SSC-SR-1009

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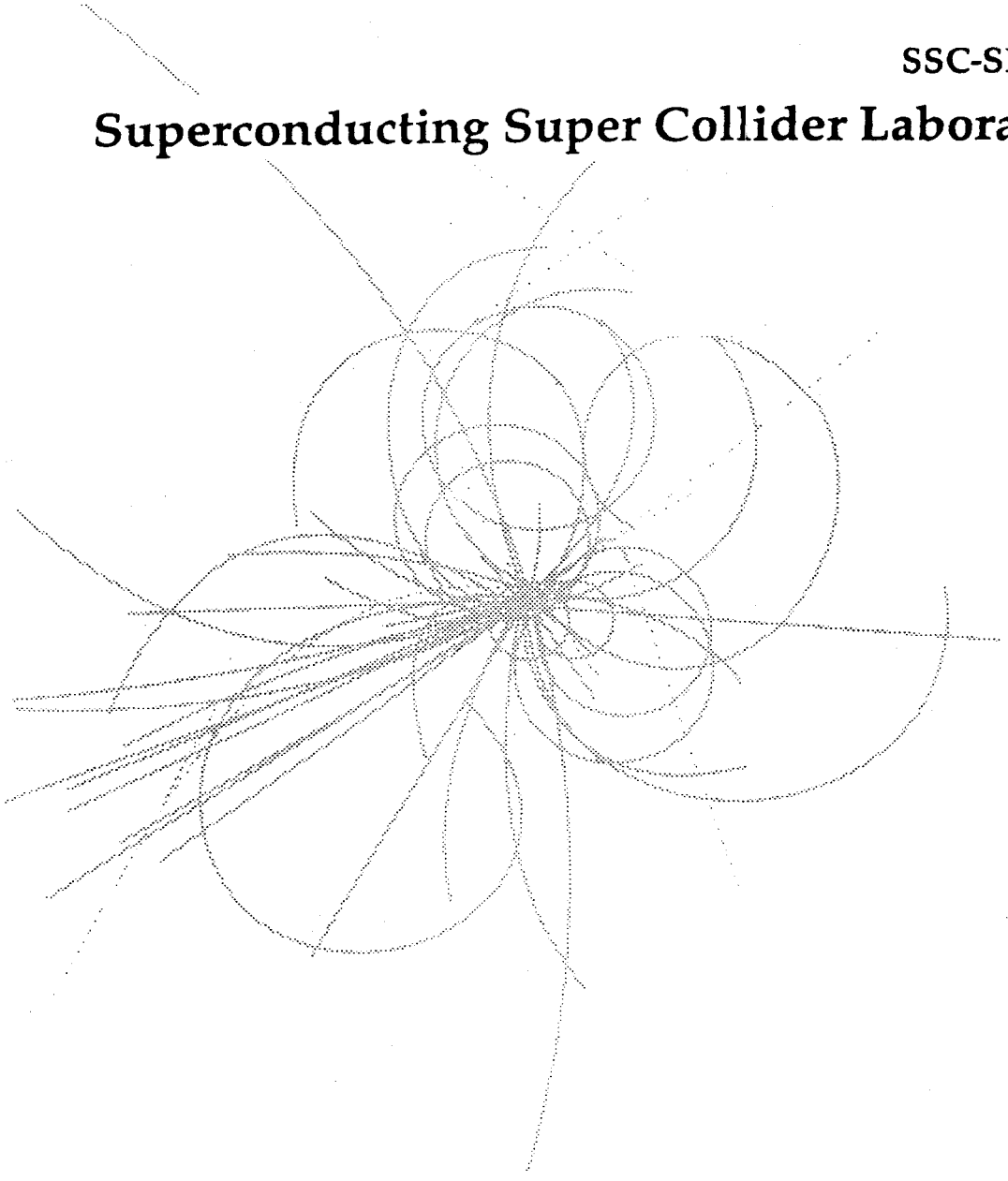
Superconducting Super Collider Laboratory



Preliminary Report from the Task Force on Detector R&D for the SSC

May 1985

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from the

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May 23 , 1985

Introduction

Our Task Force has made a preliminary study of detector R&D needs for the SSC in response to the following charge:

> The Task Force will advise on the detector R&D needed to assure the timely construction of detectors capable of exploiting the luminosity and energy of the SSC at turn-on. In addition, the Task Force should estimate the manpower requirements, costs, and time scale for this R&D. The Task Force should also recommend the procedures to be followed to accomplish the tasks.
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> 1. There should be a preliminary report on the R&D objective, manpower requirements, costs , and time scale in time for the May 24 HEPAP working group meeting at SLAC. If there are particularly pressing problems needing funding in FY86, these should be identified for HEPAP.
> 2. It is expected that some part of the 1986 Snowmass Summer Study will be devoted to detector problems. The Task Force is requested to provide recommendations on the optimum way to structure this effort.
> 3. Recommendations are requested regarding the scope and costs of the SSC detector R&D to be done in FY87.
> 4. What role, if any, should the SSC Central Design Group play in facilitating this detector R&D?

