follows immediately electron detector)

Fe

6"

Repeated 10x times total

12" x 12" x $\frac{1}{4}$" scintillator

Interrogate pulse height of all scintillation counters)

③ Muon detector (follows hadron detector)

5'

Fe

repeated 3x total

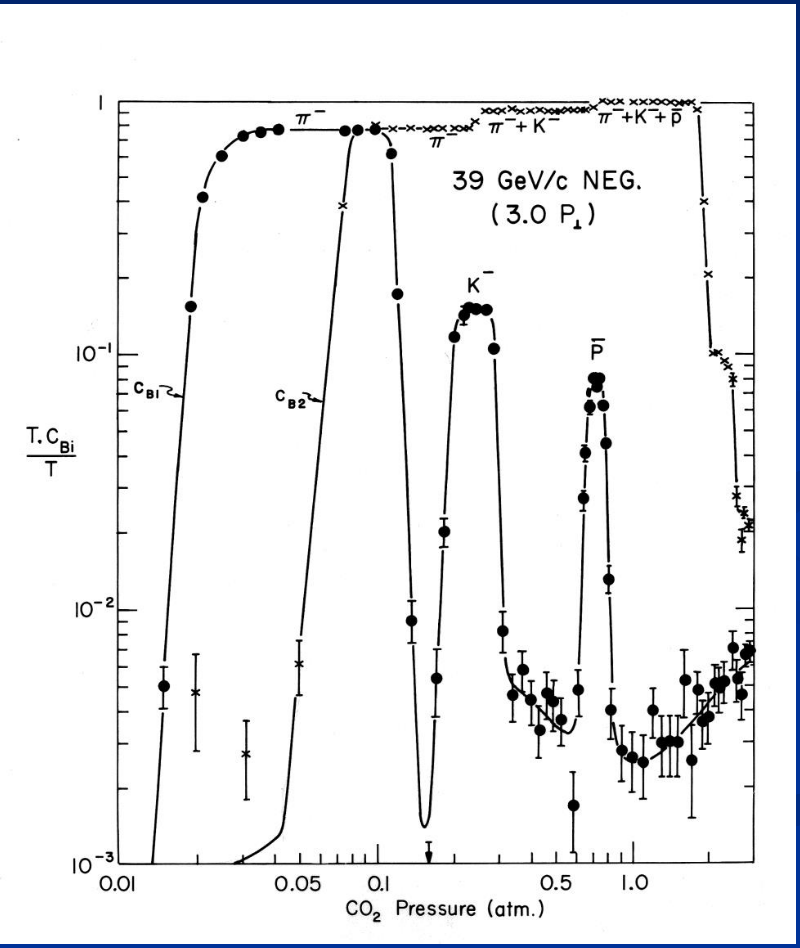
Particle Identification – not so differen from the standard collider "kit" nowadays (exept for Pierre's beautiful Cherenkov counters, and the Lorentz frame):

1. **Magnetic Spectrometer for momentum**
2. **Pb/Scint EM Calorimeter for Electron ID**
3. **Steel/Scint Stack for Muon/Hadron Separation**
4. **Innovative "Shutter" for Lifetime Extrapolation**

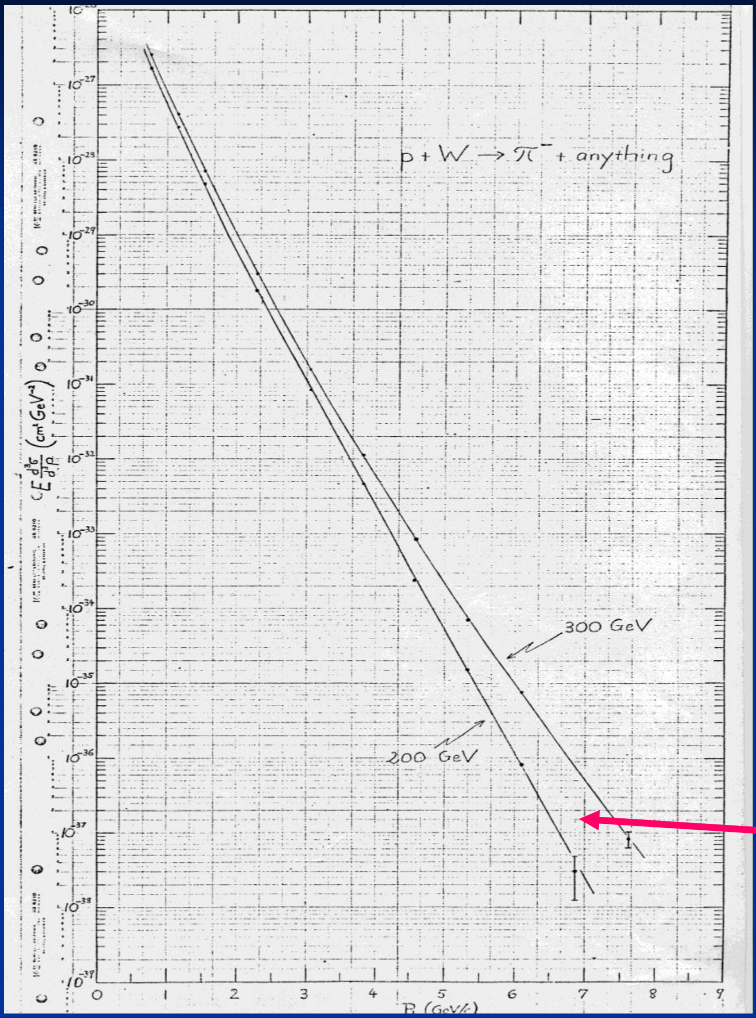
E100 at Fermilab: 1970-77

One real strength of E100 was particle identification via Pierre's Cherenkov ctrs- a capability largely lost in modern collider detectors:

A `Pressure Curve'- index of refraction of gas changes vs pressure, and particles at the same momenta but different velocities produce light at angle cos(theta) = 1/(beta n)



E100 at Fermilab: 1970-77



First Results- 1972- see power-law behavior and energy dependence at large Pt

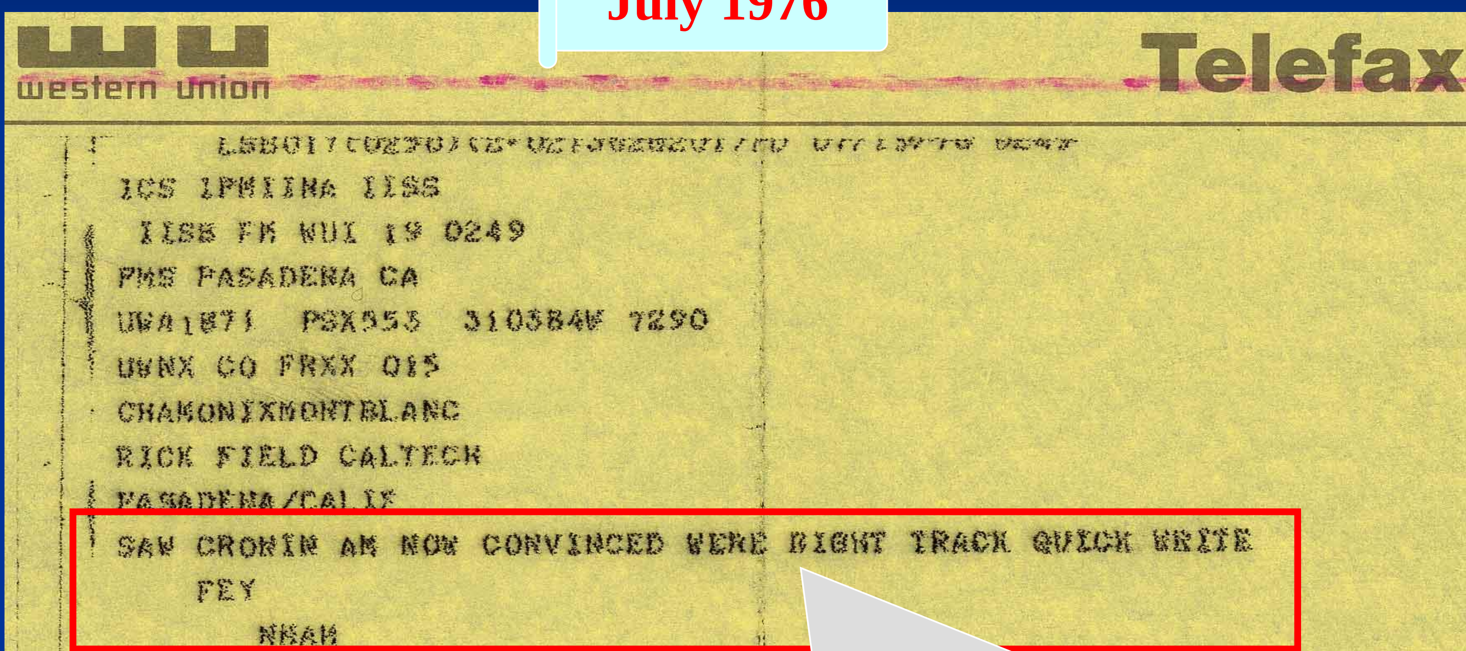
BUT- ISR beat us to punch line (sadly, and barely)

Note energy-dependence at high Pt- evidence of hard scatters

From Rick Field's Lectures at UC, July 2006

Telagram (sic) from Feynman

July 1976


western union Telefax

ICS IPMIINA IIS6
II56 FM WUI 19 0249
PMS PASADENA CA
UWA1871 PSX953 310384W 7290
UWNX CO FRXX 015
CHAMONIXMONTBLANC
RICK FIELD CALTECH
PASADENA/CALIF
SAW CRONIN AM NOW CONVINCED WERE RIGHT TRACK QUICK WRITE
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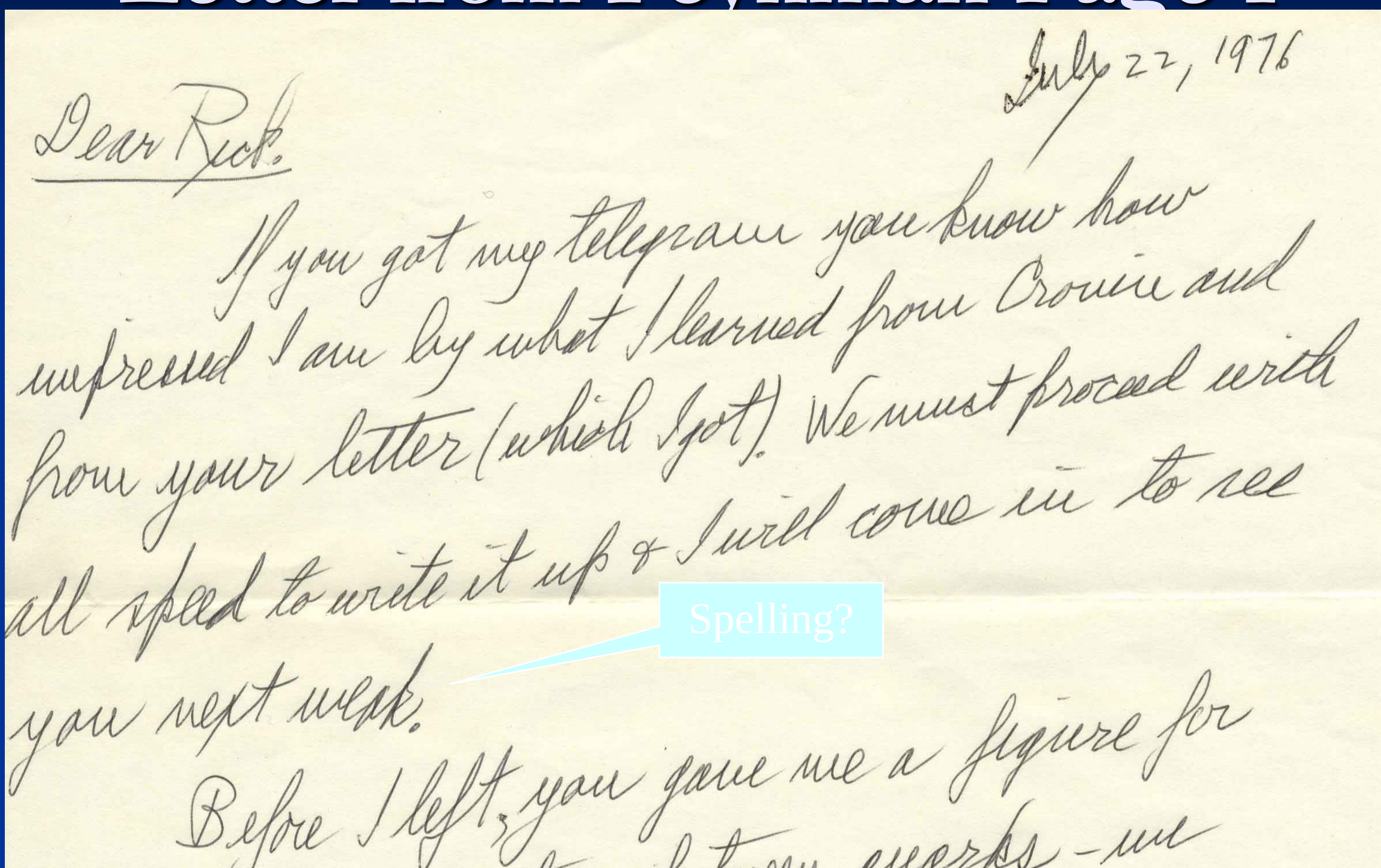
SAW CRONIN AM NOW CONVINCED WERE RIGHT TRACK QUICK WRITE FEYNMAN

Letter from Feynman Page 1



July 22, 1976

Dear Rick.

If you got my telegram you know how impressed I am by what I learned from Cronin and from your letter (which I got). We must proceed with all speed to write it up & I will come in to see you next weak.

Before I left, you gave me a figure for ... between quarks - ...

From Rick Field's Lectures at UC, July 2006

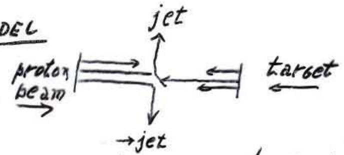
Feynman Talk at Coral Gables (December 1976)

1st transparency

Field & Feynman CALT-68-565

Fox (Brookhaven APS) CALT-68-573

MODEL



Quark - Quark Collision.

But $P_\perp^{-8}$ Not $P_\perp^{-4}$?

Need: (a) Quark distribution in hadron. (Pion?)

