A SUBNUCLEAR PHYSICS MANIFESTO[†]

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INTRODUCTION

I want to immediately reassure you that I do not intend to summarize the twenty plenary talks you have heard over the last week with the eight parallel session summaries you have just heard thrown in for good measure. In fact, if you read your program carefully, you will see that I was not asked to summarize anything, but rather to give a "Conference Overview and Outlook." Although I do not really think I am old enough to be asked to do such a thing, I agreed because this public task resonated with a private assessment in which I was engaged already. (This soul-searching has been triggered by taking on a challenging new job in the last year, experiencing the birth of a new son in the last month, and anticipating the observation of a new birthday during this conference!) Of course there was one immediate problem with this assignment: an overview requires distance, while an outlook requires proximity. You may have noticed that I solved this problem by watching the first half of the conference from Virginia!

I have to admit that there was a second not unrelated reason I agreed to give this talk. A surprising number of people, including many in this room today, have related to me their experiences of being told by a well-meaning High Energy colleague:

"Why are you doing that messy stuff? The real physics is at the High Energy Frontier: the search for the Higgs and supersymmetric particles, superstring theory, the challenge of building Superconducting Super Collider detectors, and so on! What you are doing is old hat: we solved all those problems already!"

That makes me mad!

So now you know: I agreed to give this talk in a fit of misty-eyed bad temper. Standing here, I suspect that I made a big mistake, but since it seems to be too late to do anything about it now, I will try to minimize the damage by at least being brief.

A SUBNUCLEAR PHYSICS MANIFESTO

I want to address three critical questions for our field: who are we?, where are we going?, and how do we get there?

WHO ARE WE?

There are many facets to this question. In the first place, we might answer it by examining our roots: we and our High Energy and Nuclear Structure colleagues are all children (and grandchildren) of nuclear physics:

Fig. 1. Our family tree

Let me note in passing that this tree has grown so tall and strong that its head is now in the clouds. However, my main purpose in presenting our family tree is simply to make the point that ours is a completely respectable heritage!

Having presented our family history, we might next try to answer "who are we?" in a more substantial way by explaining what it is we do: what is our game? In answering this question I believe we must take a very broad perspective. To help me do this, I transformed this question into another: what will we tell our grandchildren we did? (I chose to skip a generation to imply a time scale of decades and a child to emphasize that the issues involved should be meaningful to a bright young student or any other intelligent member of the public. If we cannot explain what we are trying to do to such an audience, then our field should and will die.) What follows is a list of some possibilities with:

1. A 10 - 30 year time-frame,

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2. No dollar constraints, so any technically feasible machines and detectors are assumed available,
3. Beam energies in the 0.1 - 100 GeV range, and
4. Allowance for some good luck.

In more or less random order, I hope to tell my grandchildren that:

1. We tested QCD.

We made precision tests of QCD predictions based on perturbation theory, lattice theory, chiral perturbation theory, and heavy quark effective theory. We made qualitative new tests by discovering hybrids and glueballs, understanding the $\Delta I = \frac{1}{2}$ and OZI rules, etc.

2. We established the quark-gluon basis of nuclear physics.

We understood why a nucleus behaves like a collection of A nucleons, and were able to derive the nucleon-nucleon force from QCD.

3. We developed powerful models of hadronic structure.

We developed a good qualitative picture of the basic internal structure of the proton. Our models also allowed us to understand the QCD vacuum, baryons, mesons, nuclear matter, multi-quark states, etc., and to map out the duality between quarks and hadrons.

4. We discovered failures of the Standard Model:

We finally found phenomena which the Standard Model cannot explain by making precision electroweak measurements, by discovering some forbidden rare decays, and by studying in detail CP violation in the *s*- and *b*-quark systems.

5. We accurately probed and understood simple nuclei.

We pushed the nucleonic picture of the nucleus to its extreme in a few simple cases, saw the picture break down, and understood how to go beyond it.

6. We understood the strongly-interacting many body problem.

We explored the properties of nuclear matter under extremes of density and temperature, and discovered not only the quark-gluon plasma but unexpected new cooperative effects.

7. We determined all of the basic physical constants of the Standard Model.

We determined the masses and mixing angles of all the quarks. Then we discovered that neutrinos had masses and we measured all the lepton masses and mixing angles, too. With this information, a new theory of the origin of mass was developed.

8. We made new tests of fundamental principles.

We tested more accurately than ever before such basic principles as relativity and quantum mechanics.

Of course this list is incredibly optimistic, but I would be proud to say we accomplished even a small fraction of these goals, and not sorry to have tried to accomplish the rest.

