SLAC BEAM LINE

Bj, I think you had better go to the lab now.

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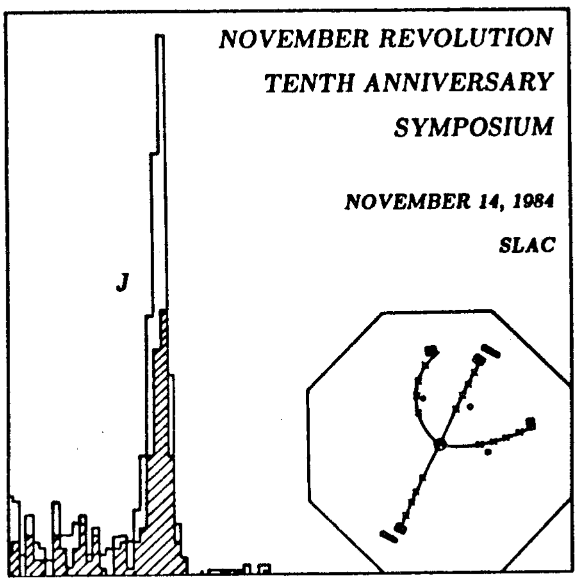

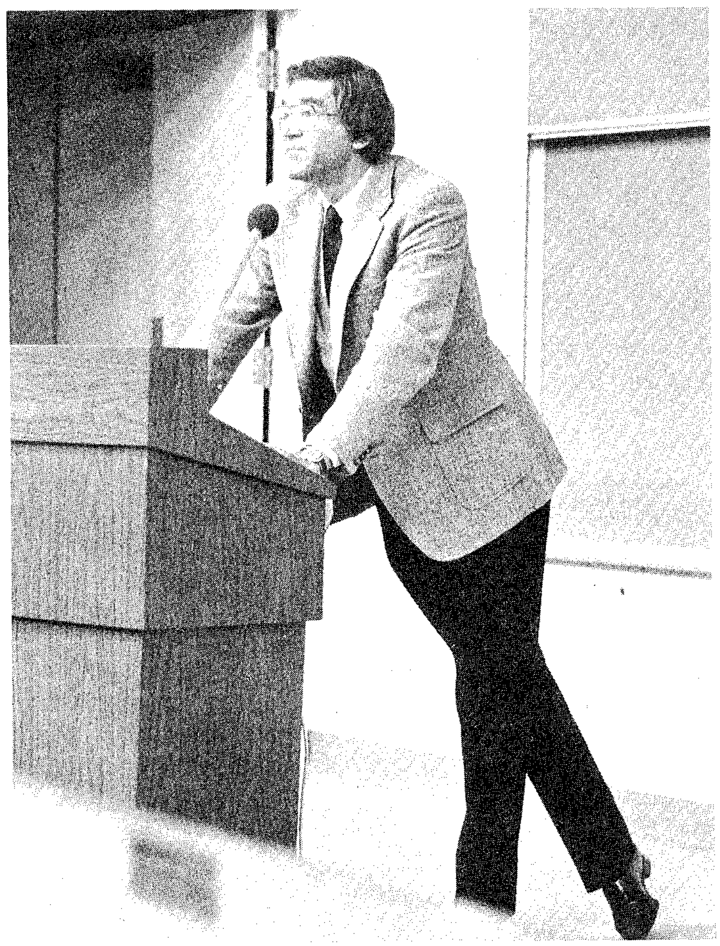

The discovery of the J/ψ, announced in the fall of 1974, resulted in such a rich flow of new physics and new experimental technique that physicists call the era the 'November Revolution.'

The *Symposium on the Tenth Anniversary of the November Revolution*, held at *SLAC* on November 14, 1984, was a recollection of the discoveries and a review of the consequences.

James D. Bjorken, now Associate Director for Physics at Fermilab, was a Professor at *SLAC* from 1963 to 1979, well-placed in all respects to record the excitement.