(b) The way quark makes hadron jet.

FROM EXPERIMENTS WITH LEPTONS,

(c) Quark-Quark scattering x-section.

$$\frac{d\hat{\sigma}}{d\hat{t}} = \frac{2300\,\text{mb}}{s(-t)^3}$$

Try to fit all correlation experiments with no new parameters.

Last transparency

"Feynman-Field Jet Model"

WORK IN PROGRESS

a) More detailed Calculations

b) Theory of $q \to$ hadron cascade

FUTURE.

Protons & baryons at high $P_\perp$.

Single γ's at high $P_\perp$.

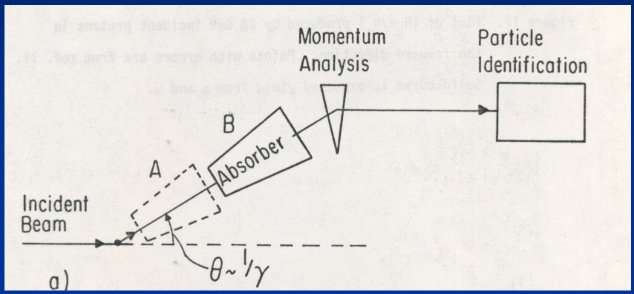
Nuclear targets.

Are we really in trouble from Ptransverse of quarks?

Unify theory to that of main collision at low $P_\perp$.

Direct Muon Production

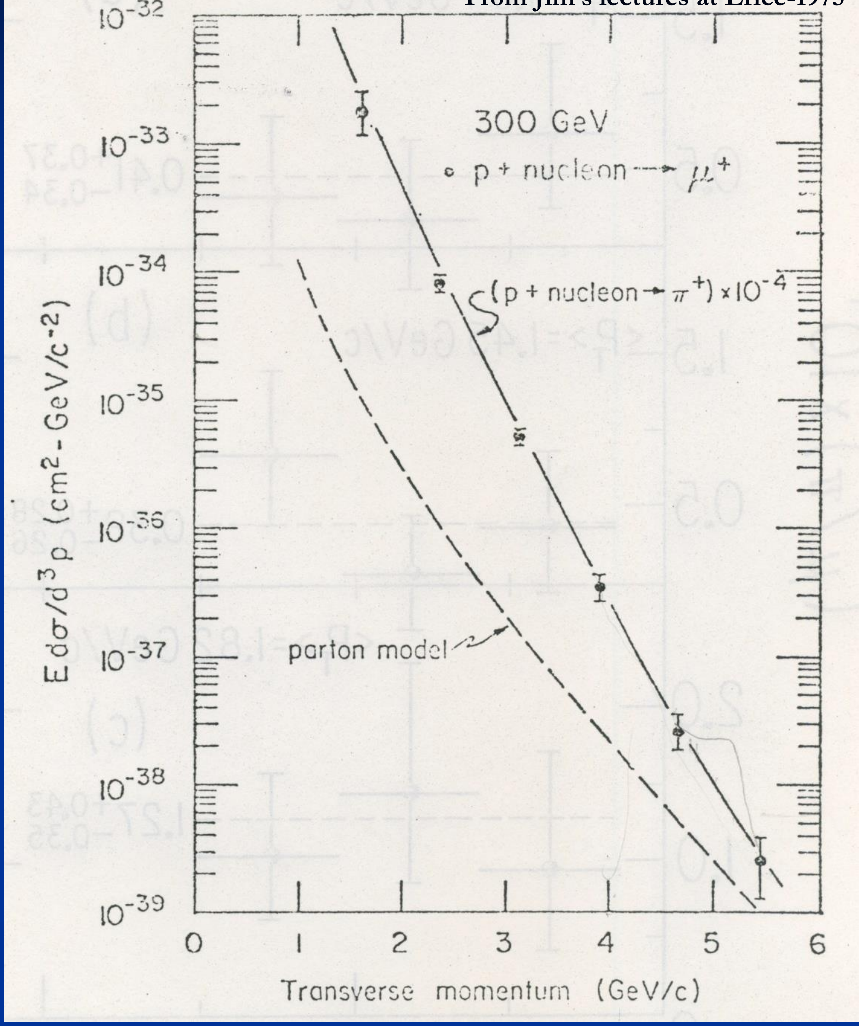
Pions and Kaons decay into muons- large background



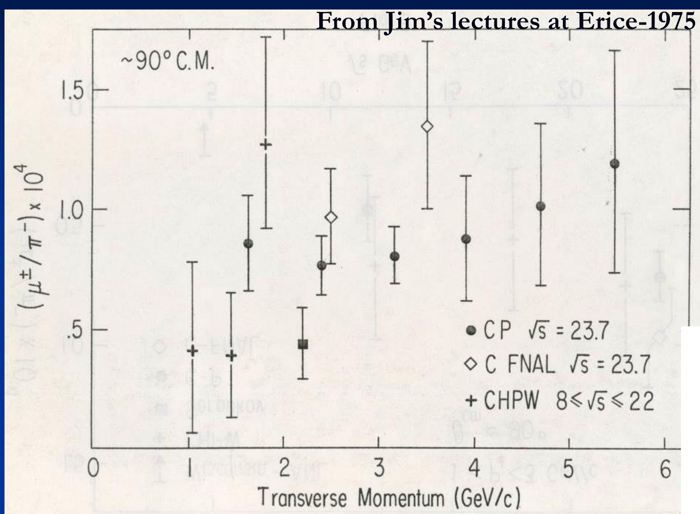
Use a pair of movable 'shutters' to absorb pions, kaons, protons…

2 points allows extrapolating to zero lifetime- `aka direct'.

From Jim's lectures at Erice-1975



Direct Muon Production- July 74



Ratio of mu-to-pi. Note CP precision

Publication in PRL

(only 1 of 3 times I ever saw Jim angry- actually 1 of 2..
Stories over dinner or by request)

VOLUME 33, NUMBER 2 PHYSICAL REVIEW LETTERS 8 JULY 1974

Observation of Large-Transverse-Momentum Muons Directly Produced by 300-GeV Protons*

J. P. Boymond, R. Mermod,† P. A. Piroué, and R. L. Sumner
Department of Physics, Joseph Henry Laboratories, Princeton University, Princeton, New Jersey 08540
and
J. W. Cronin, H. J. Frisch, and M. J. Shochet
The Enrico Fermi Institute, University of Chicago, Chicago, Illinois 60637
(Received 8 May 1974)

We have observed muons produced directly in Cu and W targets by 300-GeV incident protons. We find a yield of muons which is approximately a constant fraction (0.8×10^{-4}) of the pion yield for both positive and negative charges and for transverse momenta between 1.5 and 5.4 GeV/c.

In this Letter we report on the observation of muons produced directly in nuclear targets by 300-GeV incident protons. Study of muon production at high transverse momentum was originally motivated by the search for the intermediate vector boson. Early experiments were carried out at the Argonne zero-gradient synchrotron[1] and the Brookhaven alternating-gradient synchrotron[2] with negative results. More recently, several experiments have shown evidence for the direct production either of single muons[3,4] or of muon pairs[5] in nucleon-nucleon collisions. Extensive theoretical work[6,7] suggests that collisions of pointlike constituents of the nucleon would result in the direct production of muons.

absorber, (2) W absorber inserted only, and (3) Fe absorber inserted only. Runs were made at 10-GeV/c intervals between 20 and 70 GeV/c corresponding to transverse momenta ($P_\perp$) from 1.5 to 5.4 GeV/c. At 20 and 30 GeV/c, data were also taken with both absorbers in the beam as a consistency check. To verify that the muons were associated with the target, we also took data with the target removed for the three principal conditions at 20, 30, and 40 GeV/c.

For each absorber condition we measured the ratio of muons detected at the end of our apparatus to pions of the same charge detected by the apparatus when the absorber was absent. Two counter telescopes which looked at the target at

The 'Cronin Effect'

We had nuclear targets- but wanted cross-sections on protons (nucleon)- extrapolated from 3 nuclei to A=1

Atomic-Number Dependence of Large-Transverse-Momentum Hadron Production by Protons*

L. Kluberg,† P. A. Piroué, and R. L. Sumner
Department of Physics, Joseph Henry Laboratories, Princeton University, Princeton, New Jersey 08540

and

D. Antreasyan, J. W. Cronin, H. J. Frisch, and M. J. Shochet
The Enrico Fermi Institute, University of Chicago, Chicago, Illinois 60637

(Received 20 December 1976)

We have measured at Fermilab the production of hadrons at ~90° in the c.m. system as a function of incident proton energy, atomic number A of the production target, and the transverse momentum $p_\perp$ of the produced hadron. The A dependence of the production cross section of the hadrons can be described by a function $A^{\alpha(p_\perp)}$, where the power α rises with $p_\perp$. At $p_\perp$ ~5 GeV/c, α is ~1.1 for $\pi^\pm$ and K^+, and ~1.3 for p, $\bar{p}$, and K^-. The energy dependence of the power is also measured.

In an earlier paper[1] we reported on the atomic-number (A) dependence of hadron production at large transverse momentum ($p_\perp$). Similar data have also been reported by other groups.[2,3] These results were surprising because the A dependence of the hadron yield, when fitted to a form $A^{\alpha(p_\perp)}$,

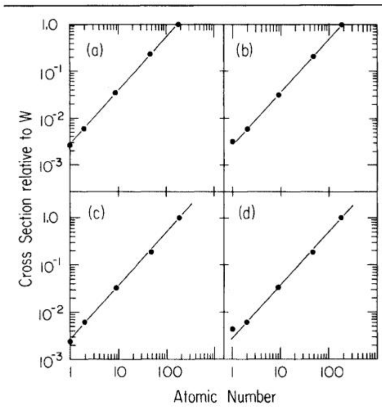


FIG. 1. The invariant cross section for π production relative to tungsten for various atomic numbers at 400 GeV; (a) π^- at $p_\perp = 3.85$ GeV/c, (b) π^+ at $p_\perp = 3.85$ GeV/c, (c) π^- at $p_\perp = 5.38$ GeV/c, (d) π^+ at $p_\perp = 5.38$ GeV/c. The errors are smaller than or equal to the size of the points.

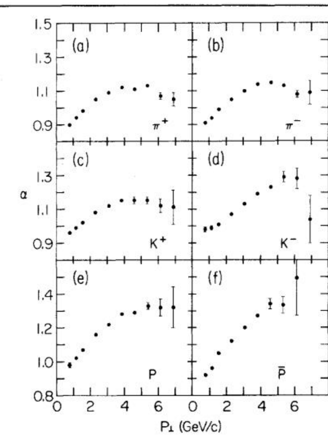


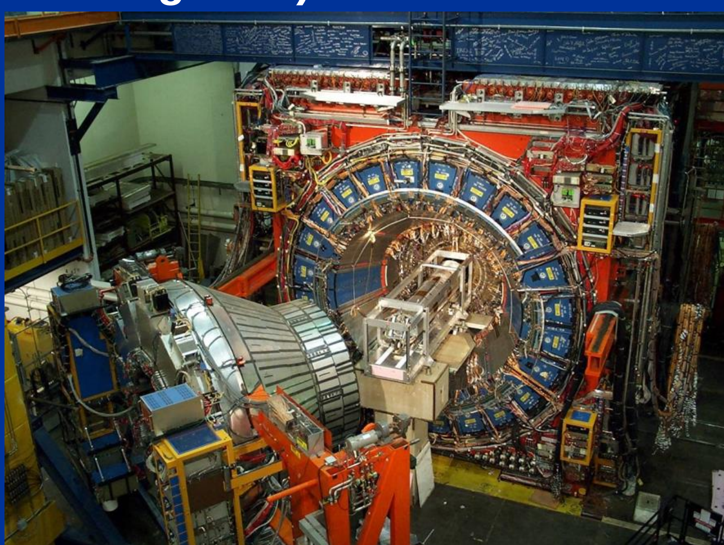
FIG. 2. The power α of the A dependence of the invariant cross section vs $p_\perp$ for the production of hadrons by 400-GeV protons; (a) π^+, (b) π^-, (c) K^+, (d) K^-, (e) p, and (f) $\bar{p}$. Unless indicated otherwise, the errors are smaller than or equal to the size of the points.

Found a surprising effect- the `Cronin Effect'- stronger dependence than $A^{1.0.}$ Turns out to be scattering in the nucleus- now a major industry in the nuclear community.

“ Colliding Beam Experiments Department”

Fermilab (not Jim’s Dept.) still a mess a year later…

But, with Dennis Theriot and a really good crew derived from the group… (Dennis is a much unsung hero):



Fermilab

Colliding Detector Facility Meeting Minutes

September 15, 1978

Present: H. Frisch, M. Peshkin, A. Tollestrup, J. Rhoades, J. Walker, B. Diebold, L. Holloway, R. Loveless, I. Gaines, T. Collins, T. Rhoades, P. Limon, C. Ankenbrandt

Alvin announced that there will be a review of the entire colliding beam possibilities at Fermilab in the second week in November. In order to present this Group's work in a coherent fashion at that time, Alvin asked that each Group Leader have a written report on his section by October 1, 1978.

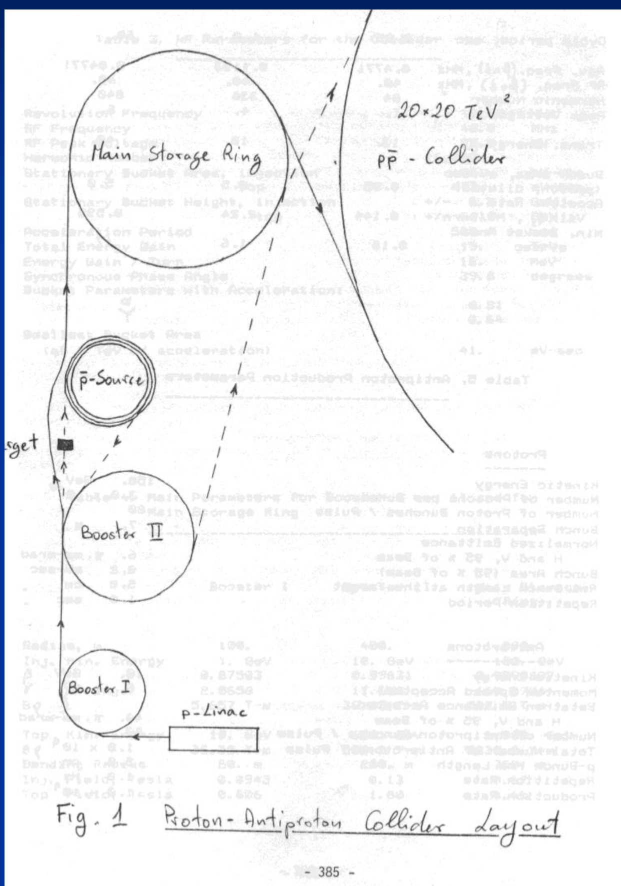
A very lively discussion followed on which of the several options (pp, $\bar{p}p$ in MR, $\bar{p}p$ in Doubler, etc.) was the best one to push here at Fermilab given CERN's $\bar{p}p$ program and their much larger financial commitment. Alvin appointed three groups to study various questions since the answers were not clear to those present at this meeting.