The Task Force(see Appendix A for a list of participants and meetings) began less than one month ago and therefore our conclusions are highly preliminary at this point.

Essentially all of the members of the Task Force agree that in general the best detector R&D is done within the context of an actual detector design with well defined common goals. However, since the initiation of detector proposals and construction for the SSC is unlikely to start within the next few years, we have identified those items(workshops, study groups and

real R&D) which could substantially improve our understanding of detector requirements for the SSC.

Our overall conclusion is that a relatively modest investment of 4-5M$ per year for SSC detector related R&D and workshops could significantly enhance the chance of a timely implementation of the complex new detectors required for the SSC environment. In some cases, eg. electronics, investment in R&D in the next few years could save millions of dollars in final construction costs. In many crucial areas basic R&D will be required to define detector parameters or in some cases(tracking devices, for example) to assess the viability of a particular technique at high luminosity and/or high energy. New techniques will also be required to fully exploit the physics potential at the SSC. In short a successful R&D effort now will save time, perhaps as much as two years, in the design stage of detectors for the SSC.

The Task Force has so far evaluated the R&D efforts required in the following categories:

Tracking Devices
Calorimetry
Electronics
Data Acquisition
Triggering
Computing
Muon Detection
New Techniques

At a later date we will also discuss very large superconducting magnets and particle identification. As noted in some sections below there is substantial overlap among many of these topics.

We have made very preliminary estimates of required funding and manpower. The latter is given in terms of full-time-equivalents for physicists and engineers but does not include technicians. Funding does not include salary support for physicists or engineers unless specifically noted. We emphasize that our estimates are highly preliminary; better estimates will require additional work and eventually submission of specific proposals.

Workshops and Travel Funds

In the sections below there are many recommendations for specific workshops and study groups. We would like to emphasize that well focussed, relatively small workshops and study groups tend to be more productive than large multi-purpose efforts. The success of specific-topic workshops depends not only on the organization and location, but the ablility to attract experts in the particular topic under study. Such experts could be more easily attracted if travel funds for SSC workshop and study group participation could be provided directly through the CDG. This particulary applies to non US physicists.The funds required clearly depend strongly on the number of workshops and study groups but could amount to 200-300K$ per year.

Organization of the R&D Effort

We have primarily discussed organizational matters in response to item 4 in the charge to our Task Force. Our preliminary conclusion is that the Central Design Group for the SSC should be involved in some of the organizational aspects of the detector R&D effort for the SSC. The involvement of the CDG potentially has the following advantages:

(1) Continuity and coherence in the R&D effort, particularly workshop type activities.

(2) A means to influence the budgetary process.

(3) To act as a full-time advocate of detector R&D for the SSC.

(4) To act as a source of information for the community about R&D needs.

A possible model is to have one or more members of the CDG facilitate detector R&D in consultation with the funding agencies and the community. The CDG member or members should be advised by a permanent Detector R&D Advisory Committee to help coordinate workshop activities, to assess funding needs on a yearly basis and to fight in general terms for such funds. It seems likely that substantive R&D will only proceed if members of the community are willing to submit proposals for specific items of research. The exact mechanism for review and support of such proposals should be worked out by the CDG and the funding agencies. However, some

workshops and associated travel could and should be funded directly by the CDG.

Industrial Involvement

There are substantial opportunities for close and productive industrial involvement in the detector R&D effort for the SSC. Some examples include

(1) custom integrated circuit development
(2) production of radiation hardened electronics
(3) the design and implementation of very high rate data acquisition systems
(4) the development of very powerful multiprocessor computer systems
(5) development of new solid state detectors
(6) large superconducting magnets

item (4) deserves particular mention since it is possible that the demand for computing at the SSC will require the development of multiproccesor systems and related software that could be at the frontiers of computer science and therefore have an impact outside high energy physics.