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THE NOVEMBER REVOLUTION — A THEORIST REMINISCES

J.D. Bjorken

It is my task to reminisce, from the point of view of a theorist, on how the 1974 November revolution changed our way of thinking. It seems that, as the years go by, I change more from making exercises in futurism to exercises in reminiscences. In the past I have been almost a professional futurist. But these days there is clearly no future for me in futurism. With the Superconducting Super Collider and the Large Hadron Collider and large linear colliders, that field is overcrowded, and it will probably remain overcrowded for the next ten years. So I am out of the market in that activity. But reminiscences are fun too — in fact too much fun. When that becomes a dominant pastime, it's clearly past time to get out of this business.

Actually, reminiscing is getting to be a big activity. Maybe because of the success of the field, there is a growing interest in looking at high-energy physics as a cultural phenomenon. The historians and social scientists are moving in. We have our own stable of historians around Fermilab, and they are setting up a history-of-science conference for next spring. It is on the physics of the '50s, and so I was asked as a consultant to comment on the program. It suddenly struck me — my god, I was there. No wonder I am into reminiscing.

I also noticed there is an anthropologist poking around, studying us for the last ten years. There is a little article by her[1] in the *MIT* journal, *Technology Review*. In it is an interesting quote that "there is no one in-house at Fermilab who can tie his shoes experimentally." So, especially given my new position [Associate Director for Physics at Fermilab — *Ed.*], I found that an interesting statement. It is clear that it is important, because it has been said that members of one of the most successful experimental groups working elsewhere not only can tie their shoes but check the tension in their shoelaces by hand every half hour.[2]

Clearly action is required, and I am happy to report that Fermilab has decided to act on this issue. We are going to sponsor a workshop. I thought it would be easy to at least get theorists to come, because there are so many experts on topology, string tension, and all that; in fact, they are very active. But when I looked around and solicited them they are all in at least ten dimensions. That's not likely to do us much good.

Anyway, getting back to reminiscing, it really is fun. But it is easy when one looks back to the past to round off all of the sharp edges, forget all of the difficulties, confusion, and personal shortcomings, and bask in the reflection of how one would have liked to have had things happen. This is true not only at the personal level but also, I think, with the field itself. It is not so much so when we are communicating with each other. But, when we present the history to the outside would and have to simplify it, a lot of the complications get removed. A new mythology is created, with a linear line of logic. Everything progresses neatly from a few ideas of the past to the orderly situation that we find now. Well, as we know, it just was not like that.

So, what does the mythology say? Let us start back in the 1960s, when all of the pieces of the standard model and the present orthodoxy were present. We had the quarks and we had the non-Abelian gauge theories; they had been around for ten years. Charm appeared in that period. The Higgs phenomenon was there. The weak mixing angle was introduced in 1960. The partons and hard collision ideas came in near the end of the 1960s, along with $SU(2) \times U(1)$. Even color emerged, albeit at a rather primitive level. But it was there in the mid-'60s. Why did it take so long to put it together? Why was it so hard? The point is, of course, that that wasn't all that was there. There were lots of other ideas around at the same time, and it was very confusing, really a mess. It was only *QED* which was not a mess. It was not a time, like it is now, for writing textbooks. The right thing to do was to write short monographs in paperback which could be thrown away in two years or so.

What was some of the other clutter around? I will just list some buzz words; they are very evocative. You may remember them and then fill in all of the details:

nuclear democracy

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1 Sharon Traweek, 'High Energy Physics: A Male Preserve,' *Technology Review*, Nov-Dec 1984, p. 42.