A.	I. Gaines, B. Diebold:	Monte Carlo pp interactions to determine if the unequal energies present any problems for the detector we have been considering.
B.	R. Loveless, T. Collins, S. Ecklund:	Squeezer magnets if no pre-bending.
C.	P. Limon, H. Frisch, C. Ankenbrandt:	$\bar{p}p$ luminosity estimates.

RL:clc

Jim’s initiative led to the (now long-standing) involvement of Carla Pilcher, Mel Shochet, and myself in CDF and collider physics.

The Path Not Taken: LHC, ILC, and the pbarp SSC Option (mrcfly brief)



Jim had immense wisdom and vision, and the remarkable ability to apply his economical elegant style even to the largest projects. The idea was to go more adiabatically, and use resources at hand (Fermilab), and get to 40 TeV with pbarp and only one ring as a step along the way. It's a pity that we didn't start this way

(Aside- I was told in the Japanese Embassy in DC that Japan would have been willing to pay for the 2nd ring – - Jim's instincts were so on target.).

The pbarp SSC Option

2/9/84

WORKSHOP ON $\bar{p}p$ OPTIONS FOR THE SUPER COLLIDER

PROGRAM

Sunday, February 12

Registration at Hilton Inn	6:00 PM - 10:00 PM
Reception at Hilton Inn	8:00 PM - 10:00 PM
Meeting of Organizing Committee Working Group Leaders and Speakers at Hilton Inn	9:00 PM

Monday, February 13

Registration at Oriental Institute		9:00 AM - Noon
FIRST PLENARY SESSION, Oriental Institute (Breasted Hall) 1155 E. 58th Street		9:30 AM - Noon
Opening Remarks	Jim Cronin	
Speakers:		
"Views on a $\bar{p}p$ Super Collider"	Carlo Rubbia	
"Physics Signatures in Hadronic Collisions"	Frank Paige	
"Present Status of the SSC"	Maury Tigner	
LUNCH		12:00 - 1:30 PM
SECOND PLENARY SESSION, Oriental Institute		1:30 PM - 4:00 PM
Brief Talks by Working Group Leaders		
Speaker: "Vacuum Deformation by Heavy Particles"	Frank Wilczek	
Adjourn to Fermi Institute, 5640 S. Ellis Avenue		4:00 PM
Coffee in RI 480		4:00 PM - 4:30 PM
Organization of Working Groups		4:30 PM - 6:00 PM
OPEN HOUSE - after dinner - home of Jim Cronin 5825 Dorchester Ave.		8:00 PM - 10:00 PM

Tuesday, February 14

Working day (offices and seminar rooms open from 7:30 AM to midnight).
Research Institutes

Wednesday, February 15

Working day	Research Institutes
RECEPTION for Workshop Participants hosted by Enrico Fermi Institute at the QUADRANGLE CLUB. 1155 E. 57th St.	5:30 PM
BANQUET at Greek Islands Restaurant (Board buses at 1155 E. 57th St.)	7:30 PM

Thursday, February 16

Working day	Research Institutes
Coffee in RI 480	4:00 PM - 4:30 PM
Physics Colloquium: Eckhart 133	4:30 PM

"The Fly's Eye: Cosmic Ray Detector"
George L. Cassiday, Jr.
University of Utah

Friday, February 17

Summary Talks (Goodspeed Hall) (program to be arranged)	9:00 AM - 4:30 PM

1984 Workshop Initiated by Jim

The pbarp SSC Option

J.W. Cronin ①
Feb 12, 1984

Workshop on $\bar{p}p$ Options for the Super-Collider

Goal of the workshop:

To consider all aspects which concern the relative merits of pp and $\bar{p}p$ hadron colliders. Considerations should concern physics, detectors and accelerator design.

Some specific questions are:

1. For a $\bar{p}p$ collider what is a realistic maximum luminosity? What is its time structure.

2. For physics thought to be independent of pp or $\bar{p}p$ what luminosity is required to have a reasonable rate for the various processes

3. Can detectors be envisaged that can observe the processes considered above.

4. What are the physics distinctions between pp and $\bar{p}p$? Are there cases where $\bar{p}p$ is better than pp, or vice versa.

5. What physics would benefit from both pp and $\bar{p}p$ capability?

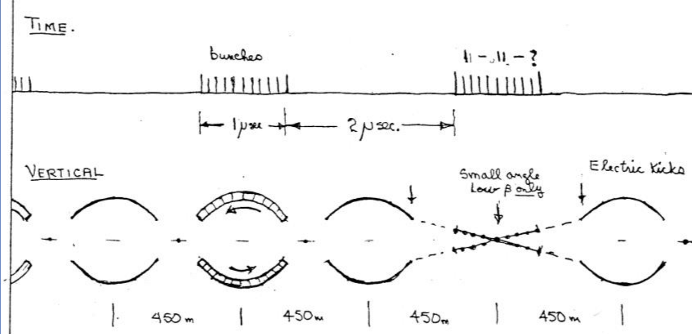
6. If one builds a pp collider what are its capabilities for $\bar{p}p$ collisions?

Discussion, debate and answers to these questions should be consisely documented in the proceedings. The working group leaders must provide the proper interaction between theorists, experimentalists, and accelerator physicists to accomplish these goals. It is hoped that a lively and provocative workshop will take place.

"Goals of the Workshop"

Hand-written detailed technical design- Jim's style as a leader (as opposed to Feynman's def. of a "position of responsibility")

10^{32} in each of 6 INTERACTION REGION.



For $\Delta\nu = .003$, every P bunch is 2.6×10^{10} P's. ($\epsilon = 5\pi$)

$\bar{P}$ bunch varies 1/no. of bunches TOTAL 10^{13} $\bar{P}$'s (102 batches)

11 bunches/batch → 100 nsec space → 2.7×10^{25}/hit for $\mathcal{L} = 10^{32}$ → 6.5×10^{9}/bunch

SINGLE RING ~6 Tesla magnets - 91.800 Km (306 μsec)

Same size as one P-P ring. Separation is small and easy.

I.R. 1m β^* ± 20 m. space.

The pbarp SSC Option

Jim several times was so right on major directions/facilities at critical junctures in the science:

– Fermilab Collider (went well after some initial "screwing around"

- SSC (not so much)..

Picture from the Workshop Proceedings

Enrico Fermi on Fundamental Forces

Fermi in his 1951 Yale Lectures:

" Perhaps future developments of the theory will enable us to understand the reasons for the existence and strength of these various interactions…."

As you know, Jim admired and studied Fermi. There is a wonderful, but not unexpected, strong intellectual connection between Jim's pioneering work on hard-scattering at the shortest distances and the questions Fermi laid out for us 65 years ago:

E. Fermi
Elementary Particles,
Yale Press, 1951

12. *ELECTROMAGNETIC AND YUKAWA INTERACTION CONSTANTS*

In the preceding chapter six interaction processes have been discussed. They do not cover all possibilities. There could be additional interactions among the elementary particles, and besides there are particles whose existence is either known or suspected which we have left out of consideration because too little is known of their properties. For each of the six interaction processes of Chapter II a constant has been introduced that determines its strength. Three of them have the dimensions of an electric charge and three have the dimensions of energy × volume. The first three are

EM

e—the elementary electric charge that determines the strength of the electromagnetic interaction.

STRONG

e_2—the interaction constant of the Yukawa theory determining the strength of the interaction between pions and nucleons.

WEAK

e_3—the constant of an interaction that has been postulated to act between pions, muons, and neutrinos, which could be responsible for the spontaneous decay of the pion.

The three constants with dimensions energy × volume are

WEAK

g_1—the interaction constant of the beta processes.

g_2—an interaction that has been postulated to act between muons, electrons, and neutrinos and which could be responsible for the spontaneous decay of the muon.

g_3—the interaction constant of a hypothetical process similar to the beta interaction except that the electron is replaced by a muon.

Perhaps future developments of the theory will enable us to understand the reasons for the existence and the strength of these various interactions. At present, however, we must take an empirical approach and determine the values of the various constants from the intensity of the phenomena that are caused by them. In Appendix 5 some of the possible relationships between various constants are discussed.

STILL NAME OF THE GAME!

I'd like to return to 1974- the Multi-Hole Spectrometer

What is was like to work with Jim

NAL PROPOSAL No. 325

Scientific Spokesman:

J. W. Cronin
Enrico Fermi Institute
University of Chicago
Chicago, IL 60637

PH: 312 - 667-4700

STUDY OF DI-MUON PRODUCTION AT HIGH TRANSVERSE MOMENTA

J. W. Cronin and Henry Frisch
University of Chicago

P. A. Piroué
Princeton University

Fermilab E325 Proposal June 1974

We have given further consideration to the study of high mass dimuon events. In the original Proposal 325 (E-300 Addendum), we suggested using the east end of the pit being built for Adair (E-48). (We assume the reader has also read the E-300 Addendum). At the time of writing this note (August 1, 1974), the exact location of the pit is still uncertain. In addition, we have done more detailed calculations on muon background and find that a wide detector transverse to the muon direction is far from optimum. For the small angle muons there is insufficient thickness to suppress the μ background from π and K decay, while at larger angles, the desired muons do not have sufficient range. Thus,in this note we propose an alternative scheme which, on the one hand, is an escalation, but, on the other hand, is far superior and sensibly designed.

One should recall that E-100 was the first experiment at FNAL to successfully measure direct muons. Our results are now published (Phys. Rev. Letters 33, 114, 1974). We are most eager to continue this work in a modest but significant way. We realize that there are many muon experiments approved or proposed. We are still behaving as scientists, trying to follow up on a discovery with a reasonable next step, given the limitations of our location and apparatus.

It is well known that the invariant π and K production cross sections are functions only of $p_\perp$ in the central region ($x = 0.$) Thus, if one builds a detector parallel to the proton beam, the decay muons must penetrate a fixed amount of transverse shielding independent of angle

There's a subtext

Paper and pencil detector design

The detector is a set of 10 6' x 4' x 1' liquid scintillation counters, each placed in a 4' diameter 17' deep hole. The 10 holes are placed along a line 19' displaced from the incident beam direction. One has 15' of transverse earth shielding which corresponds to a 1.5 GeV/c cutoff in transverse momentum. The holes, which begin at 140' from the target, increase in distances from another in geometric progression with a factor 1.166 in distance from one to another.

Detector design details

Such a device cannot measure the $\mu\mu$ mass accurately. It can however measure the minimum mass which is given by $M_{\mu\mu} \gtrsim (p_\perp^s + 1.5)$ GeV/c^2 where $p_\perp^s$ is the transverse momentum setting of the spectrometer and 1.5 is the transverse momentum cutoff of the MHS. If the RMS transverse momentum of $M_{\mu\mu}$ is less than .5 GeV/c, then the dimuon mass resolution is $\Delta M_{\mu\mu}/M_{\mu\mu} \sim 0.1$.

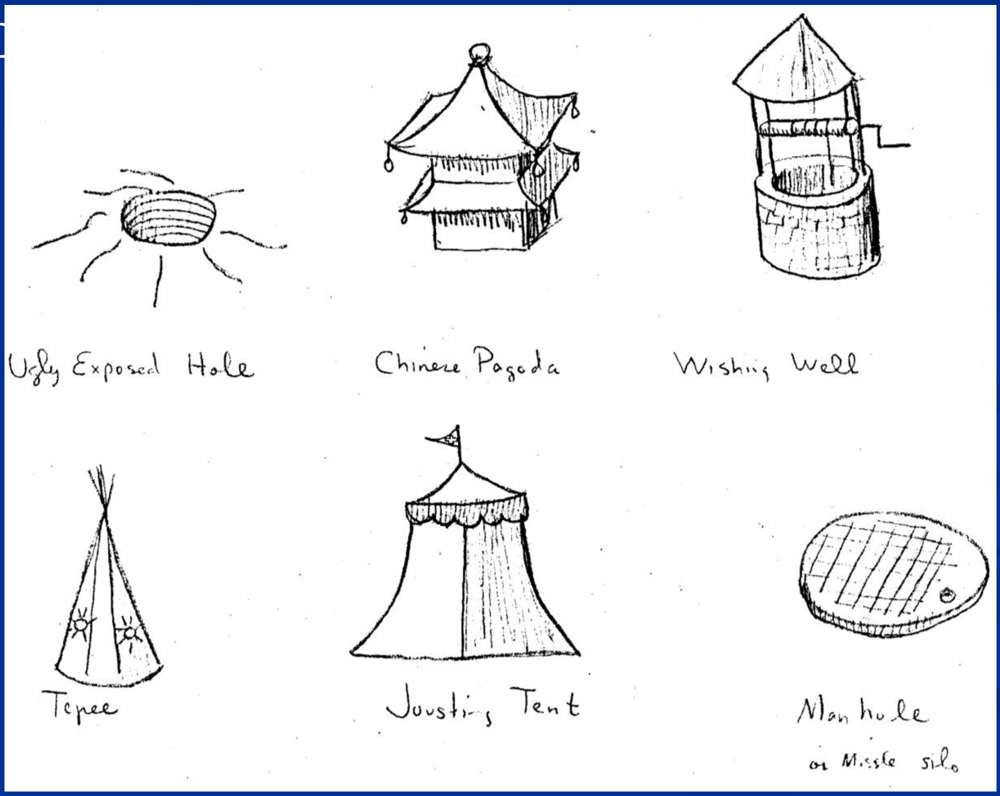
Performance

Practical details

We have consulted a contractor (Case, Roselle, Ill.) for the price of holes. The contractor stated \$1500/per hole for 10 4' diameter 17' deep holes lined with corregated steel. The additional cost to place a cover on each hole and a Sears-Roebuck sump inside may cost NAL \$500/hole. Our detector cost is estimated to be \$1000 each (4 665PM's, 24 cu. ft. liquid scintillator, and a rough aluminum tank.)