International Collaboration

The involvement of non-US physicists in detector R&D for the SSC is highly desirable. We recommend that Canadian, Japanese and European physicists be involved directly in organizing and participating in workshops and study groups whenever feasible. In some cases joint hardware development efforts can reduce duplication and save money and time. The resultant transfer of experience and expertise can only help the R&D effort. As noted above this would be greatly facilitated if travel funds could be provided when needed for non- US visitors.

Impact of Detectors on the Design of the SSC

At the Snowmass '84 meeting a number of areas were identified which potentially impact the design of the SSC. These include the appropriate collision frequency,the clustering of intersection regions and test beams

derived from the 20 TeV ring. The conclusion from the Snowmass meeting was that a time between collisions of 10ns was to be preferred to longer times at the highest luminosity. We recommend that this topic be studied in the relevant study groups recommended later, triggering, tracking and calorimetry. Also of importance is the relation between clustered IR's and muon background. Better calculations are needed to assess spurious trigger rates for clustered IRs. Finally there was a strong request by the Snowmass Calorimeter group that multi-TeV test beams derived from the 20 TeV rings be made available for calibration and testing. This should be reevaluated. We would like to point out to the CDG that these are serious issues for the accelerator complex and should receive attention in the overall accelerator design.

Time Scale of the R&D Effort

The appropriate time scale for detector R&D obviously depends on the the particular item of study and research. There is, however, an approximate natural scale for most of the R&D effort. In general the output of workshops and study groups on generic R&D problems is likely to become repetitious roughly 2 years from now. By mid 1987 the emphasis of workshops should shift to very specific detector component problems on one hand, and overall detector/experiment design on the other. Assuming that funds and manpower become available starting in FY87, a generic R&D effort will become less and less fruitful by FY89 and beyond. After two years of substantive generic R&D, one must begin to focus on specific prototype designs and tests to make real progress. As noted before, usually the best detector development is done in the context of an overall design. For large detectors of the approximate scale of theose discussed at Snowmass '84, the total time for design, construction and installation is likely to be 5-8 years. The sooner real detector design and construction begins, the more likely detectors will be ready at the turn-on of the SSC.

Tracking Devices

Studies of the expected tracking environment at the next generation pp colliders, first done for the CBA and more recently for the SSC, have pointed our the shortcomings of present charged particle tracking techniques at high luminosity and high energy. Some of these problems are discussed below.

What Chamber Performance is Required?

This is a large question, but it deserves continued attention so that we can understand what technical questions should be addressed. The answers in the near future should be specified in terms of the following parameters:

- spatial resolution per measurement
- spatial track-pair resolution
- temporal track-pair resolution
- maximum drift time

The physics we want to study and the machine environment will determine the required chamber parameters. Both sorts of effects can be studied by Monte Carlo without a full detector simulation or real tracking code. These parameters should be investigated for all aspects of the tracking problem; vertex detectors, central and forward tracking. One could get a reasonable estimate of tracking efficiency, suppression of out-of-time tracks, etc. A preliminary look at this subject might take about 6 months of part time work for a study group of about 6 experienced people. It would be useful to initiate such a study group, if possible, this year with the goal of reporting their conclusions and recommendations at the Snowmass '86 meeting.

What Chamber Performance is Possible?

The development of a new generation of central track chambers and vertex detectors for the SLC, LEP and the hadron colliders has advanced the art of wire chambers considerably. Especially relevant to the SSC are improvements in track pair resolution and spatial resolution, control of systematics in large chambers and construction problems in building large chambers. It would be useful to summarize these advances at Snowmass '86.

Radiation Damage to Wire Chambers

An effort should begin as soon as possible to address the problems associated with radiation damage in wire chambers. Although there are various recipes for chamber survival, there is little coherent understanding of radiation damage mechanisms. Many questions are worth investigating. Examples are; what is the maximum instantaneous current draw that can be supported before breakdown in the commonly used gases: does it depend on gain or cathode geometry; does it depend on wire surface quality; can results from single wires be extrapolated reliably to large wire systems; and can additives to commonly used gases improve radiation resistance?. It is likely to be crucial to high luminosity operation at the SSC to attempt to find the underlying mechanisms for radiation damage and to try to develop cures.

The required work could easily require two or more groups of 2FTE each plus substantial technical support(in part to construct chambers) working for two years. Yearly budgets for each group would be in the 100-200K$ range or more if substantial new chamber construction is required. Input from areas outside high energy physics, such as organic chemistry or atomic physics, might be very useful.