2 John Alice et al., 'Alice in WonderCERN' (never published).

current algebra
Regge poles
bootstrap
dispersion theory
field algebra
field-current identities
vector dominance
chiral dynamics
Melosh transformation
$SU(6)_W$
$U(12)$
light cone current algebra
Mandelstam representation
Veneziano formula
Kallen-Lehmann representation
strings
flavor groups
LSZ
Wightman axioms

Don't get me wrong; I don't want to put down these ideas. Most of them are very correct, and still relevant. They are important foundations upon which many concepts that are in the new mythology are built. They may indeed serve again as bases for some future mythology. For example, current algebra gave a solid foundation for the quark-parton picture of hadron substructure as well as a description of the weak interactions upon which the gauge theories were built. Regge poles are right; the theory works. Duality was important in giving linear Regge trajectories and the first hints of the ideas of confinement and confining potentials. Nuclear democracy was right, and very important in eliminating the old mindset that somehow the proton was special and the delta was just an excited state of a proton rather than co-equal. Of course, we see nucleon and delta now as a hyperfine doublet, but hyperfine doublet were not the words that were used then. Nevertheless, the concept of equality in 'fundamentalness' or 'non-fundamentalness' of all of the different hadrons, including the stable ones, was a very important breakthrough and part of the emergent conceptual structure.

In summary, in the '60s there was a lot of clutter, with a plethora of concepts around. We had a good theory of electrodynamics, but the theory of the strong force was just not understood. The weak force was not understood any better. Field theory for the strong and weak interactions was deeply mistrusted. But there was, in the absence of good fundamental theory, a very sophisticated phenomenology, thanks to current algebra and dispersion theory. A big problem in going further was a difficulty in choosing among many options. In the case of *QED*, an established theory, it was easy to know what to do. One tried to knock down the edifice that had been built with the strongest experimental weapons that one had at his disposal. But with no edifice to begin with, one instead had to choose the materials with which one was to be built. The difficulty of choice was really only solved by accumulation of evidence. That means experiment, and there is no substitute for that. One might think there could be a substitute if one just followed the theoretical superheroes who eventually led us to the truth. But when one looks at what those superheroes were doing, they were not only making their truly great and profound contributions to the signal, but they were also contributing to the background. By definition, superheroes are so talented that they tend to make major contributions to most of the 'irrelevant' ideas as well. So that method doesn't work either. There is no substitute for facts and evidence.

Let us now go on to the next stage, say from 1970 to the 1974 revolution. During this period there was clearly a rapid change from a state of disorder toward a state of balance between disorder and order. First of all, the quarks came into their own. The deep-inelastic scattering experiments at *SLAC*, soon followed by neutrino experiments elsewhere, gave evidence for the pointlike structure of hadrons and argued very strongly for fractional charge. Spectroscopy — especially baryon spectroscopy — became during that period very orderly, very beautiful, and very consistent with the quark model. This accomplishment consisted of the accumulation of a large number of individually unremarkable experiments followed by arduous phase-shift analyses. But the totality of this effort led to a clean, beautiful picture.