Class

In order to make the MHS less unsightly, we have considered more decorative covers which may enhance the beauty of the site. Figure 3 shows several disguises which might be appropriate.



Taking Stock

Jim did all the right things at the right time- wonderful taste, sense of discovery, minimalist experimental style

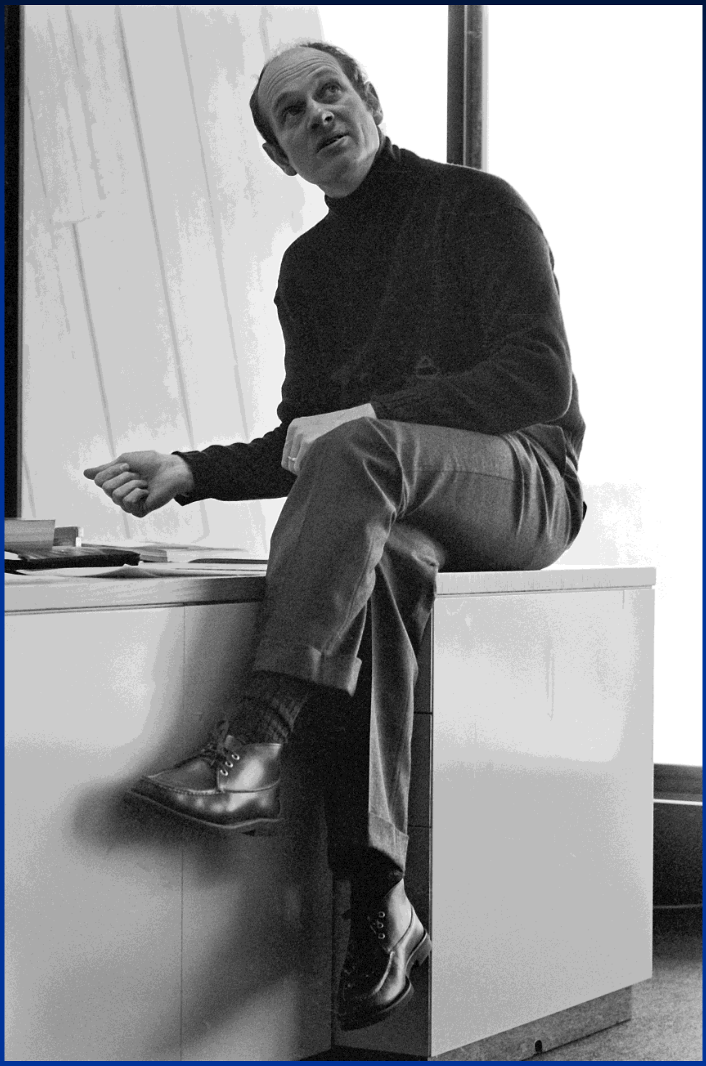
E100/325- the high-Pt 'investigations' at Fermilab were in the thick of the development of today's parton model- power-law behavior of cross-sections (point-like scattering), fragmentation of partons (PT^{-8} vs PT^{-4}) , direct muons (charm, June 1974 vs Nov), Cronin Effect.

Jim was instrumental in the start of the collider program at FNAL- at CDF alonge discovery of top, precision W and Z measurements, precision b-quark measurements, development of tools, hardware, ….

Jim was right on target on the SSC- if he had prevailed we would be running today at 40 TeV in pp with 2 rings.

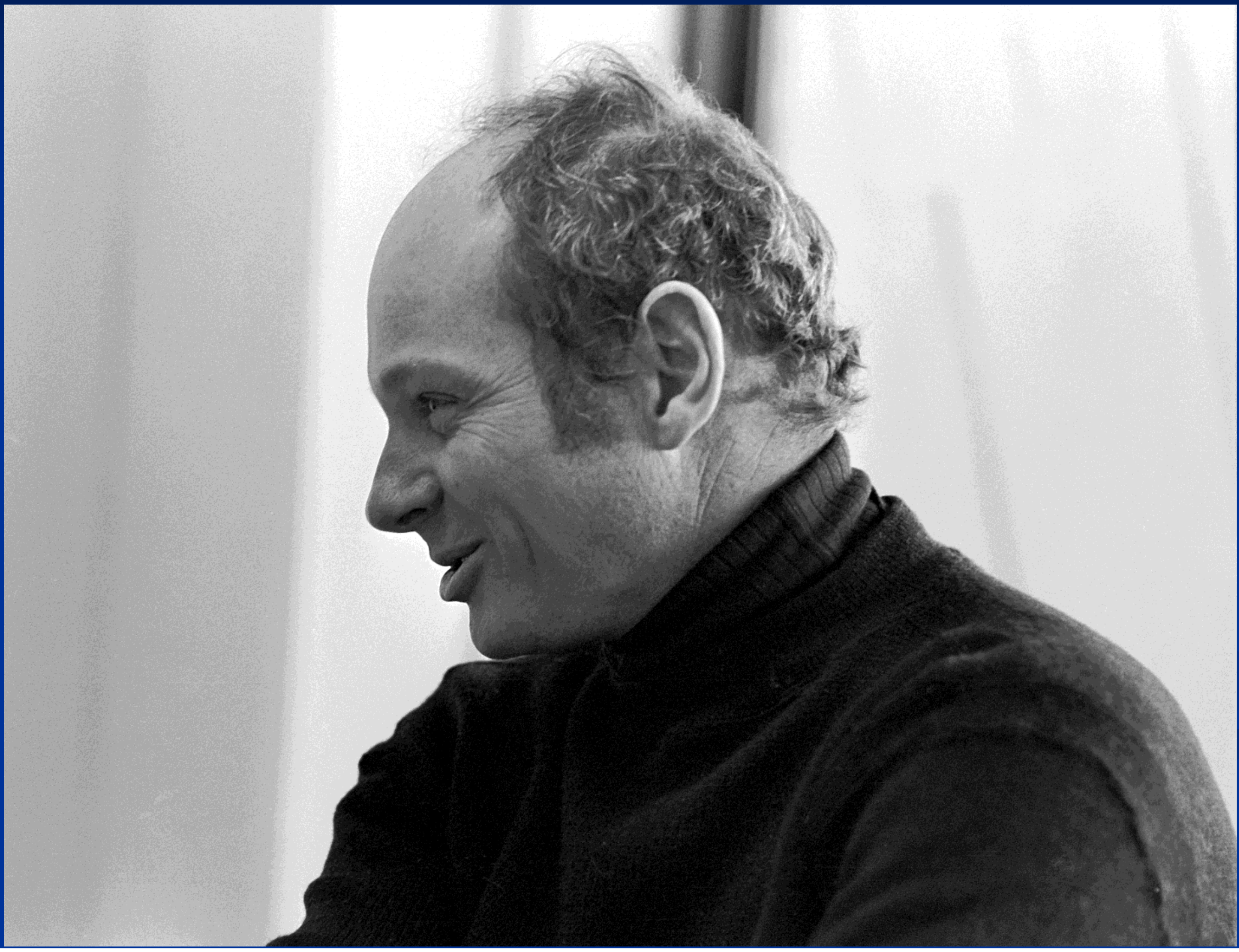
Jim left a large legacy in protégé's- we owe him big-time.

Jim in 1977-



Hard-parton scattering and JWC

1977



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

Talk to Graduate Students at New Perspectives 2001
Fermilab, June 13, 2001

Henry Frisch
Enrico Fermi Institute, University of Chicago

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.

June 8, 1999
H.J. Frisch

The Three Kinds of Light

1 What is Science?

Answer the following four 'Optics' questions correctly and you can go on to the next round of the Academic Decathlon to represent your school at the forefront of science:

Question 1: What are the three kinds of light?

Question 2: What is it called when an object loses its electric charge?

Question 3: Light is reflected at a _____ angle from a rough surface (fill in the blank).

Question 4: What is the opposite of an electric motor?

These are 'Optics' questions from the workbook for the high school Academic Decathlon of last year. I was asked by C., a young friend who was a student at a large (>4000 students) public technical high school in Chicago, to help their team prepare. Optics was baffling them, and their coach was an English teacher, and he was having a hard time with the questions as well. Science (sic) seemed illogical and arcane; they felt it was obviously a different world, and one that they were not cut out for (these were bright kids, as most are).

2 C., An Eighth Grader

I had met C. through Lourdes Monteagudo, the Director of the Teachers Academy of Mathematics and Science (TAMS) in Chicago. Lourdes had just come back from an 8th-grade graduation in a public school in one of the roughest neighborhoods in Chicago, and where she had been bowled over by the speech of the valedictorian. I asked his name, and hired him for the summer to work with us on the CDF experiment at Fermilab (of top quark fame- in fact that was what I was working on at the time).

C. comes from the far South Side in Chicago, from a neighborhood known best, unfortunately, as the home of ‘Yummy’, the 14-year old who shot an 11-year old as a contract killing for his older gang leaders. I would often find him in my lab late at night doing homework; he said it was ’easier’ to go home after everybody at home was asleep.

In high school C. decided on a Science Fair project based on work he had done the previous summer with me on cross-talk in the long multichannel cables we use in the trigger electronics for CDF. The cables are custom, with a relatively high impedance and 10 sets of twisted pairs. The question was how much cross-talk there was between individual pairs. One brief story- I left C. alone with a modern digital Tektronix scope, asking him to see if he could figure out how it worked (I’m much happier with an old analog 454- I never can tell with the new ones whether I’m really looking at the signal or accessing storage and seeing an old one). I came back half an hour later to see how C. was doing, and he said ’fine. I found the instruction manual on the web and printed it out, and I think it’s pretty easy.’ (He had a trace, and the scope was triggering on a signal happily).

But now to the second point. When C. presented his project to the science teacher, the teacher said ’What do *you* know about cross-talk?’ So C. then gave the teacher his written report on cross-talk from the summer, and the teacher said ’No kid should know that much about cross-talk’, and wouldn’t let him submit the project or be in the Science Fair. As C. said, the teacher said ’I had an attitude’ (Chicago-ese for uppity).

3 D.

A second anecdote about the real problems bright curious kids have in urban schools. Every year I used to invite the local 4th graders to the University where I’d put on a ‘Lecture Demonstration Spectacular’. Hellmut Fritzsche, when he was Chair of the Department, had instituted the Spectacular as a way of showing the Faculty the depth and breadth of the Departmental lecture demonstrations, which are the product of a first-rate staff. I had done this in a number of years, and so it seemed natural to repeat the show for my kids’ class. It was so popular that I kept it up for a number of years after my kids had left the 4th grade.

One of the demonstrations was to roll two cans of soup, one pea soup, and the other beef bouillon, down an inclined plane. I’d tell the kids about the soup, and then ask them which would roll fastest. Usually the majority voted for the pea soup, as it was ‘heavier’. We’d then do it, and lo-and-behold the bouillon would win.

I’d then show them that it didn’t have anything to do with heavier- we’d take a wooden hoop and a wooden disk of the same diameter, and I’d show that they weighed the same on a simple balance. I then asked which would go faster, and this was a harder question, usually with no clear consensus. On doing it, the disk would win handily.

One year, I did this early in the Spectacular, and said ’see- it’s like the soup’, as usual. Forty-five minutes later a little pudgy black kid in the front row raised his hand, and said 'I have a question. You said that the disk won because it was like the soup, but the pea soup is like the disk and the bouillon is like the hoop. I don't understand what you meant.' A teacher rushed up and said 'D., be quiet', and then apologized to me (in front of the kids): 'He's a trouble maker, Professor Frisch, don't mind him.' D. had sat there for 45 minutes thinking about this, and then decided to ask. He clearly had an attitude. He also wasn't into science.

4 To Be Continued

This has been anecdotal, to use a favorite word of my UC colleagues in the social sciences. However there's one important fact: we now have solid assessment numbers from TAMS showing that there are methods to change how math and science are taught that work on the scale of the 400,000 students in the Chicago Public School system. And so to make it short, I would like to list some of my more general observations.

- I like 3rd and 4th graders. They are still curious, and they have the real scientific instincts for thinking, asking, testing, and observing. It hasn't yet been beaten out of them, and the fear teachers have of science hasn't yet been transmitted to them. Identifying the really talented ones early can by done; they're there, and one only has to look to find them. K-8 is where kids are programmed to do well or poorly in math and science; high school is important, but cannot succeed if the elementary schools fail.

- Keeping kids from being done in as they grow older is much harder; it's not a question of curriculum alone (although having a curriculum teachers are comfortable with is an essential element- take a look at Howard Goldberg's wonderful TIMS modules). A critical element is the occasional bad teacher. Kids are proud, and, from watching my own, won't play a game in which they're set up to lose. One year of a teacher who is on a kid's case can be enough to undo the work of many good teachers. Getting rid of the teachers who attack a curious kid's self-esteem is one of the most important aspects of good urban math and science education.

- What science *is* is a mystery to most teachers. In Chicago there aren't specialists; there are 17,000 teachers who teach math and science on a regular basis. There is a heavy emphasis on memorizing names; science is seen more as a body of knowledge than a mix of curiosity and method. Teachers consequently avoid it, and their uncertainty is transmitted to the kids. In the early grades, quantitative work as in the TIMS program mixed with curiosity-driven questions can really turn kids on. A physics department can have a big impact on local schools with a rather small investment by each person if grad students, postdocs, and faculty make regular visits. There are a lot of us.

- In a presentation to the American Physical Society Council one evening at an APS meeting it was stated that 'We do not know how to deal with the problems of math and science education in the big urban schools.' I truly believe that we *do* know many if not most of the ingredients, and have working models for how to confront and solve the problems. We need to move as a community away from a narrow view of these problems to dealing with them on a large scale.

It made a big difference to the kids at the high school I visited to be told that there are *not* three kinds of light, and that it wasn't them, but the questions that were 'stupid'. Even more important to them, on the first visit I had brought along an undergraduate from UC who had been in the Academic Decathlon, and she told them something even more basic to getting good grades in 'science' classes. This was that each of these questions had the answer in the workbook, and that the way to study for the Decathlon wasn't to think, but to make flash cards that gave the answers (she also talked with the girls in the class at length, telling them that women can be scientists, and can go to college and major in science. They had never met a scientist before.).