It would be very useful to have a small workshop before the Snowmass' 86 meeting to collect the current folklore on this problem. If nothing else such a workshop would allow work, much of which is unpublished, to be collected in a single document. Such a workshop would require substantial preorganization. One might also consider having such a workshop as part of or before/after the 1986 Vienna Wire Chamber Conference.

Wire Chamber Readout

Several electronic developments might help alleviate problems with wire chamber readout. The limits of high sensitivity and fast risetime front-end electronics should be explored to allow chamber operation at lower gas gain. Advances in waveform sampling could improve two-track resolution. It might also be possible to develop special cicuits to aid in pattern recognition in an overlapping event environment and to reduce the amount of data to be permanently recorded. Some of these developments,

such as better waveform sampling, will likely proceed without dedicated R&D funds. The estimated cost of the development of custom circuits is given in the Electronics section.

Silicon Detectors

Silicon microstrip detectors are developing at a rapid pace for LEP, SLC, the hadron colliders and fixed target experiments. Three questions have particular relevance at the SSC; (1) radiation hardness of the detectors; (2) radiation hardness of the attached electronics; and (3) the detection element geometry necessary for the SSC eg. 2 vs 1 dimensional readout. The answers to some of these questions may be forthcoming from ongoing work but it is likely that new R&D will be required to enhance radiation hardness and to understand the geometry requirements.

Recommendations for Tracking Devices

(1) Form a study group to investigate pattern recognition, required device parameters and related problems for tracking at high luminosity. This group would report on their activities at Snowmass '86 but should begin their study by the end of this year. Required level of support: travel funds, very modest computer time and about 6 people involved part-time.

(2) Encourage proposals starting in FY87 by one or more groups to systematically investigate radiation damage problems in wire chambers. Encourage the formation of a small workshop in 1986 to bring together current experience on this subject. Required support: funds for the workshop, 100-200K$ per year for each group involved in radiation damage studies and 1-2 FTE per group.

(3) Starting in FY87 encourage the development of custom chips for wire chamber and silicon micro-strip detector readout - see the Electronics section for required level of support.

(4) Radiation damage to silicon detectors should also be investigated. Existing programs should be extended for these tests when possible. Required support: 100K$ total and beam time.

Calorimetry

It is quite likely that many physics results at the SSC will depend directly on the quality of the calorimetry used in detectors. Furthermore calorimetery will provide the primary trigger mechanism at the SSC. For these two reasons studies of calorimetry should rank near the top for any SSC detector R&D program. The major areas requiring work are:

(1) Continued studies of the required calorimeter parameters including trigger simulation by Monte Carlo studies
(2) Evaluation of existing or forthcoming data - what is possible?
(3) Tests(including beam tests) and evaluations of specific techniques that are required for the SSC environment
(4) Development of reliable hadron shower simulation codes(this may require new measurements to be made)
(5) New ideas in calorimetry

Some of these items may be fruitfully investigated in workshops or study groups but many will require hardware development and tests.

Monte Carlo Studies

Some of the work to understand the optimum calorimeter parameters was started at Snowmass '84 and should continue. Crucial parameters include energy resolution, angular resolution, size of dead regions, time resolution, etc. Since many of these items have been studied before, further Monte Carlo investigations must be well directed to avoid rehashing old results. Also important are studies of the effect of event overlaps on rare event recognition, lepton identification and missing P_T tagging. Again a few studies have been done at past workshops, sometimes with conflicting results. A systematic understanding of the overlap problem, including the utility of time tagging of calorimeter signals, is definitely needed. Since

overlapping events can influence the choice of calorimeter parameters, it would seem most fruitlful to combine the efforts of establishing parameters and investigating event overlap problems. Answers to these problems cannot be obtained at a meeting like Snowmass '86 alone; substantial prior work must be done and adequate computer resources must be available for participants in the study.

Exchange of Information

Many of the large detectors currently under construction or being upgraded will involve state of the art calorimetry. By summer of 1986 there will be substantial new information from prototype tests or in situ running from many different types of calorimeters. An exchange of information regarding

- required depth for shower containment and optimum resolution
- albedo
- compensation

would be useful.

Hadron Shower Code Development

The existence of a hadron shower code equivalent in reliability to the EGS code for electromagnetic showers would be a powerful and useful tool for calorimeter development and parametric studies. The improvement of existing codes and the development of new codes should be encouraged.