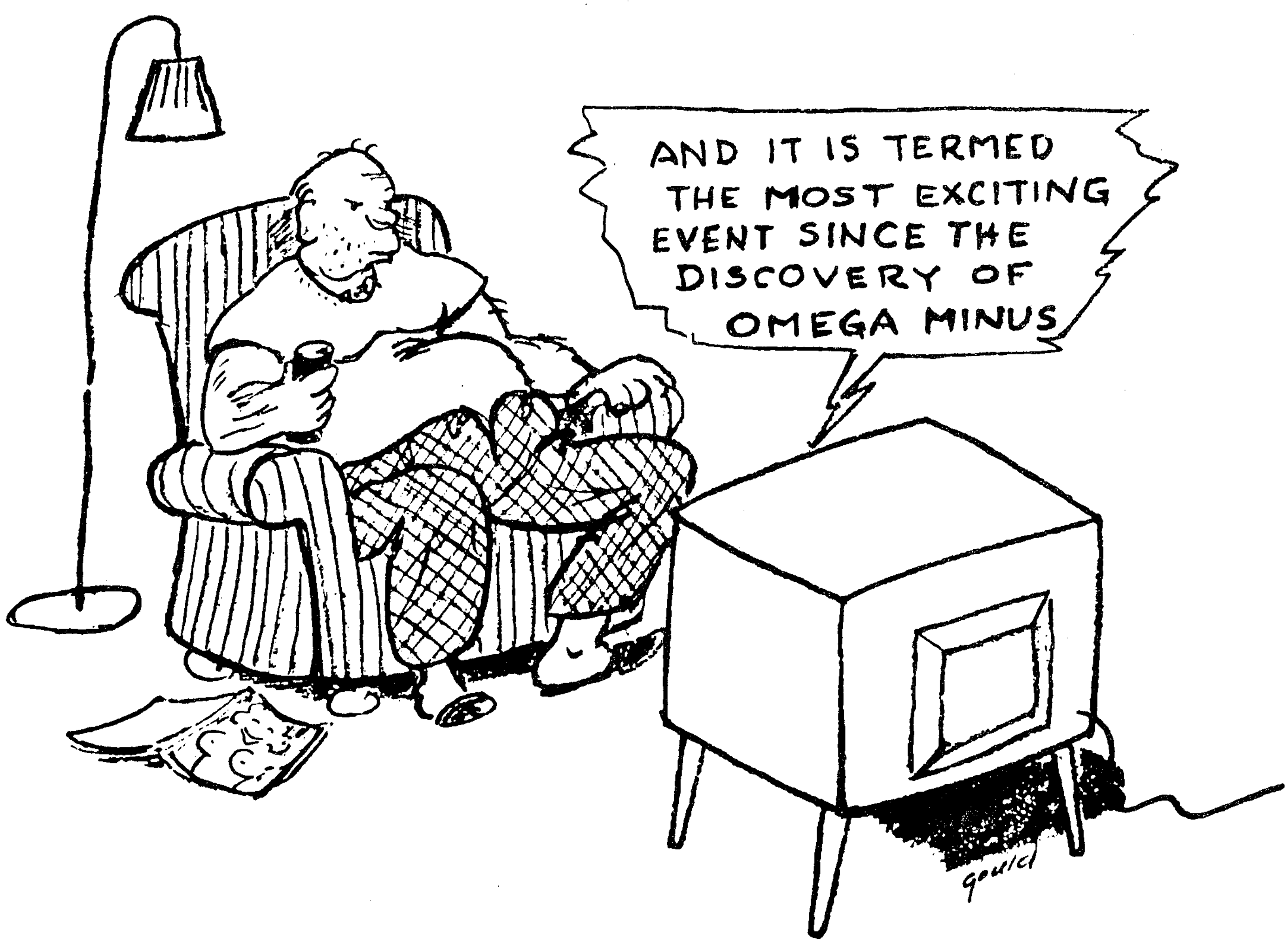
Emergent during this period was the renormalization of the non-Abelian gauge theories. This strongly stimulated the revival of the dormant electroweak theory and the beginnings of *QCD* as well. 'Asymptotic freedom' also appeared for the *QCD* gauge theory description of strong interactions. To a limited extent it began to be supported by scaling-violation evidence in deep inelastic scattering. There was the understanding of the *GIM* mechanism and the role of a fourth quark in obviating the growing problems of the absence of strangeness-changing neutral currents.

This became especially relevant after the discovery of the strangeness-conserving neutral currents during this period.

Thus as one went from the beginning of the decade toward the November revolution one had more credible candidates for orderly theoretical structures underlying strong and weak interactions than one had before. So, by the time one came to mid-1974, the summer before the November revolution, really all of the pieces were in place. In fact, the balance between theoretical confusion and an orderly theoretical structure was clearly shifting; I would say it was roughly 50-50.

The big international conference of that year in London was a turning point and evidenced, very accurately in fact, the nature of the situation at that time. I was there. What I remember taking back from the London conference was the report of Hey, Cashmore and Litchfield on baryon spectroscopy; that was the beautiful (!) result for me that happened there.