The correct answer to 'What are the three kinds of light?' is: 'Neon, fluorescent, and incandescent'. It's in the book. You can look it up. In fact, you'll have to- you'll never figure it out on your own. [1]

[1](Answers I: My answers to the 4 optics questions were, in order 1) I have no idea- there's only one kind of light. 2) A shame...?; 3) again, no idea- it reflects at all kinds of angles (the kids in fact said this to me, and were puzzled by the question's implication that there was a single word that described this); 4) A horse? (again, no idea)

Answers II: The 'correct' (sic) answers were: 1) neon, fluorescent, and incandescent (yes, the question was worded as above); 2) a discharge, 3) A 'diffuse' angle, and 4) an electric generator.

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May 14, 2000

Editor
Chicago Tribune

To the Editor:

In regards your editorial of Saturday, May 13, 'Keeping College Customers Satisfied?', I'd like to offer a contrary opinion. The concern that 79% of grades of the Univ. of Ill at Champaign/Urbana were B- or higher implies an eroding of standards is based on the (dubious) assumption that the average grade in any class should be a C. I've taught college for almost 30 years now, and nothing gives me more pleasure in teaching than to have my class do really well. Good teaching, good students, the excitement of learning new things- why should a student who really knows her stuff be given a C, or, in a class of anal-retentive hot-shots (read pre-meds) even lower (if you're going to have a C average, there better be a fair number of D's and F's)?

E.E. Moise, the James Bryant Conant Professor of Mathematics and Education at Harvard, pointed out the fallacy of grading 'on the curve' to a class I took from him. Selective schools, such as the ones you cite, pick above-average kids. This cohort represents the upper part of the 'bell curve' that often is used in grading 'on the curve'. There is no mathematical basis in distorting the curve of the ability and achievement of these kids into another, new, bell curve. One could either make the average the initial average, in which case, yes, more than 50% of the kids would get above a C. Alternatively the average could be based on only the upper part of the curve that we picked, in which case the average should be a D or even a D- (to understand this, draw a picture of a bell curve, erase the lower half, and take the average of the remaining piece). Either way, a blindly enforced C average makes little sense.

The kids in the schools you cite are placed with other interesting kids and teachers who are excited about their fields of research, and who are (in general) deeply dedicated to innovative creative teaching. Why shouldn't they do well? Taking the attitude that we're going to filter them out is deeply wrong, I believe. I teach Physics, and believe I can teach real unadulterated Physics to (almost) anybody, much as ski instructors claim to believe that anybody can learn to ski. The challenge for me is to present the material clearly and carefully so that it's understood, developed, and used. My experience is that with effort and time (lots of it), even the least mathematically inclined can learn Special Relativity in one quarter, for example, at the level that most Physics graduate students understand and use it. If a student majoring in Fine Arts or Near Eastern languages can solve the standard graduate-level problems in Special Relativity (many due to Einstein, but requiring only simple math), should they get a C or lower?

The other side of the coin is that there are indeed classes in many schools where the average is *below* a grade of C. I haven't yet seen an editorial bemoaning 'grade deflation' in the city's schools indicated by the lower-than-average performance on standardized tests. To be consistent,the same folks who want a C average at UI should lobby for a C average in our elementary schools on the IGAP tests.

The point is that there are absolute standards, and grading on the curve is a relative standard that makes little sense. Elementary school kids should know how to read, rite, and rithmetic; kids in college should master the material in their classes. These are largely absolute standards- the standard is set by a relative comparison to what we've come to expect as good performance, not by the average in the class.

The tension over how to grade at a university is long-standing. My dad taught Physics at MIT, and one year taught the huge first-year course taken by almost every first-year student (MIT is largely a technical school, so almost everybody takes physics). The title of the course was 'Maxwell's Equations', the four fundamental equations governing the behaviour of electricity, magnetism, and the propagation of light, radio, and TV waves. He put the question 'Write down Maxwell's Equations' on the final exam; he gave an F to any student who got them wrong. Unfortunately more than 800 students got them wrong. I was a kid- I remember obscene phone calls, and many phone calls from Deans and others who were faced with most of the college repeating their freshman year (Physics is a prerequisite for many other courses). Was he right? Should the average have been a C? He remarked ' It's possible I'm a lousy teacher, but certainly in a course with the title of 'Maxwell's Equations' the least you can ask is that a student be able to write them down by the end of the semester. I'm with him. Absolute standards (well, tempered by judgement and experience). And if everybody does really well on a hard exam, give them good grades! (For those curious how it ended up- he finally buckled under and gave a second exam. Not surprisingly, the second time everybody knew Maxwell's Equations. He said 'well, at least we didn't have the normal spring riots over the food this year'.)

Lastly, I can't resist commenting on your title 'Keeping College Customers Satisfied'. The notion of a college student as customer is shallow rancid hog-wash. The *parents* of the students are customers- no question. I just finished putting two kids through college, and I was treated like a customer– paid my bills, asked no questions, hoped for the best. But I hoped and trusted that my kids were treated not as customers, but as bright, funny, interesting young people, with a life ahead of them. They chose big, good, public colleges with a strong research faculty, where they could meet other interesting young folk and interesting dedicated faculty. I hoped that their strengths would be strengthened, and their weaknesses, most of which curiously look much like my own, and which in spite of many years of trying I couldn't change much, would be finally attended to. Rough edges get smoothed, horizons broadened; a good school instills a confidence in being able to learn new things that a parent is no longer able to impart to a teenager. In contrast, customers of HMO's, the airlines, the telephone companies, are treated differently. I don't want my kids treated as customers, any more than I want my doctor to treat me as a customer (in both cases I'm not interested in repeat business). 'The Student is Your Customer' is a slogan for the mail-order degree factory, not for any school with teachers in it. We should stamp it out, and those who mindlessly promulgate it should move to professions other than education. Here's to old-fashioned standards...

Best wishes,

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Version 1.0
May 5, 2017

Topics in Pedagogy

1 Introduction

The common reactions from non-scientists on learning that I'm a physicist reflect, I believe, mistakes we make in how we teach physics. I am interested to know if there are commonalities with your experience in other fields.

I am also interested to know if others have found a lack of traction with their colleagues for similar ideas; the following have been hobby horses for a long time. These issues still bother me, though I've by-and-large given up. However, I thought it would be fun to talk about them, and maybe I'd learn why some of my colleagues seem unable to hear them. And, I may be wrong.

2 Basic Pedagogy

Teaching in the Native Language; Physics as a Language Course

No subject should be taught in translation. Physics in particular has a precise language (mathematics), while in translation ('English') is confusing and often deeply incorrect. The most thoughtful students are often the ones most troubled by 'English' translations of physics techniques and principles that would be clearly understood in the native language.

Less is More: Coordinated Curricula

The Physics Department Curriculum is over-stuffed; there are too many required courses, some essential topics are barely covered or not covered at all (Thermodynamics, Optics, Special Relativity), and individual courses have curricula that do not fit in a quarter. We should instead teach commonalities and the necessary 'tool kit' early in the curriculum, so that one can then go fast and deep in the following courses [1].

Implicit Assumptions and Consequent 'Proof by Intimidation': 3 Examples

Physicists make implicit assumptions in posing problems, and then are troubled that students and the public seem scientifically illiterate. Some much-cited examples in which we (physicists) are talking about an ideal situation and the lay person (Phy Sci student) isn't:

1. "The public thinks that a child falling off a swing falls straight down" (they usually do);
2. "The public thinks that a ball rolling down a curved ramp will continue to curve on the floor after it leaves the ramp". (tennis balls often do);

[1] For example, linear algebra is the elegant and concise language in which to teach many topics in Classical Mechanics, Electricity and Magnetism, and Quantum Mechanics. Taught to proficiency once saves weeks in a 3-quarter introductory sequence.

3. “The public doesn’t understand that Science is what brought us the IPhone.”

When teaching, physicists often seem unable to see our own unstated approximations: In #1 the swing spends more time stopped than moving and that’s when a kid would let go; In #2 we assume no friction, so no spin on the ball; For #3, unlike the two distinguished scientists who waved their Iphones in the air at lunch last week while making this claim, the students and public also credit Capitalism and Steve Jobs. Oy.

3 Assessment and Incentives

We are stuck on some very old and ill-motivated ideas. Some examples, bad and good:

Grading on a Curve– Statistical Basis?
(from E.E. Moise) Even if the shape of the curve for the US population is a bell curve, the correct *a priori* curve for the class is most likely the ‘high tail’ of a peaked distribution, i.e. a rapidly falling curve, rather than another bell curve.

Grading on a Curve– Goal?
My goal is that *every* student has mastered the material at a level beyond normal expectation [2]. Why plan on giving C’s and D’s?

The Ski Instructor Model
Ski schools are a good model to emulate. Students are self-selected to learn. Students are carefully placed according to individual placement tests, and if mis-placed moved to the right level. The goal is to foster enthusiasm while pushing current ability. There is no intent to permanently ‘weed out’ seemingly weaker students (who are often eventually the best).

Truncated Means Rather than Averages
In a quarter I typically give 8 Quizzes, 9 Problem Sets, a Midterm, and a Final. Rather than average the Quiz scores, I discard the 2 lowest grades, and average the remaining 6. The 2 lowest are typically not representative of what the student can do, and are often due to external effects (illness, room-mates, other commitments, e.g.). Einstein’s, Pauli’s, and (I’m told) Da Ponte’s averages aren’t so high–there are some zeros that drag them down. Average is the wrong measure of capability.

Final Grade: Not Holding Grudges
If a student has mastered the material by the end of the course (i.e. on the Final) they get a good grade. Why not?

4 At the Student Level

Socializing the Bullies
First-year Honors Physics (P141) suffers from ‘hot-shots’– products (largely males) of

[2] This requires identifying early in the quarter students who just aren’t going to make it–tends to be no more than two or three out of 40-50 in P141, e.g.; slightly higher in Phy Sci 111.

good schools with AP courses that covered the same material, and eager to show off. I have found that starting with a topic that none of them have had in a language none of them know is a 'levelling mechanism' while they get over it (see study groups below).

Study Groups

I announce that my course is paced too fast for anyone working alone–each student has to have a study group to keep up with the problem sets. However, 'the work you do has to be yours alone', and there's a quiz every week. It seems that study groups are particularly important for (some) women who take a while to realize that they can kick hot-shot butts.

Placement, Advising, Supporting the Weaker and Challenging the Stronger

1. Our Physics majors are advised by (well-meaning) folks who often know little physics and little about individual Physics Dept. professors and courses;
2. The ultra-conservative advice from the Physics Dept. encourages stronger students to go much more slowly than they could and should;
3. At the same time students with weaker backgrounds are given a weaker curriculum;
4. I confess that I advise joint Physics/Math Majors to drop the Physics degree–Math has fewer required courses and college is too good to waste on your major, especially if it is very dilute and cumbersome;

5 Other Annoyances/Malpractices

Whingeing on practices that won't ever be changed:

Testing Untaught Skills- 'Unpacking'

We (physicists) give problems on exams that students haven't seen; the problems are based on principles that we've taught, but gussied up so that the principles are disguised. However, 'unpacking' the underlying principle is a separate skill, not obvious, nowhere taught, and encountered only on Midterms and Finals.

Early morning classes and finals (Indefensible)

For many years the Phy Sci classes I taught had 8am Finals, it turns out for no reason (slots later in the day were assigned to classes that had papers rather than exams). We insist on teaching 9am classes to teenagers. We would be very attractive if 10:30 were the earliest start time; also much more effective.

May 2, 2017
H.J. Frisch

The Spare Parts Theorem

(From David H. Frisch, who learned this from Henry Eyring (1901-1981), the distinguished physical chemist at Princeton, who was his mentor when he was an undergraduate there:

Theorem:

There are more Horses Asses than Horses

(no proof was given: I suspect that it is by induction. HJF)

HJF
Sept.
22, 2004

Quoted at: http://www.pbs.org/benfranklin/l3_citizen_founding.html

Franklin was appointed by the Continental Congress to a committee charged with drafting a formal document to justify the colonies' decision of severing political ties with Britain. The other members of the committee included Thomas Jefferson, John Adams, Robert Livingston and Roger Sherman. The committee gave Jefferson the task of writing the first draft. Franklin, although a talented writer, took a back seat in drafting the document, blaming his lack of participation on poor health.

Jefferson sent his finished draft to Franklin for review. Franklin put on his editor's hat, but made only a few slight changes to Jefferson's prose. When the draft was submitted to Congress, however, sentence after sentence was either deleted or changed, much to the dismay of Jefferson.

Later, Jefferson recalled a story that Franklin told him as members of Congress picked away at the draft.

"I have made a rule, whenever in my power, to avoid becoming the draughtsman of papers to be reviewed by a public body. I took my lesson from an incident which I will relate to you. When I was a journeyman printer, one of my companions, an apprentice hatter, having served out his time, was about to open shop for himself. His first concern was to have a handsome signboard, with a proper inscription. He composed it in these words, 'John Thompson, Hatter, makes and sells hats for ready money,' with a figure of a hat subjoined. But thought he would submit it to his friends for their amendments. The first he showed it to thought the word 'Hatter' tautologous, because followed by the words 'makes hats,' which showed he was a hatter. It was struck out. The next observed that the word 'makes' might as well be omitted, because his customers would not care who made the hats. If good and to their mind, they would buy them, by whomsoever made. He struck it out. A third said he thought the words 'for ready money' were useless, as it was not the custom of the place to sell on credit. Every one who purchased expected to pay. They were parted with, and the inscription now stood, 'John Thompson sells hats.' 'Sells hats!' says the next friend. 'Why, nobody will expect you to give them away. What then is the use of that word?' It was stricken out, and 'hats' followed it, the rather as there was one painted on the board. So the inscription was reduced ultimately to 'John Thompson,' with the figure of a hat subjoined."

November 27,1999

Dear Toronto Colleagues,

I want to thank you for the good suggestions you have made, in particular the finding of mistakes. This is really useful- in the welter of many changes mistakes creep in, particularly late in the game when the authors are completely worn out with responding.

However I would like to make a suggestion. Could you take a look at item App. B.1 in the CDF guidelines for publication? It explicitly asks that collaborators NOT rewrite papers. There are many different styles of writing, and as long as it's clear, and above all, correct, the prose should be left to the authors and the literary godparent. Why, you might ask, should this be so, when we obviously like it to be different? It is because of the effect I referred to above: eventually one does more harm than good by making changes. I referee many papers from D0, and often find sentences without verbs, missing articles, etc.; how, you may ask, does this happen in a collaboration of 500 people. Are they less literate than we are? What is the mechanism that obvious errors creep in and are not caught before it's mailed?

