

COMMENTS ON CHANGING SOCIOLOGY IN PARTICLE PHYSICS

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An evening session was organized on "sociology" by Dick Gustafson, Bob Lanou, and Dave Pellet (users organization chairmen from Fermilab, Brookhaven, and SLAC); about 75% of the attendees from graduate students to all the laboratory leaders participated. Four short provocative presentations were made by the users chairmen plus Al Abashian, leading to animated discussions.

The first point made was the changing style of particle physics; many fixed-target experiments and groups are tending to a few collider experiments. The timescale for these is greater than five years. This is longer than a post-doctoral period, longer than an assistant professor tenure trial period, longer than a contract review renewal period, perhaps longer than a (good) graduate student's attention span, and maybe longer than one's own intellectual attention span. In addition, the size of groups is tending towards $50 \times n$; this implies a personal dilution of experience, growth, and role in the "enterprise," plus a diminution of stature in the eyes of the university community. The effects of these forces are seen as driving the field towards a conglomeration of people, groups, and laboratories--thus driving the participants from the university community out of the business. It was then asserted that the universities are the reproductive organs of the field and that judging by the amount of greying/white hair in the discussion room we could already see the effects of these forces; we are a very "mature" field. The field can just wither and atrophy of unnatural causes if the input in quality and numbers are not forthcoming, with no obvious alarms.

Bob Lanou reviewed some of the available demographic information about the field (mostly from the Sullivan report of 1978 and some preliminary work by Herman Feshbach on the Trilling Committee). He concluded we are probably barely reproducing the field at about 120 Ph.D.'s per year. Are we attracting the "best" students and then are we keeping the best of those? Many people felt that we were not. They also felt uneasy about our long-claimed position as the intellectual cutting edge of science. Something about the factory atmosphere of big-group organization science belies this illusion. Lanou appealed for an in-depth study of our manpower to find:

- Statistics
- Any new, recent trends in theoretical and experimental personnel
- Job futures for new people (tenure)
- Role of post doc in future
- Projections of quantity for facility utilization
- Trends in university participation and effectiveness
- Are the best young people (not) entering the field?
- If so why and what can be done about it?
- What can be done by universities and laboratories to reduce the number of years to get an experimental Ph.D.?

The role (exploitation) of post docs and graduate students was brought up; 25% of the high-energy physics manpower (highest science) is post doctoral, and a large fraction of these eventually leave the field. The situation in theoretical post docs is worse. Lederman opined that the export of our expertise by this means is one of the great things we do for the financially supportive society. Although most agree with this, there was a general discomfort with the reality of the personal agony (in grinding up of people to make our mortar) that occurs when two-thirds of the people getting to high-energy physics in graduate school are "deflected" to other careers sometime between graduate school and the end of "first post doc."

During a somewhat unreal point in the discussion, Jim Volk (the token graduate student) at the meeting leapt up and animatedly told the "Pooh-Bahs" of the field what it was like in this "deflection" process:

- no student leaves the field voluntarily
- he asked why he was the only graduate student at the summer study
- no student believes a Ph.D. takes four to five years; it depends on when one starts counting
- reality of pulling cables, bolts, chambers in isolation from university environment
- putting up with follies of the weekend warriors who come to visit
- the need to involve younger people in the field, especially more students, post docs, etc., in plans, dreams, schemes.

A number of subjects were ducked by the study itself. Many of these were discussed at the evening session or touched upon in workshop sessions or private discussions; we here mention some of these.

Many people have been concerned with the lack of influence of the university user community in the laboratory programs and in directing the larger U. S. particle physics program. A suggestion was made that a "voucher system," funds given to the laboratories through the user groups, would instantly rectify this situation. It is likely that even a partial version of this would have a large effect. At present we see the opposite trend with funds partially given through the laboratories for building detectors with expected effects. Related other suggestions involve university groups taking on responsibility for major accelerator systems and their functioning through the agencies. Others (including some users) reply the university groups are not up to or ready for such responsibility (unreliable dilettante weekend warriors).

This Summer Study has clearly made a giant step in the direction of national community participation in planning for the U. S. program. Yet even here we have avoided planning but rather done ground work for someone/group to do the planning.

The slippery subject of planning itself got no direct attention but was alluded to continually.

Planning is establishing the Facility and Resource Allocation direction of the field. Hopefully it is driven by the physics opportunities; in the real world this gets modified by these considerations:

- money
- pork barrel
- survival imperative (at both laboratory and university group level)
- competition
- geography
- and strong personalities.

In recent times DOE/Wallenmeyer has acted as executive, balancing the forces of government (\$) with demands and opportunities using inputs perceived through review, personal contact (lab director dominated), HEPAP, and Woods Hole committees, subpanels, etc. This process has evolved out of the World War II project days and clearly works; however, a number of people have felt unrepresented in this process; the university community even though represented, on HEPAP, for example, have been relatively fragmented. To some it appears the representation is more in the manner of the Eastern European version of socialism. It has been suggested that the users organizations of the principal laboratories (where the field is actively practiced and discussed) could nominate/elect a few of the HEPAP/Woods Hole committee members, a small peasant revolution, bringing in the views of an active coherent part of the field.

Should laboratories live forever, preserving the carefully nurtured knowledge and cadres of professional skill and wisdom--or is it actually desirable to close a laboratory as it picks up the bureaucratic arthritis, barnacles, etc., and other symptoms of old age, making it possible for a new enterprise (laboratory with a new mission) to start anew with new leadership creating new traditions and freedom to leave old ones? These questions are perhaps related to more general ones concerning style

and management of groups, laboratories, and of the field. We have seen some managers having identifiably nearly the same position in the field for 10-20 years and seen laboratory management structures endure in the same way; other places have had almost a revolving door structure in every sense. Perhaps Lederman, Panofsky, or Samios could be persuaded to go to Washington to put in a period of orchestrating the field for maximum effect after a decent period of experience and success in their present positions.

If there is to be a new large machine there is a question of siting. It is not clear this should really be in the desert. Building an Ice Station Zebra or Novosibirsk science city is expensive both cost-wise and success-wise, and has implications regarding what fraction of the community will move to the new place and the relative fraction of time the visiting community will work there. We have a good range of examples from which to study: Los Alamos, Fermilab, Brookhaven, and CERN. The sociological questions for the future and success of the new project should be weighed comparably with the financial imperatives in this selection.

The summer study succeeded in exploring new accelerator technology and ideas, consequences of the new conventional physics, and a few new physics ideas. It also brought together a good sampling of the individuals involved in current and near future high-energy physics enterprises. A large number of ideas and opinions were exchanged and partially reconciled. The DOE and NSF agencies both had participants/representatives involved in the study, mixing with the participants. There was a fair balance between the politicians, who came with axes to grind, papers in their pockets, and who worked popularizing their causes, with those who explored new ground and ideas. All the participants left Snowmass with a greater sense of the high-energy physics scientific community, their role in it, and their goals for it. The study was a tremendous success for the field.