But probably the occurrence of greatest notoriety was the situation in e^+e^- collisions; there was an apparent linear rise in R with energy, and the theoretical interpretations were legion. John Ellis reported on them; he made a catalog that filled a



A Bob Gould cartoon of the era.

page on all of the theories of R. They predicted zero, infinity, and everything in between. Ellis's catalog well reflected the state of theoretical confusion and general disarray in trying to interpret the e^+e^- data.

But in the midst of all of this was a talk by John Iliopoulos (I think I was there too). With passionate zealotry, he laid out with great accuracy what we call the standard model. Everything was there: proton decay, charm, the *GIM* mechanism of course, QCD, the $SU(2) \times U(1)$ electroweak theory, $SU(5)$ grand unification, Higgs, etc. It was all presented with absolute conviction and sounded at the time just a little mad, at least to me (I am a conservative).

So at London the pressure to search for charm was there. But even so this was immersed in a rather large degree of confusion. And despite the pressure for charm and the theoretical awareness of the need to search for charm there were also some real lapses. I think most theorists will admit to this at this point — certainly it is true for me. I don't think anyone really pushed to look for the psi in e^+e^- colliding beams. There were some murmurs about looking for it in photoproduction, but no emphasis on searching for a resonance in colliding beams, the thing which is so obvious now. The favored method of search was to look for open charm in hadron-hadron collisions and other places, and not for the bound, 'hidden' charm.

That brings us up to November 1974. The stage was really set. The balance had changed, and the November revolution just set everything into motion toward the standard model that we have now. Most high energy physicists will probably remember where they were when they first heard about the psi. It was like the moon landing, Pearl Harbor or the Kennedy assassination. (See the figure on the previous page.)

I was home and it was dinner hour. Burt Richter called me up and told me the basic parameters over the phone. He said three GeV. I said three GeV per beam, right? He said no, three GeV in the center of mass. I couldn't believe such a crazy thing was so low in mass, was so narrow, and had such a high peak cross-section. It was sensational.

I went back and sat down to finish dinner. I don't remember what we had for dinner. But my family does; it included baked potatoes. I scooped up an enormous spoonful of what I thought was sour cream to put on my baked potato. It happened to be very sharp horseradish sauce. I sat there with a glazed look on my face, not responding to anybody, eating my baked potato while my family looked at me with surprise and puzzlement. When I finished my meal, my wife turned to me and said in an uncharacteristic, rather quiet voice, "Bj, I think you had better go to the lab now." So, off I went.

The next memory of those days was the electricity in the *SLAC* auditorium on Monday morning when the results were publicly announced. In the theory group there was a continuous workshop organized. Most of the theorists worked on interpreting the data, classifying the theories and trying to help, in whatever way one could, to expedite the experimental development of the subject. I remember the cathartic nature of the enterprise. The lines of communication were very open. There was little thought about the usual kinds of priority and proprietary attitudes toward 'ownership' of new ideas. The activity was extremely intense and very exciting. It was this way not only at *SLAC*, I am told. At many institutions it was somewhat the same way; the psi just electrified everybody. There was no question that it was important and a great turning point right from the start. Nevertheless, there was no instant consensus about what it all meant. Certainly, charm was the leading candidate from the start. At the time I quoted 50-50 odds that it was charm. But 50-50 is not good enough. One had to hack away at all of the different hypotheses before being sure. The 'hidden color' hypothesis was maybe 20% probable. Others thought the psi was an electroweak intermediate boson — that was a serious proposition!! It reminds me of the notion of the zeta being a Higgs. The $\psi = Z$ idea didn't take long to eliminate, along with more bizarre ones. One idea was that psi was a bound state of the omega baryon with its antiparticle. That interpretation lasted for even less time.