First, the question of whether D0 is less literate than we are. It's unlikely on a statistical basis. More than that, our papers suffer from the same disease; our dilepton mass paper went out without a reference to the D0 paper, to pick an example where I know that the authors are exceptionally careful and conscientious. How did this happen?

I have long claimed these obvious errors get through because we do not focus on the important issues in the review process for our papers. Very few collaborators read our papers, and those who do spend much time and effort in rewriting. Every rewrite introduces errors: it's a standard rule of thumb in the software industry that every change has a 50\% chance of introducing a new error. For papers, I would guess it's not far off: the integrity and coherence of having a paper written by authors who take full responsibility gets diluted, and eventually the damn thing just gets mailed.

The biggest issue in the prose, I would posit, is not whether we really want to use a comma there, or replace "contribution from" with "branching ratio for", or remove the word "rather", but are there any glaring blunders, or sentences that just aren't clear? Rewriting it your way will most likely make it worse; take a look at Ben Franklin's essay on writing by committee. (I really recommend this- take it to heart!).

Lastly, I could use some help on some serious authorship issues. We still don't have a top cross-section; the written record of the CDF top cross section is, to put it politely, a mess. Would you be willing to use some of your (obvious) excess time and energy to push on getting the top cross section paper out? It's much more important than many of the nits you have picked. It's important for young folk to focus on the important issues in a big collaboration; remember Pauli: 'so young, and already done so little'.

So, in conclusion, I really appreciate the comments related to content; finding the errors you've commented on has been really important. The Toronto group is doing a real service by reading the papers carefully. All I ask is that you restrain yourselves in rewriting; it generally makes things worse, and is contrary to CDF policy for that reason. And, if you can identify the really important papers that we HAVEN'T written, and can use your talents to writing them, rather than rewriting extant papers, you will make a major contribution to the collaboration.

Sincerely, and best wishes,
Henry

Oct. 3, 1994
H.J. Frisch

Is D0 More Sensitive Than CDF?

"The Two Experiments Have Comparable Sensitivity"
(quote from a draft of the DPF Electroweak Working Group report.)

Well, I don't know. Starting with the leadership, I think that Mel and Bill are certainly as sensitive as Mont and Paul, though Mont can be teased more easily than Bill, perhaps. *A priori* it's certainly reasonable that the collaborations have comparable sensitivity, as it's unlikely that two collections of more than 400 people each would select very differently on sensitivity. There are fluctuations- we on CDF have a few hyper-sensitive people, and there are a few who are insensitive. But by-and-large it's a collection of typical physicists: sensitive to those things that affect them directly, and oblivious to much else going on in the collaboration.

One could consciously work to increase the sensitivity of one of the experiments- a type of 'sensitivity training', to use a phrase from another, more sensitive, context. It might not be hard: one could add one tape to the videotapes Dee Hahn uses for safety training, for example. I'm just not sure the goal is worth the effort, and it could lead to a 'sensitivity race', wherein each experiment invests substantial effort in increasing its sensitivity. But as long as neither achieves 'incomparable sensitivity', unlikely given the opportunities that are being thrown away by the Lab, it's not unreasonable that the two experiments will have comparable sensitivity for a long long time.

(Note added June, 1999: Five years have gone by since I wrote this, and Run II is still a long ways away. The 'long long time' of no beam when 'sensitivity' is just wishful thinking still looks long.)

The Twin Questions of Authorship and the Reproducibility of Results in Large Scientific Collaborations

Henry Frisch

Enrico Fermi Institute and Physics Department

University of Chicago

Austin Texas, November 18, 2004

Abstract

The Tevatron Collider experimental collaborations have ~ 550 ($D\emptyset$) to ~ 800 (CDF) authors on their author list. The LHC experiments, several years from taking data, are already much more than twice that size. This phenomenon is not limited to High Energy Physics; collaboration size is growing in Astrophysics, Space Physics, and the biomedical world. But, as in the development of the Web, HEP has been a leader in these new areas of cooperation and communication. Who should be listed as an author, what is valued from collaborators, what from collaborators is rewarded, and how contributions are known, acknowledged, and archived are difficult but critical questions, especially important to the field's most important resource, young scientists. How a scientist external to the collaboration explores, understands, and if possible reproduces a published result is a question that is intertwined with the way results are published, the availability of internal documentation and the data themselves, and the custodial responsibilities and structures set up by the collaborations themselves.

1 Introduction

The intellectual achievements of High Energy Physics in the approximately last 30 years form one of the great cathedrals of science, with the discoveries of partons (quarks and gluons), the W and Z bosons, the charmed, bottom, and top quarks, direct CP violation in the kaon and B systems, neutrino masses and mixing, and the precise determination of the parameters of the Standard Model. As seen by an experimentalist, progress on the theoretical side has been equally impressive, starting with the remarkably robust Standard Model itself with its gauge theories of the electromagnetic, weak and strong interactions, and extending to a range of predicted phenomena including new extra space dimensions and structures in a wildly different geometries, a doubling of the number of elementary particles ('Supersymmetry'), new families of quarks and leptons, and new larger group structures.

During this time the size of experimental collaborations has grown enormously, with the Tevatron Collider experiments each being between 500 and 800 collaborators. So far this year CDF has published 26 physics papers and has 19 drafts in the internal review process; this pace will increase dramatically when the analysis software becomes less fluid. The current convention is that every eligible collaborator puts her or his name on every paper by default.

The Current CDF Default Author List

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(CDF Collaboration)

Authorship in Large Scientific Collaborations: Writing

Franklin was appointed by the Continental Congress to a committee charged with drafting a formal document to justify the colonies' decision of severing political ties with Britain. The other members of the committee included Thomas Jefferson, John Adams, Robert Livingston and Roger Sherman. The committee gave Jefferson the task of writing the first draft. Franklin, although a talented writer, took a back seat in drafting the document, blaming his lack of participation on poor health.

Jefferson sent his finished draft to Franklin for review. Franklin put on his editor's hat, but made only a few slight changes to Jefferson's prose. When the draft was submitted to Congress, however, sentence after sentence was either deleted or changed, much to the dismay of Jefferson.

Later, Jefferson recalled a story that Franklin told him as members of Congress picked away at the draft.

"I have made a rule, whenever in my power, to avoid becoming the draughtsman of papers to be reviewed by a public body. I took my lesson from an incident which I will relate to you. When I was a journeyman printer, one of my companions, an apprentice hatter, having served out his time, was about to open shop for himself. His first concern was to have a handsome signboard, with a proper inscription. He composed it in these words, 'John Thompson, Hatter, makes and sells hats for ready money,' with a figure of a hat subjoined. But thought he would submit it to his friends for their amendments. The first he showed it to thought the word 'Hatter' tautologous, because followed by the words 'makes hats,' which showed he was a hatter. It was struck out. The next observed that the word 'makes' might as well be omitted, because his customers would not care who made the hats. If good and to their mind, they would buy them, by whomsoever made. He struck it out. A third said he thought the words 'for ready money' were useless, as it was not the custom of the place to sell on credit. Every one who purchased expected to pay. They were parted with, and the inscription now stood, 'John Thompson sells hats.' 'Sells hats!' says the next friend. 'Why, nobody will expect you to give them away. What then is the use of that word?' It was stricken out, and 'hats' followed it, the rather as there was one painted on the board. So the inscription was reduced ultimately to 'John Thompson,' with the figure of a hat subjoined."

(Quoted at: http://www.pbs.org/benfranklin/l3_citizen_founding.html)

The APS Guidelines: Conventional Wisdom on Authorship

From the present (Nov. 2004) APS web page on Professional Conduct [2]

"APS Ethics & Values Statements

02.2 APS GUIDELINES FOR PROFESSIONAL CONDUCT

Authorship should be limited to those who have made a significant contribution to the concept, design, execution **or** *interpretation of the research study. All those who have made significant contributions should be offered the opportunity to be listed as authors. Other individuals who have contributed to the study should be acknowledged, but not identified as authors. "*

(http://www.aps.org/statements/02_2.cfm)

(**Note: I am fairly sure that before 2002 the 'or' in the list of requirements for an author used to be 'and', an interesting and important evolution in meaning, but have not been able to verify this to my complete satisfaction).**

Further:

"SUPPLEMENTARY GUIDELINES ON RESPONSIBILITIES OF COAUTHORS AND COLLABORATORS
(Adopted by Council on November 10, 2002) [2]

All collaborators share some degree of responsibility[1] *for any paper they coauthor. Some coauthors have responsibility for the entire paper as an accurate, verifiable, report of the research. These include, for example, coauthors who are accountable for the integrity of the critical data reported in the paper, carry out the analysis, write the manuscript, present major findings at conferences, or provide scientific leadership for junior colleagues.*

Coauthors who make specific, limited, contributions to a paper are responsible for them, but may have only limited responsibility for other results. While not all coauthors may be familiar with all aspects of the research presented in their paper, all collaborations should have in place an appropriate process for reviewing and ensuring the accuracy and validity of the reported results, and all coauthors should be aware of this process. ..."

[1] Emphasis added by HJF. I wonder what Darwin would make of this.

Authorship: Status Quo in HEP: CDF e.g.

The large collaborations take authorship very seriously, with a tight control of the author list, a grueling internal review process, and mechanisms to ensure collaborators read the papers. However due to the rapid pace of publication and the breadth of physics topics and personal interests most papers are ever read by a small fraction of authors.

The CDF bylaws read [4]:

```
0.) Definitions:
        i) "List of Authors" means the names of people to be
        listed on a paper submitted by the CDF Collaboration
        for publication in a scientific journal.
        ii) "Standard Author list" represents a default
        group of people who are to be included in all papers
        for publication with the exception listed below.

1.) Members of the CDF Collaboration become part of the Standard
Author list after they have completed a minimum of 1 FTE-year
of service work in the CDF Collaboration. ....
2)...
3.) Any person on the List of Authors for a specific
publication may request that their name be removed.....
```

Note: I refer to this as 'Opt Out'- You are an author unless you ask not.).

```
4)...
5.) The List of Authors for all publications shall be listed
alphabetically, sorted by the last name, first name, regardless
of institutional affiliation.  ....
6.)....
7.)....
8.) A person who ceases to be a CDF Member will have his/her
name included on publications for one year after their
membership has ended, ....
```

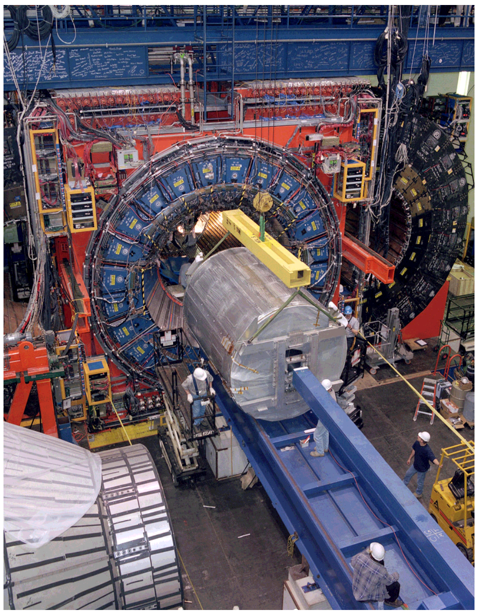
Authorship: Why It's This Way

These issues have been debated inside most big collaborations, and I can give a sample of the arguments that are made in the favor of the present policy over one that emphasizes writing the paper:

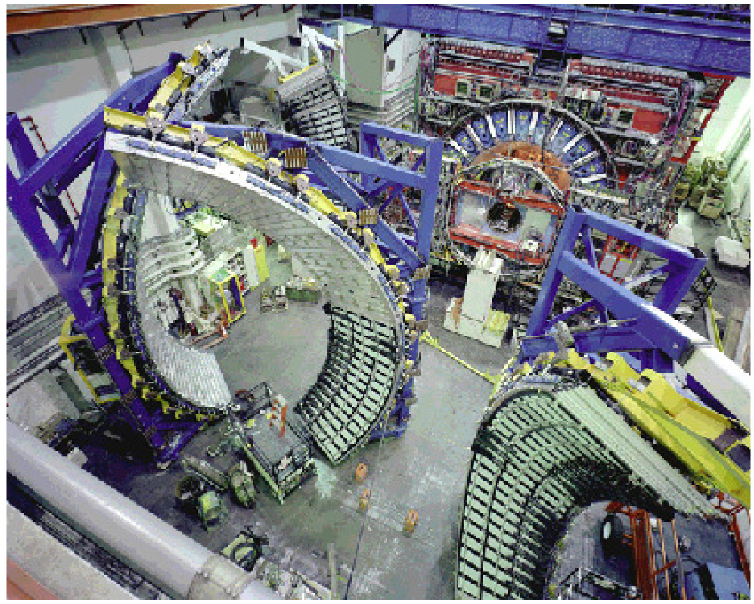
- Young physicists working hard on the nitty-gritty detector details (often hardware, in the parlance of the field, but lately increasingly complex software) will get no credit, while more aggressive and less principled folk will 'skim the cream' by preparing the analyses while waiting for the detector to be built and commissioned so that they can jump on the data.
- There is a type of physicist who understands the care and planning that it takes to get first-rate data. These are often 'instrument-builders'; people without whom the experiment would not happen. Often they are the originators of crucial ideas (for example, the silicon vertex detector at CDF was critical to our discovering the top quark), and have followed those ideas through to fruition. They are often by nature self-effacing and independent, and would not put their names on papers written by others, even those that depend critically on their work.
- It is difficult and painful to decide who among 500+ authors is deserving and who isn't; spokespeople have too much to do as it is, and it could occupy a large number of people arbitrating disputes for priority and credit. It is much easier to have a uniform policy, with clearly defined rather mechanical guidelines.

There is a great deal of truth in all these arguments.