Hardware Research and Development

There are a number of items that should receive study in the next few years. These include

- fast response calorimeters
- radiation damage to sampling media
- new sampling media(see the discussion under New Techniques)
- extension of present prototype programs to the SSC scale in absorption lengths and energy

Some of these items are already being investigated but the efforts applicable to the SSC should receive additional support starting in FY87. In particular the extension of existing prototype tests by adding more

instrumented absorber and by testing at high energies at FNAL should be strongly supported. Groups involved in calorimeter testing should be encouraged to submit proposals for this purpose. If properly funded and implemented, this would be a very cost effective way of inititiating prototype testing for the SSC energy range within the next few years. We estimate that a substantive program of extension might require 400K$ per year not including costs of operating beam lines.

Test Beams for SSC Detector Development

A serious problem for detector R&D for the SSC is the potential lack of beam time in test beams, particularly at FNAL. This would primarily affect calorimeter development , to a lesser extent, muon identification studies and some radiation damage studies. We note that the Snowmass '84 study of test beams showed that a major detector can effectively employ most or all of a test beam for many years. In fact present large detectors under development are already each in need of a dedicated test beam. A serious R&D effort would also require substantial beam time starting in FY87 or FY88. We therefore suggest that a brief study be made of test beam availability and requirements for FY87 and beyond. Recommendations should be made as to future direct support of test beam time for SSC detector development.

Recommendations for Calorimetry

(1) Additional Monte Carlo studies of calorimeter requirements and the impact of overlapping events should be done before and during the Snowmass '86 Summer Study. To some extent this should be coordinated with trigger studies recommended later. A strong effort should be made to avoid repitition with previous work. Required support: travel funds and a modest amount of computer time.

(2) An exchange of information regarding recent prototype tests should be organized for Snowmass '86. Required support: travel funds.

(3) The development of reliable hadron shower simulation codes should be encouraged. Required support: computer time, travel funds and 2FTE per effort.

(4) Continue the development of fast response calorimeters. Required support: 200K$ per year (including current support) test beam time and 2FTE per effort.

(5) Radiation damage studies should be continued. Required support: 100K$ per year(including present support) and 2FTE.

(6) Current prototype tests should be extended when possible to longer absorption lengths and higher energies. Required support: 400K$ per year, test beam time and 4FTE.

(7) The availability of test beam time for SSC detector R&D should be evaluated in the near future and recommendations for future needs should be made.

Electronics

The three generic detectors which were studied at Snowmass in 1984 provide a basis to set the scale of the detectors which will be proposed for the SSC. This scale is a total capital cost of approximately 250M$ with the electronics and the data acquisition system representing approximately one third of the total. There are typically 500K or more electronic channels in these detectors and, if present day techniques were to be employed, the cross section of the cables coming from the central tracking system alone would be one square meter which would necessarily be inert as far as the calorimetry is concerned. The potential payoff in cost savings and in reduced complexity by employing custom VLSI integrated circuits is very large. There are several problems in realizing these savings in the actual SSC detectors which a modest R & D effort over the next two to three years could hope to solve. These problem areas are:

(1) Radiation damage in presently available commercial CMOS devices.

(2) Lack of people trained in using semiconductor industry standard CAD tools.

(3) Very limited experience within the field in producing VLSI prototype integrated circuits.

(4) Limited mechanisms for incorporating industrial participation in finding solutions to our problems in electronics.

(5) Development of simulation techniques to predict the performance of these complex systems in the very high data rate environment of the SSC.

Solutions to these problems primarily involve enhancing our expertise to a level whereby we can at least ask the right questions and have some hope of understanding the answers to these questions.

SSC detectors will be large complex devices and if they are to be successful, a much higher level of systems engineering will be required compared to present day large hadron collider detectors, e.g. CDF, DO,UA1 and UA2. This includes both the software and the hardware with the distinction between electronics, data acquisition, triggering and computing becoming very fuzzy in detectors of the SSC class. Every effort should be made to optimize the complete system.

The last paragraph unfortunately does not provide much guidance in getting from here to there. The following topics are specific proposals which can be started now and which will be very useful in developing a successful SSC detector.

Electronics Workshops

An appropriate topic for one of the sections of Snowmass 1986 would be to try to specify the detector systems well enough so that a moderately detailed plan could be laid out for the electronics, data acquisition, triggering and online computing. From this plan specific areas could be identified which would have the largest payoff for immediate R & D.

Examples might be an integrated multihit sensitive drift chamber TDC, fast trigger processors, and a fast switching network to parcel out the work of the level three trigger. Again substantial preorganization would be required to implement a useful workshop.