Anyway, there was a long list of theories, and it took a while to sort through all of the new background noise, if you like, and really confirm charm by finding the D. There were two more years of confusion before the D was found, and in looking back on that period, the real question is why did it take two years? Part of the problem was that for the e^+e^- data one expected a large fraction of the total cross section to be charm. Therefore there should have been an increase above $D\bar{D}$ threshold in the number of strange particles and an increase in multiplicity, but not as big a ΔR as observed. All of these things did not occur because of the tau lepton; there was this crazy accidental coincidence of having the tau threshold right underneath the charm threshold. It was just the luck of the draw that created that kind of confusion. Also, the radiative transitions from the chi states were late in coming in. And there were no reconstructed Ds found in the sample for more obscure experimental reasons. In addition to all this there was experimental background noise coming from elsewhere in the country — things like the high-y anomaly, same-sign di-leptons, tri-leptons, etc. (This is evidence that our superheroes on the experimental side sometimes also contribute to the background as well as to the signal.) All of this stimulated all sorts of exotic gauge theories that went well beyond what John Iliopoulos had laid out in the summer of '74. But once past '76, the situation rapidly consolidated. The charm discovery, of course, was a major step forward. So also was the consistency of the value of the weak mixing angle found in all the neutrino experiments. For *QCD*, part of the progression was just getting used to the ideas on confinement, which were very novel. After people talked about chromoelectric flux tubes a lot, they began to believe it all. It is an example of 'Drell's theorem,' which states that anything that is repeated three times is true.

Other observations, such as the copious yield of high-$p_\perp$ pions at the *ISR* and the scaling violations, started pushing the theory towards *QCD* rather rapidly. The final solution to the electroweak problem was largely contributed by the beautiful *SLAC* polarized electron scattering experiment. It really established for most people the great credibility, if not the proof, of the standard electroweak model at low energies. That did not include me, although by now the W and the Z discoveries have taken care of that. For *QCD* the gluons of *DESY* plus the hard collisions at the $SP\bar{P}S$ have done the equivalent job.

This leads us to, say, 1980 or thereabouts when the standard model became firmly set in place, with the balance completely changed. In going from 1976 to 1980, the balance tilted so far that in fact the weak and the strong interaction theories have taken their place, along with *QED*, as edifices which we marshall our strongest weapons to attack. So far, the edifices have stood firm. The latest round of experiments have been so successful that it is hard to distinguish the Monte Carlo 'data' in the proposals from the actual data that come out of the experiments. We are getting used to a situation where one designs experiments to verify the theory or try to kill it, rather than experiments which are grossly exploratory. It is a situation which, relative to the 1960s, lacks all of that splendid confusion.

But what about the future? I think the balance is going to swing back. We have the Higgs problem, which leads us out beyond the standard model. I think the problem with Higgs is very much like the problem with the strong interactions in, say, the 1950s. We know there is a force. We at least know something about the range of the force, although we don't even know as much about the range of the Higgs force as we did about the strong interaction force in the '50s. For a given range, we know the coupling strength. But whatever the uncertainties may be, almost everybody is sure Higgs forces do exist.

So, I think that what is in our future is a new adventure in confusion. For a long time the evidence that can be uncovered about the nature of the Higgs sector is likely to be small compared to the number of hypotheses bandied about upon what it really is — too small for decisive conclusions. We will be forced back into the mode I remember so vividly in the 1960s — one with a great variety of hypotheses, a great variety of approaches, a great uncertainty as to which approach is going to win and which one isn't, and a great uncertainty as to which energy scale is going to provide the key to the solution. It may be as surprising as in 1974, when 3 GeV in the center of mass for e^+e^- was sufficient, and when an unfashionable experiment at an old, antique laboratory like Brookhaven was a big key to opening up the future.

So in closing, I hope that as we look forward to these next ten years preceding the commissioning of a new super-machine, they will be just as rich and fruitful as the last ten years have been. In order to ensure that, I think we have to protect the variety and the richness of techniques, instruments, and experimental approaches in order to maximize our chances for happening upon the key to the next revolution.