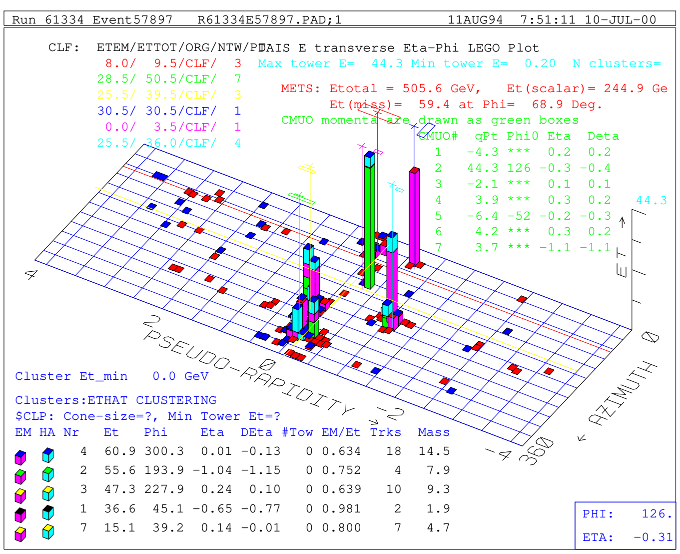
It's Hard to Convey the Complexity of A Big Detector



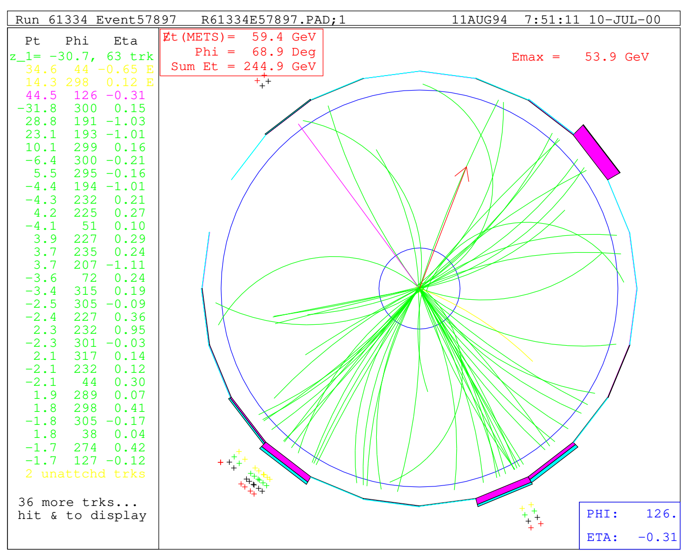
(a) The Central Detector Alone



(b) Central Detector and Some CMX



(c) A(n A)typical Event ($t\bar{t}\gamma$?): Lego



(d) A(n A)typical Event ($t\bar{t}\gamma$?): CTC

Figure 1: The CDF detector, and what may be a lovely $t\bar{t} + \gamma$ event.

Authorship: The Other Side to the Arguments

However, I believe that these arguments are based on some unwritten assumptions:

- **Having one's name listed on a paper with hundreds of authors has an impact on getting a job in a university physics department.**
- **Physicists can do sophisticated analyses without understanding the detector.**
- **Getting credit for what you actually do will carry less weight than assigning equal credit to everybody for everything.**
- **The 'instrument-builders' benefit from credit they get from being authors on all papers from the collaboration.**

Each of these assumptions I believe to be flawed. Taking them in order:

A short list of papers that one has actually written carries much more weight in a faculty meeting than 5 pages of titles all attributed to A. Aardvark et al.

Those who try to 'skim' have a huge disadvantage compared to someone intimate with the detector and the data.

And 'instrument-builders' can and should be recognized for what they do, give talks, and write papers on their contributions. Those who do are internationally known and are highly respected. Adding their names to papers they know nothing about does not increase this respect.

Reproducibility of Results in Science

This question of authorship is related, I believe, to a fundamental tenet of science: scientific results should be reproducible by others. This concept also has evolved with the advent of big unique facilities: one cannot oneself replicate results from a Mars Lander, or even from CDF. High Energy Physics has met this change by having several competing collaborations: 4 experiments at LEP, 2 at the Tevatron, Belle and Babar, as well as Cornell, in e^+e^- B-factories. Beyond that, a certain transparency is necessary to establish the credibility of results: one should have enough details to explore, understand, and discuss the methods, including access to broader documentation, contacting the authors, and, possibly access to data. There is a responsibility and custodial role for the data and the analysis framework so that results from unique data can be revisited and reproduced.

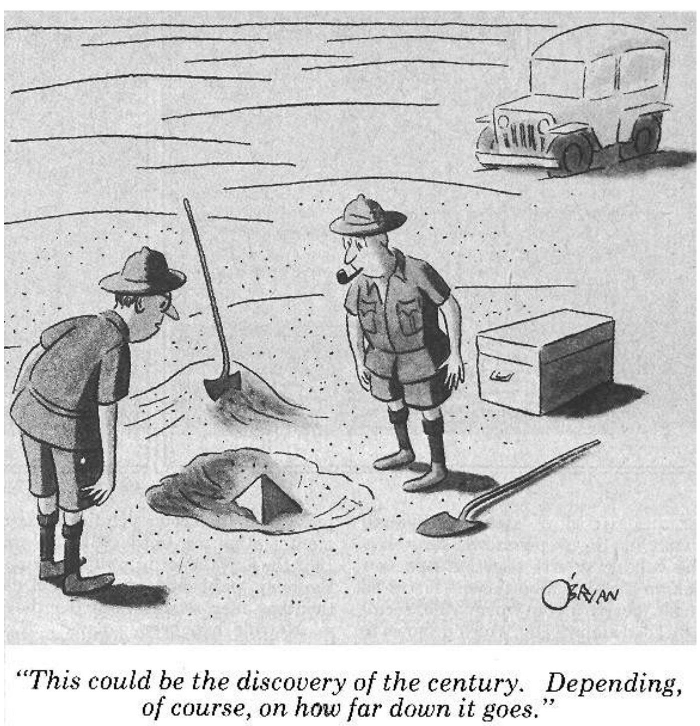


Figure 2: Reconstructing a CDF analysis from Run 1

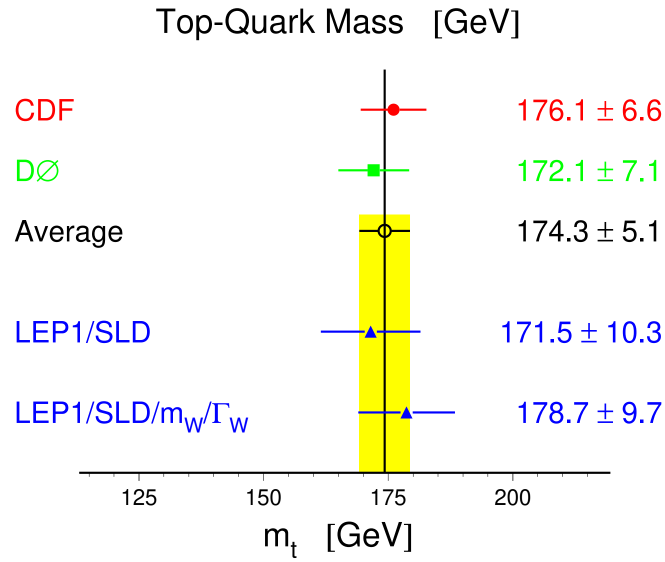
Reproducibility of Results in Science

However in a big collaboration only a few people know the details. The code has gotten exceptionally complex, so that reproducibility at a later time is difficult. And often the work has been done by a grad student or postdoc who has then moved on.

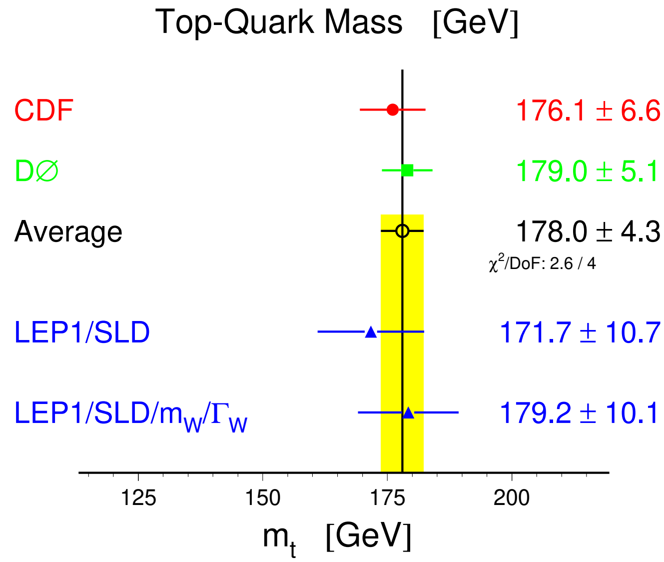
The upshot is that it is getting very hard to explore and understand an older result, much less reproduce it. As long as new and better data supercede the old this isn't a problem. It can be, however, a problem in precision measurements, where numbers are averaged.

In the next page I discuss a recent example, the re-measurement by the D0 collaboration of the top quark mass using Run I data and a much more sophisticated method [2]. The data are the same in both the old and the new analyses, and, in my understanding, all the calibrations are the same. The new method produces a result for the top mass of $180.1 \pm 3.6(stat) \pm 3.9$ GeV/c^2, versus the older measurement [6] of $173.3 \pm 5.6(stat) \pm 5.5$ GeV/c^2. The new paper says [5] "we expect the difference between the original and the new mass measurement to be on the order of 4 GeV/c^2. Thus, the two results differ by less than two standard deviations." The new measurement is an important result, as shown on the next page; moreover understanding how a change in analysis technique with the same data can significantly change a precision measurement may be important for the field. Can it be understood event-by-event?

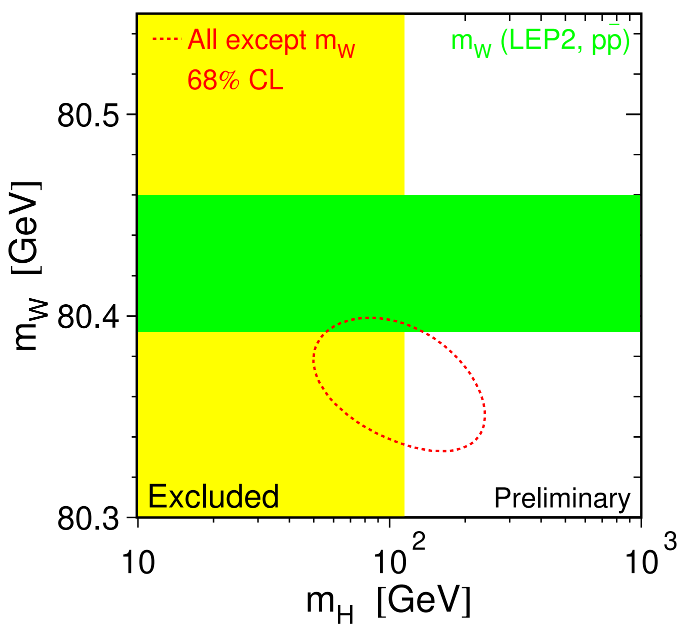
[2] I see similar cases in CDF; I do this not to point fingers, but because it's such a good example of a growing problem.



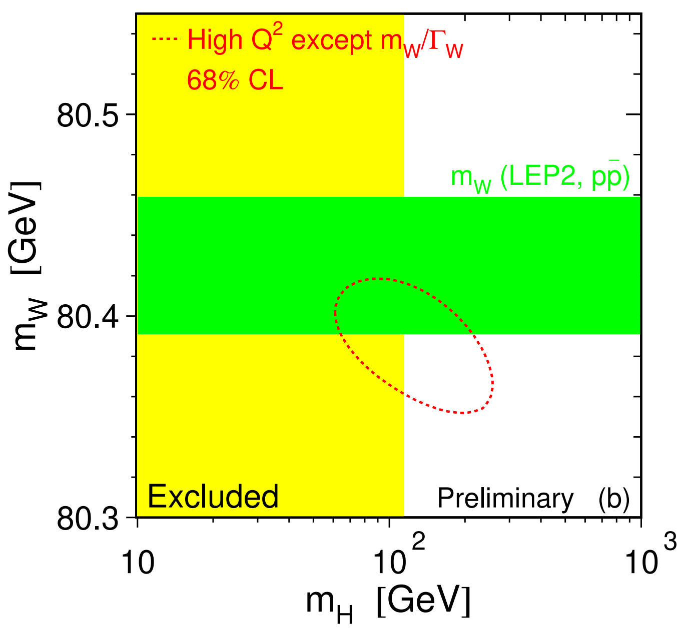
(a) Top Quark mass, Summer 2003



(b) Top Quark mass, Winter 2004



(c) Higgs/W mass plane, Summer 2003



(d) Higgs/W mass plane, Winter 2004

Figure 3: The measured and fitted values of the mass of the top quark, summer 2003 (top left) and winter 2004 (top right). The constraints on the higgs mass (red dotted oval) in the W-mass- Higgs plane from precision measurements of the SM, especially the mass of the top quark. The plots from winter 2004 (right hand plots), include the D0 top mass reanalysis of the Run I data. Plots from the LEP EWK Working Group [3].

What Should be the Goals of an Authorship Policy?

1. To allow scientific results to have as open and complete a scrutiny as possible over an extended time ('reproducibility', in short-hand, but sometimes translated as 'transparency' by necessity.), by identifying those who will carry that responsibility.

2. To give credit for the creativity and hard work of those to whom it is due, including those whose work may be critical to, but not obvious from, the work described in the paper.

3. To allow those outside the field to judge the contributions of young scientists who may be applying for jobs, promotions, or awards.

4. To encourage the publication of technological advances, possibly including software, as a means of documentation and as intellectual work in its own right.

5. To encourage more members of a large Collaboration to read widely of 'their own' work in subfields outside their own specific areas.

Discussion: Looking Forward

Some Suggestions

1. **Separate the list of Collaboration Members as a separate entity from the paper author lists. Refer to the Collaboration list in the author list in each paper as well as to the authors listed by name (see next item).**
2. **Change the default from 'Opt Out' to 'Opt In'. 'Opt In' starts with only those who have taken part in the specific analysis as authors on the draft. All eligible authors who acknowledge having read the paper are welcome to put their names on it. The Belle collaboration has done this using a web form; it is easily and cleanly implemented.**
3. **Have senior managers put more emphasis on a continuing publication of the technical (instrumentation and software developments by those physicists who work primarily on them. These papers have traditionally have only the primary authors on them. This documentation is beneficial both inside and outside the collaborations.**
4. **Encourage physicists in 'support roles' to adopt a physics topic and to study and vet the papers in that area [8].**
5. **Make public access to the internal notes associated with each paper. This gives a paper trail and allows a detailed understanding of what was done.**
6. **Identify in the author list those to whom questions should be addressed. This (short) list should start with the graduate student whose thesis this is (this is the usual case), and include up to several others.**

Summary

I believe that having clarifying authorship will help rather than hurt young folk. The related problem of what I call 'reproducibility', but which often means exploring and understanding a result that cannot be directly reproduced, will also benefit from a clarified authorship. These are very hard problems: high energy physics has evolved rapidly into these huge collaborations of immensely talented driven young physicists, with a benign management structure of the scientific output itself (as opposed to fiscal management, which is tightly run). I hope physicists in other fields aren't too critical; the problems are different, and inside the field the conventions are understood. But I think the present policy isn't serving well the very people it was intended to protect.