Before Snowmass 1986 a workshop or school needs to be organized with the specific goal of training interested physicists planning to work on SSC detector development in the art of custom VLSI chip developement. The focus of the workshop should be a hands on tutorial using semiconductor industry standard CAD systems. As a followup to this workshop funding should be provided for purchase of the necessary CAD workstations and software by at least two developement centers which could be at the major laboratories.

An additional area where a workshop would be very useful is radiation damage to VLSI circuits. Here the emphasis would be education of the interested high energy physicists as to present performance levels and likely 2 to 4 year term trends. This would be an excellent opportunity to begin a close association with industry. There will be a first pass attempt to do this during the IEEE Nuclear Science Symposium to be held in San Francisco this October. Travel support for industrial representatives would be most welcome especailly those from Japan. A similar workshop on waveform digitization was held at the Orlando meeting of the same conference last October and was moderately successful at this educational process.

VLSI Prototype Development

An excellent candidate for an initial project using the CAD systems at the developement centers would be a high input sensitivity multihit drift chamber TDC chip or chip set to enable 120 micron resolution at gas gains of 10*4. This would be a generally useful developement which could be used in upgrades to present large detector tracking systems. Once the layout is complete, the cost of producing prototypes of the circuit in Si is less than 30K$. It would be an excellent way to learn how to produce custom IC's.

Association with IEEE and ICFA

The good experience of the CDF collaboration with Japanese industry in developing a monolithic preamplifier for their drift chamber needs to be understood. Similar mechanisms need to be setup for industrial collaboration in this country. At the moment the IEEE is the principal avenue for communication in this country and that needs to be improved and expanded. The subpanel on Instrumentation for ICFA is perhaps a useful mechanism to obtain international collaboration on SSC detector developement in the electronics area. To be useful all of these will require money for travel and help in publishing workshop proceedings etc.

Recommendations for Electronics

(1) A school/study group should be initiated in the next year to educate those people interested about the techniques and possibilities of custom IC development. Required support: funds for the school and travel funds.

(2) Upgrade CAD facilities test facilities and associated hardware and software for custom chip design. Required support: 400K$ total and 1FTE(engineer) per effort. At least two such efforts will be needed.

(3) Starting in FY87 or FY88 support the development of at least one custom chip design for use on existing detectors and for the SSC environment. Required support:100K$ for development(does not include production) and 2FTE per design.

(4) A small study group should be formed within the next year to investigate, with industrial consultation, what is known about radiation damage problems to electronics and to make recommendations regarding future R&D. This might proceed after the IEEE meeting in October of this year. Required support: travel funds.

Triggering

There are a number of aspects to the triggering problems at the SSC. Most of our comments here apply to large 4π detectors but we note that triggering requirements for more specialized experiments may also be stringent and deserve additional study by interested persons. The triggering problem naturally divides into two pieces; a fast analog trigger to reduce the basic interaction rate by a factor of 1000 or more and "higher" level triggering(probably in multiple pieces) which involves manipulation of digitized information. The first level trigger is primarily based on calorimetry and somewhat on muon detection. Such triggers have been studied at the DPF Workshop at Berkeley in 1983, are under construction for CDF, under design for D0 and working, at relatively low luminosity, for UA1 and UA2. At this point hardware R&D on this subject is not required but additional studies would be useful in the context of a more general workshop on triggering. Topics for study include

(1) calorimeter response time, noise levels and delay time

(2) required subdivision for triggering

The problems associated with higher level triggers are more difficult. The complexity of detectors and data, combined with the scale of the required trigger reduction, will require processors programmable in a high level language(call it FORTRAN) at the highest levels of the trigger chain. At this point the distinction between triggering and computing becomes blurred because these processors will run programs similar if not identical to analysis codes. At the intermediate levels the triggering problem is even more fuzzy. Do we use a few layers of triggering with many parallel processors or are many layers required with rejection at each step? Such an understanding can only come from meetings that bring together both

those knowledgeable about the physics requirements for triggering(Monte Carlo experts) and those with knowledge about what is technically feasible in hardware. To understand, for example, the requirements for microprocessors, related buffers, buses, etc sophisticated modelling of data flow should be done. It is essential that studies along these lines be carefully coordinated and involve real experts in the areas of concern. A weak effort will be of little use.

Recommendations for Triggering

(1) A substantial part of the Snowmass '86 study should be devoted to triggering and related problems including data acquisition. We suggest that Snowmass '86 be the culmination of a longer term and more extensive study beginning soon. An organizing committe should be formed in the near future to ensure complete coverage of the many related problems and to divide up the work.Every effort should be made to involve expertise from non-US physicists, particular those now working on the hadron collider at CERN, in addition to US experts. This study group should be prepared to make recommendations regarding additional work and R&D after the Snowmass '86 meeting. Required support: considerable travel funds and computer time for Monte Carlo work.