It is apparent that this list is extremely rich and diverse in physics. It also includes topics in which there is real potential to see beyond the Standard Model. Also note that most of the issues in the list are extremely relevant to how the world works at a tangible level. Finally, it should be mentioned that the sociology of the experiments which are required is still on a "family" scale: there are lots of "spigots", allowing humanely-sized collaborations.

The third aspect of who we are seems trivial compared to the last, but I believe we can continue to ignore it only at great peril: what is our name? What is in a name?

Fig. 2: What is our name?

Sometimes a great deal: the names of Fig. 2 define us to be on the margins of fields of physics. If you live on such a margin, you must constantly *beware of chasms!*

I don't really care what we call our new field so long as it is not a self-marginalizing name. In an informal poll I have been taking, a popular suggestion has been "Hadronic Physics." This has certain advantages:

- a) the nucleus is a hadron,
- b) it would include the study of the weak interactions in a hadronic environment, and

- c) it avoids the use of the N-word.

It also has some disadvantages:

- a) it introduces a new jargony word which is foreign to the public,
- b) it excludes purely leptonic issues (e.g., $\mu \rightarrow e\gamma$), and
- c) it avoids the use of the N-word.

My favorite suggestion is "Subnuclear Physics." Its advantages include:

- a) it is inclusive with respect to energy (i.e., distance) scale,
- b) it can include nuclear structure physics, and
- c) it uses the N-word

while its only disadvantage is that it uses the N-word. Although this is my favorite, anything less *middling* will do. For example, I understand that the Organizing Committee of this conference independently worried about this same issue on Sunday and that "Quark and Lepton Physics" was suggested.

WHERE ARE WE GOING?

To a first approximation, we are our machines, so where we are going is to this order defined by our success at machine building. Among the machines which are built or nearly built are LAMPF, PSI, TRIUMF, IUCF, Cel-sius, Saclay, MAMI, BNL, KEK, CERN, FNAL, Serpukhov, Bates, Bonn, SAL, NIKHEF, VEPP, SLAC, CEBAF, DAΦNE, and various neutrino observatories. It is an impressive list. However, our field will bog down rapidly without new machines like RHIC, an Intense Hadron Facility (like KAON), a *B*-factory, a τ -Charm Factory, SuperLEAR, etc. At the moment our prospects for having these new machines are very uncertain.

HOW DO WE GET THERE?

I believe that a prerequisite for our success in obtaining the new machines we need is a dual realignment: one intellectual and spiritual, the other intellectual and political. Almost all of us consider ourselves either nuclear or particle physicists. We have to start thinking of ourselves instead as (for example) sub-nuclear physicists. Those of us coming from a particle physics background must (while hoping for surprises) accept that the next few decades will most likely be a time for consolidating the advances made by the Standard Model: we must redefine our goal as being primarily the exploration of the phenomena of the Standard Model and not the discovery of new basic laws. This involves a big change in attitude: we must accept complexity into our lives and prepare to get our hands dirty. The reorientation required of those of us with a nuclear heritage is no less difficult. We must first of all realize that the Standard Model now defines most of the interesting phenomenological issues. This involves a big change in attitude: we must avoid paying attention to things simply because

they are not yet understood. In other words, we must try to keep our hands clean!

In addition to this spiritual realignment, we must accomplish a political (i.e., funding) realignment. The hegemony of the Standard Model has made the old division into Nuclear Physics and Particle Physics obsolete. It is now more appropriate to divide the field into Subnuclear Physics and Superphysics. The primary goal of the latter is to discover the next Lagrangian ("The Golden Lagrangian in the Sky", as Ken Johnson calls it), *via* supersymmetry, superstrings and supercolliders. This is a laudable ambition, and we should all support our colleagues who are pursuing this goal. However, the primary goal of Subnuclear Physics is almost orthogonal: the exploration of the phenomena of the Standard Model. These two qualitatively different approaches would in the 1960's have properly characterized the main difference between Nuclear and Particle Physics. Today the major part of Nuclear Physics and a very substantial part of Particle Physics share common goals and it is inefficient of resources and politically foolish for them to remain separated.

OUTLOOK

The future of our field is not necessarily rosy no matter what we do, but there is a great deal we can do to improve its prospects.

We must certainly be aggressive in defining our new field. At the moment it is too diffuse: we must give it a focus (by recognizing and agreeing upon its key issues) and we must insist upon admitting to the field only endeavors which meet the highest intellectual standards. In doing this we will have to overcome the substantial intellectual, spiritual, and political barriers which divide our community. It is imperative that we succeed in this realignment, because our physics is real, it is excellent, and it is important. However, success is not assured, and if we don't change we may very well not have a story to tell.