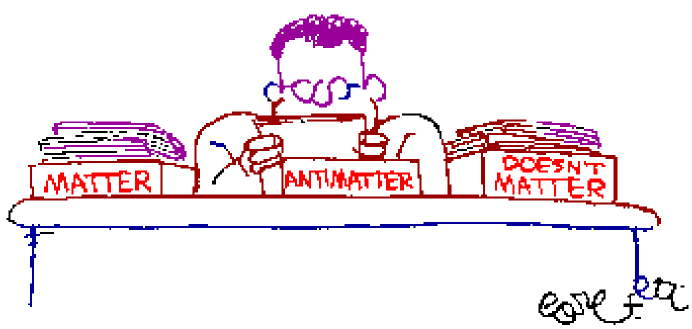


Figure 4: Too many CDF papers to read!

Acknowledgments

I would like to thank Kent Staley for the opportunity to talk about these issues in a broader context. I benefited from conversations in 2002 with Steve Mrenna, Jack Sandweiss, and George Trilling, and more recently several extended conversations with Vera Luth, Chair of the C11 Commission of IUPAP. I thank Martin Blume for historical information on the evolution of the APS guidelines. My wife Priscilla found the cartoons [9].

The opinions are my own, however, and slings and arrows should come at me alone. I have no special wisdom in these matters, but the dilution of the meaning of authorship, particularly with respect to the questions of reproducibility and responsibility, has troubled me for a long time.

References

[1] The pre-2002 APS guidelines were found on the APS pages at http://www.aps.org/conduct.html. The link now seems to be broken. My original essay on this topic quoted from this page with the 'and' in the place of the present 'or'.

[2] The 2004 web site of the APS under Professional Conduct: (http://www.aps.org/statements/02_2.cfm).

[3] The LEP EWK Working Group http://lepewwg.web.cern.ch/LEPEWWG/plots.

[4] From the CDF bylaws: on an (internal) page at: http://www-cdf.fnal.gov/internal/spokes/Laws.html. I can probably smuggle out the full text on request.

[5] New Measurement of the Top Quark Mass in lepton + jets t anti-t Events at DØ. By DØ Collaboration (V.M. Abazov et al.). FERMILAB-PUB-04-102-E, July 2004. e-Print Archive: hep-ex/0407005

[6] DØ Collaboration, S. Abachi *et al.*, Phys. Ref. D58, 052001 (1998).

[7] This solution may not be workable in the huge international collaborations at the LHC where national issues also intrude (I thank George Trilling for pointing this out).

[8] I thank Vera Luth for this idea.

[9] Figure 1 was on http://www.homepages.dsu.edu/Mukhopai/cart0479.jpg (Dakota State University, Madison S. Dakota);
Figure 3 is from: http://www.skyrootuni.com/forum_main.htm. The credits at this URL are: "Original cartoon by Jack Corbett from American Scientist, Jan.-Feb. 2001, vol.89, #1, p.45.

June 11, 1999
H.J. Frisch

Thoughts on Authorship on Big Experiments

1 The APS Guidelines on Authorship

The APS guidelines on authorship are very clear[1]:

'PUBLICATION AND AUTHORSHIP PRACTICES'
'Authorship should be limited to those who have made a significant contribution to the concept, design, execution and interpretation of the research study. All those who have made significant contributions should be offered the opportunity to be listed as authors. Other individuals who have contributed to the study should be acknowledged, but not identified as authors.'

2 Collaboration Guidelines in HEP

In my experiment, CDF, (which stood originally for the Collider Detector Facility, until it was realized that a Facility was a formally defined object, after which it became the Collider Detector at Fermilab), the 500+ collaborators work under the following guidelines for authorship[2]:

```
0.) Definitions:

        i) "List of Authors" means the names of people to be
        listed on a paper submitted by the CDF Collaboration
        for publication in a scientific journal.

        ii) "Standard Author list" represents a default
        group of people who are to be included in all papers
        for publication with the exception listed below.

1.) Members of the CDF Collaboration become part of the Standard
Author list after they have completed a minimum of 1 FTE-year
of service work in the CDF Collaboration. The definitions
and standards for service work are determined by the Spokespersons
and Project Managers (see appendix A for more specifics).
When the experiment is in operation, Members  on the Standard Author
list must also contribute to the operation of the detector. This
is normally satisfied by taking shifts. The precise
requirements for the contribution to detector operations
is proposed by the CDF Department and is approved by
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the Executive Board.

2.) Visitors to CDF who make important contributions to one or more papers may be added to the author list for those papers. The Executive Board member from the sponsoring Institution makes the request to the Spokespersons, who then approve this addition. In exceptional circumstances, a long term visitor may be added to the Standard Author list by petition to the Executive Board.

3.) Any person on the List of Authors for a specific publication may request that their name be removed. This request must be transmitted to the Spokespersons and the Analysis Conveners associated with the publication a minimum of three days in advance of the submission of the publication to a journal. Changes to the author list are then transmitted to the CDF Secretary.

4.) The above rules do not apply to the publication of technical information (e.g. design of specific apparatus used on CDF).

5.) The List of Authors for all publications shall be listed alphabetically, sorted by the last name, first name, regardless of institutional affiliation. Institutional affiliation shall be designated by a superscript referring to a list of institutions that follows the list of names on the List of Authors.

6.) Where applicable, the editorial constraints of a specific journal may supersede item 5.) In this case, a negotiated format for publication will be employed. The CDF Spokespersons, or their representatives, are authorized to negotiate any format changes to the List of Authors.

7.) The list of authors shall be updated twice a year - once in January and once in July. It is the responsibility of the Executive Board member representing each institution to provide an accurate and current list of members to be included on the author list. In extraordinary cases, upon the request of a Collaboration member, additions to the author list may be considered between these updates by the Executive Board.

8.) A person who ceases to be a CDF Member will have his/her name included on publications for one year after their membership has ended, unless the Executive Board decides otherwise.

```
Appendix A

Specific requirements of service work in the period 1995-2001:

Authors on Run II papers must put in a minimum of 1 FTE-year
of service work on CDF.  "Service work" is defined by the
Spokespersons and Upgrade Project Managers.
```

While the details vary from big High Energy collaboration to collaboration, many of the features are the same: the default is that all physicists who have devoted more than some threshold time are authors, the listing is alphabetical or some democratic variant thereof (some collaborations make exceptions for graduate student theses, for example, or rotate the starting point), and no active role of any kind has to be taken to put one's name on a given paper once one is on the list. Moreover one's name typically lingers on the author list (and hence appears on subsequent papers) after one has stopped active participation in the science.

3 The Difficult Issues

The juxtaposition of these two sets of guidelines, both done in good faith and with high principles, seems startling to those who don't work in my field. Why are they so different?

These issues have been debated inside most big collaborations, and I can give a sample of the arguments that are made in the favor of the present policy. At this point I should confess that I have long felt that these arguments are flawed, and there is a better way that achieves the same, laudable, goals. I'm consequently not an unbiased presenter, but will try to be fair. The arguments are:

- Young physicists working hard on the nitty-gritty detector details (often hardware, in the parlance of the field, but lately increasingly complex software) will get no credit, while more aggressive and less principled folk will 'skim the cream' by preparing the analyses while waiting for the detector to be built and commissioned so that they can jump on the data.

- There is a type of physicist who understands the care and planning that it takes to get first-rate data. These are often 'instrument-builders'; people without whom the experiment would not happen. In some cases they are the originators of crucial ideas (for example, the silicon vertex detector at CDF was critical to our discovering the top quark), and have followed those ideas through to fruition. They are often by nature self-effacing and independent, and would not put their names on papers written by others, even those that depend critically on their work.

- It is difficult and painful to decide who among 500+ authors is deserving and who isn't; spokespeople have too much to do as it is, and it could occupy a large number of people arbitrating disputes for priority and credit. It is much easier to have a uniform policy, with clearly defined rather mechanical guidelines.

There is some truth in all these arguments.

4 Hard-nosed Realities

However, I believe that these arguments are in fact not solid. They are based on some unwritten assumptions:

- Having one's name listed on a paper with hundreds of authors has an impact on getting a job in a university physics department.
- Physicists can do sophisticated analyses without understanding the detector.
- Getting credit for what you actually do will carry less weight than assigning equal credit to everybody for everything.
- The 'instrument-builders' benefit from credit they get from being authors on all papers from the collaboration.

I think every one of these arguments is debatable, at the least. A short list of papers that one has actually written carries much more weight than 5 pages of titles all attributed to A. Aardvark et al. Those who try to 'skim' have a huge disadvantage compared to someone intimate with the detector and the data. And many of the 'instrument-builders' are recognized for what they do, and give talks and write papers on their contributions. Many are at National Labs, where publishing is not as critical as for junior faculty. Most are internationally known and are highly respected. Adding their names to papers they know nothing about does not increase this respect.

5 Reconciling with the APS Principles

One of the most important words in the APS guidelines is the 'and' in the phrase 'significant contribution to the concept, design, execution **and** interpretation of the research study.' There is a subtlety as well in the phrase 'research study': is this just the analysis described in the paper, or the project itself? CDF has been going on for more than 25 years now, and so to ask young physicists to have contributed significantly to the 'concept' of the original detector is not reasonable. So what is the research study?

But the 'and' cuts the other way: the present policy means that papers are published with authors who do not even know the existence of the paper and its contents. The default is that the names appear unless someone requests his or her name be deleted. It is hard to keep up with the avalanche of papers covering an enormous range of topics in HEP: diffraction, QCD, W and Z physics, the top quark, the myriad details of b-quark decays, charm production, low-x physics, etc. Most papers are read by only a few of the authors.

I believe that this violates the spirit of the APS guidelines, and that the time has come for High Energy Physics to change. This will have to happen in any case, because the present default is unworkable for the huge collaborations in the LHC (in fact discussions on how to proceed are going on inside these collaborations.)

6 One Possible Solution

One possible [3] solution is, after the collaboration has become old enough such that many of the eligible authors joined after the design and construction of the detector [4], to change the default of having all eligible authors' names on all papers. Instead one could start with only those who have taken part in the specific analysis. All eligible authors who acknowledge having read the paper are welcome to put their names on it. This doesn't change the eligibility requirement, but ensures that authors know the existence of the paper (certainly a minimum as a requirement), and have read it. One could go further and specifically cite the APS guidelines, and request collaborators feel comfortable with the guidelines in each case to request their names be added. In either of these scenarios the honor system would be used; all requests from eligible collaborators would be honored.

I believe that having authorship mean what it was intended will help rather than hurt young folk. These are very hard problems: high energy physics has evolved rapidly into these huge collaborations of immensely talented driven young physicists, with a benign management structure of the scientific output itself (as opposed to fiscal management, which is tightly run). I hope physicists in other fields aren't too critical; the problems are different, and inside the field the conventions are understood. But I think the present policy isn't serving well the very people it was intended to protect.

7 Acknowledgements

I have benefited greatly from conversations with Steve Mrenna, Jack Sandweiss, and George Trilling. The opinions are my own, however, and slings and arrows should come at me alone. There may be better solutions: constructive suggestions would be appreciated.

References

[1] The APS guidelines can be found on the APS page on Professional Conduct, at http://www.aps.org/conduct.html.

[2] From the CDF bylaws, available on: http://ncdf12.fnal.gov/CDFbylaws/spokes.html.

[3] This solution may not be workable in the huge international collaborations at the LHC where national issues also intrude (I thank George Trilling for pointing

this out). But for 'medium-size' collaborations of 500 people or less I believe it will work well, based on my experience in CDF. It's worth a try.

[4] The problem changes over the life of a collaboration, and even over the life of a 'run', the periodic taking of data. It is unlikely that there is one solution for all situations; what we are discussing here is the 'default' solution for an experiment long after data-taking, the average situation, unfortunately, at CDF.

Version 1.0
HJF
August 3, 2018

A Recipe for Clear and Quick Scientific Writing of Reports and Short Papers

1 Introduction

I often have to write grant proposals and reports, and also short scientific notes. I have developed some 'rules' for myself for writing these kinds of papers quickly ; they may help you, in particular if you worry about writing things up.

The following procedure is not gospel, but produces a readable and comprehensible report quickly. Once it's done, you can go back and improve it if you have the time. But the output should serve the purpose and if so you're done.

I assume below that you are using LateX.

2 Steps for Writing a Short Paper or Report

1. Steal a suitable template.tex file (e.g. my simple_template.tex file).
2. Chose a Title
3. Write the Abstract
4. Make an Outline, using Table of Contents (set Counter Depth to 2, e.g.)
5. Enter all the Figures and Tables in their appropriate Sections
6. Write all the captions. Be complete- one should be able to read the paper from the Title, Abstract, and captions alone.
7. Start to fill in the text by referring to the Figures. You may want to move some of the prose from the caption into the corresponding text, but the captions should be full enough so that you can read the paper from them alone. At this step include the references in the bibliography labelled by name- no need to fill them in yet.
8. To finish the text, follow Henry's rule #1 of Scientific Writing: One Thought per Paragraph (and only one), and it occurs in the first sentence. [1]
9. Write the Acknowledgements. Be gracious- better to over-include than have hurt feelings (but be honest- remember a famous thesis acknowledgement "No thanks would be too much for my advisor".).
10. Reconsider the outline; is the order correct? What's missing? What's unnecessary? Fix it.

[1]This produces brutal prose, but if faithfully adhered to it allows going fast. Often hesitation and confusion come from having multiple thoughts and goals on one's mind– this forces taking them one-at-a-time.

11. Fill in the bibliography, and check you haven't omitted anybody who should be referenced. If lots of references you may want to use the script 'ordercite', or Bibtex (complicated, I find).
12. Write the Conclusions. Make them short and quantitative.
13. Reread it and remove all 'Opinion' (for example, 'novel', 'precise', 'unprecedented', 'maximally', 'heavenly', ...)
14. Spell-check it. And then have somebody else read it for comments..