Data Acquisition

It is impossible to completely separate the problems of data acquisition for SSC detectors from electronics and triggering. The reduction and storage of the enormous amount of data produced by a large detector for the SSC will require new levels of sophistication and integration that are only now being suggested by TeV I detectors. The scope of the required systems should first be investigated in a well structured workshop in order to determine what new areas of hardware development should be pursued.

Problems areas that might be addressed in a workshop approach include:

(1) The architecture and implementation of very fast programmable

processors eg. parallel structures with much more than 100 MIP on a Fastbus board. Such devices might be needed to reduce "raw" data or for level two trigger processsing.

(2) Implementation in hardware and software for "general purpose" processors that might be used in level three triggers and for data analysis and selection.This might begin with work done on Fastbus Vax'es, the FNAL ACP and the D0 trigger.

(3) The applicability of Fastbus. While Fastbus may be fast enough to handle data at the higher levels of the system, it may not be the best choice for low level subsystems. Also by the time SSC detectors are constructed it may not be the best choice.

(4) How much of the data reduction and acquisition can be done by local electronics in or on the detector elements? Understanding radiation damage will be important for this question.

(5) The possibility of substantial data transmission serially, presumably on optical fibers. Cable plants represent a very noticeable cost and complexity in present detectors.

(6) Examination of the requirements of the media for permanent data storage. While optical disks will likely be able to meet the basic online recording needs, industry must be informed that HEP interests are not identical to the rest of the market.

Recommendations for Data Acquisition

(1) A carefully organized workshop is the next useful step in understanding problems for the SSC. A combined study of electronics, data acquisition and triggering would be the proper format as already noted in the triggering section. Such a workshop should make recommendations for future funding of hardware development in the area of data acquisition.

Computing

The demand for computing resources at all levels for the SSC is likely to be an order of magnitude or more beyond the requirements of the present or near future experimental program. There are many aspects to the

computing problem, some of which have already been discussed in the Triggering and Data Acquistition sections. In this part of the report we will concentrate on the following topics:

(1) offline computing requirements
(2) software improvements
(3) networking

Offline computing requirements for both routine data reduction and physics analysis are expected to very large at the SSC. At the present time it appears that the data reduction part of offline computing will depend heavily on the existence of relatively cheap multiprocessor sytems. A number of projects are currently well underway to produce multiprocessor systems for the next generation of colliding beam detectors. We concur with the recent suggestion of the HEPAP Subpanel on Computer Needs for the Next Decade, chaired by Joe Ballam, that such projects should receive substantially increased support. The current projects are excellent test-beds for SSC-size systems which must be 10-30 times larger. For this reason we have concluded that it is premature to recommend new R&D for producing large multiprocessor systems for the the SSC. Experts within the field are confident that multiprocessor systems and related software with equivalent power in the kilo-VAX780 range can be assembled based on extrapolations of current efforts. We do however recommend that the situation for multiprocessor systems be reevaluated about one year from now when there will be more operational experience.

Software for a major detector at the SSC is likely to require 300-500 man-years of effort. One aspect of software development that should receive support in the next few years concerns vectorization. It is very likely that vector processors will become increasingly common and cheaper in the ensuing years. Some specific problems where vectorization might substantially decrease execution time include:

(1) shower Monte Carlo codes such as EGS or hadron shower codes
(2) pattern recognition and tracking fitting code.

Experience at Los Alamos, Livermore and elsewhere has shown that substantial time is required to understand how to optimally vectorize code.

An early start would allow more informed choices to be made regarding the type of computing for the SSC.

The experimental program at the SSC will probably involve more than 1000 physicists from throughout the world. A key aspect to a coherent effort will be the access to sophisticated networking communications. Networking will be required in all aspects of the experimental program, from preparation of proposals, to detector design and construction and, finally, to physics analysis. The expansion of networking capabilities throughout the next ten years should be vigorously supported.

Recommendations for Computing

(1) Current projects to develop cost effective multiprocessor systems in the 100 VAX780 equivalent range will serve as prototype systems for the SSC. These ongoing efforts should receive enhanced and continuous support at a total incremental level of about 1M$ per year beyond current funds. Industry should be encouraged to participate in these developments. Approximately one year from now a committe should reevaluate the capabilities and costs of multiprocessor systems for the SSC and make recommendations for future activities.

(2) Support the vectorization of one or more useful codes Required support: for each effort computer time on a vector machine, 1FTE and possibly 50K$ for consulting help and salary support.

(3) Vigorously and continuously support networking of the worldwide HEP community. An early start in this effort is essential particularly in the area of standardization. The level of support required must be evaluated yearly.

New Techniques and Devices

Current techniques in vertex detection, central tracking, lepton identification and calorimetry have significant deficiencies in the context of the anticipated SSC experimental challlenge. The search for new ideas and the development of emergent promising concepts and techniques should

therefore be vigorously encouraged. However, proposals for substantial R&D effort should not represent partial solutions, and all relevant SSC concerns such as time resolution, spatial resolution, double track resolution, readout requirements, radiation hardness, cost, stability, and calibration should be addressed. Specialized workshops may be very helpful in clarifying the most promising new directions.

There are are a number of areas which merit continued support at present or slightly increased levels. The study of new sampling media for calorimetry is one such area. The study of silicon, BaF_2, very dense glasses and non-cryogenic liquids should continue but, in terms of SSC development, all relevant concerns should be addressed when possible as noted above.

There are at least two areas of current interest which appear to merit increased effort. One topic is scintillating glass fibers, which in principle could offer superb spatial resolution and double track resolution. The development program can draw on the substantial technical strength of the burgeoning fibre optics industry,. A program of effort can be defined which addresses the current problems of luminous efficiency, optical attentuation, physical arrangement in a practical detector, and readout.

The other major area of opportunity is in the development of new semiconductor devices. The integration of detectors and appropriate readout circuitry is just beginning. Spatial resolutions of 5 microns have been obtained with commercial optical CCD arrays. There is, however, a large gap between the level of current achievements and the perceived opportunity for custom devices optimized for physics. Again a vast, mature industrial base exists, but access to these resources has not been especially easy or productive due to competitive commercial pressures. To increase the chances of new devices optimized for physics in a time scale suitable for the SSC, it appears necessary to establish within the HEP community the specialized technological base necessary for the conceptual development, design and evaluation of new semicondustor devices. This will require a substantial commitment of effort and resources. We note that the time scale for new detector device development is likely to be significantly longer than for custom chip development; more basic research must be done.

Recommendations for New Techniques and Devices

(1) Continue the support of research efforts into new sampling media for calorimetry. Such efforts should, at a suitable time, be prepared to address all relevant problems of the SSC environment.

(2) Increase the support for research into scintillating glass fibers. Required support: 250K$ per year and 2FTE.

(3) Greatly expand the research into development of new semiconductor devices directed towards HEP. Required support: 1-1.2M$ over a few years, including technical support staff and 5FTE.

Muon Identification

The problems of identifying muons at high energies(hundreds of GeV to the TeV range) were discussed at Snowmass ' 84 and in a workshop at the University of Wisconsin on April 4-6. The major problems involve tracking and identifying muons above the 300 GeV critical energy in iron and the effects of apparent hadron punch through. For muons above the critical energy, the creation of electromagnetic showers is enhanced which could impair muon tracking. This can be studied by a combination of calculation and ultimately beam tests. One such beam test at FNAL has recently been proposed by members of the '84 Snowmass muon group.

Also of interest are continued studies of apparent hadron punch through for 100-1000 GeV hadron showers. Recent studies have shown that the apparent punch through is enhanced by muons created in high energy showers.

Recommendations for Muon Detection

(1) Continue the study of high energy muon detection by having one more workshop before or during the Snowmass '86 meeting. Required support: funds for the workshop and some travel funds.

(2) Well conceived beam tests of high energy muon identification should be supported sometime within the next two years. If possible studies of hadron punch through should also be done. Required support: a total of 250K$ not including funds to operate a test beam or beams and about 2FTE.

Appendix A - Task Force Members and Consultants

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Dr. Dave Nygren, University of California, Berkeley

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Dr. Tom Nash, Fermilab

Dr. Dave Anderson, Fermilab

Dr. Brad Cox, Fermilab

Dr. Abe Seiden, University of California, Santa Cruz

Dr. Dave Cutts, Brown University

Dr. Randi Ruchti, Notre Dame University

Dr. Paul Kunz, SLAC

Dr. Stu Loken, LBL

Dr. Lee Pondrom, University of Wisconsin

Dr. Sherwood Parker, University of Hawaii

Dr. Alan Bross, LBL

Dr. Stan. Majewski, Fermilab

Meetings were held on May 9 at Fermilab and May 19 at SLAC.