Report of the FNAL Accelerator Advisory Committee 2018

Fermilab, Batavia IL, December 4-6, 2018

The Accelerator Advisory Committee (AAC) meeting took place December 4-6, 2018 at the Fermi National Accelerator Laboratory. The agenda of the meeting is shown in Appendix 1; the list of the Committee members participating in this meeting is given in Appendix 2; and the charge to this committee meeting is given in Appendix 3.

General Remarks

As usual, thanks for the hospitality and arrangements. The Committee also thanks the Fermilab team for directly addressing all the charges with generally well prepared presentations, and allocating sufficient time for discussions in the agenda. Please keep this up.

The committee congratulates for the very successful FY2018. The Proton Improvement Plan (PIP) is basically completed. Proton beam ran consistently on 650 kW on target with a reliability of >85%. Also, Muon g-2 collected two times the Brookhaven data set in 2018. Fermilab will face the challenge to get to 20 times the BNL data set.

Operations faces now more operational facilities compared to the Tevatron era, is running with one operator less per shift and did shrink to 4 crews. Adequate staffing for the operation of the accelerator complex requires hiring to bring the operations group back to the strength of 5 crews.

Fermilab created the Office of Accelerator Science (OAS) under the CRO and with Sergei Nagaitsev as Head of the Accelerator Science Programs. The OAS will coordinate accelerator science research and development in the future with FAST/IOATA as the main research instrument. This step is acknowledged as there is the need to develop strategies and plans for accelerator development and research activities that will support the project commitments and future operation with maximum physics output.

The committee congratulates Fermilab for the completed construction of the IOTA ring and for launching the IOTA and FAST accelerator commissioning and for first experiments. The committee is looking forward to the science experiments that will follow in the future.

The committee congratulates Fermilab for the continued success of the SRF R&D program and for driving the roadmap on High Q and High Field Gradients.

The committee is impressed by the progress in partnering with universities to involve students and faculties in leading edge programs such as SRF, beam physics studies and targetry research. This effort is commended, and AD and APS-TD are encouraged to increase the training and education of Highly Qualified Personnel (HQP) and to further strengthen the partner programs with universities. Please continue with students' presentation.

The Illinois Accelerator Research Center (IARC) is a promising pilot program that is being incubated in the Directorate. The committee expects to see IARC grow into a viable and self-sustaining element of the laboratory's accelerator program.

Suggested future AAC charges:

- Beam physics improvement program at the booster and beam studies in general.
- Update on Fermilab's new Magnet Development Program in particular long-term targets and funding.
- The resource allocation for the operational support that is required to enable success of the FAST/IOTA research program.

Brief answers to the Charge Questions

1. Have all the recommendations by AAC 2017 been adequately addressed?

   Generally yes.

2. a. Assess the readiness for the test of the 15 T, 1 meter dipole

   The 15 T magnet construction is nearly finished with about one-year delay, and testing is foreseen early 2019 in two steps representing two pre-stress cases. There is no indication of obvious flaws at this point and when magnet assembly, in progress these days, is completed, the test phase can start without hesitation.

   b. Assess the Magnet Task Force reformulation of the Fermilab HFM R&D program

   The draft task force report was not shared with the committee thus a full assessment is premature. Based on the global abstract presented the program looks generally fine in terms of tasks addressed, though over optimistic schedule wise and today only to some roughly 30% level funded.

   c. Evaluate integration of the Fermilab program into US Magnet Development Program

   The R&D program is being integrated in the US Magnet Development Program as much as possible but specific tasks are kept for FNAL complementary program which is considered fine. The committee welcomes a substantial parallel program including a more extensive new conductor development program as well as design and construction of more generic short magnet demonstrators in the years to come.

3. a. Fermilab has finished the construction of the IOTA ring and began the IOTA and FAST accelerator commissioning, followed by accelerator science experiments. Assess the IOTA

and FAST operations and the near-term research program, and comment on the plans and issues. Are the operations plans adequate to enable success of the FAST/IOTA research program?

Yes, and the committee would like to congratulate the successful commissioning of IOTA with electrons. The early research programs performed so far are very interesting and the latest experiment with single electron is quite impressive.

b. The next major step in the development of the FAST facility is the installation and commissioning of the IOTA proton injector. Assess and comment on the plan and schedule of the IOTA proton injector construction, installation and commissioning.

As long as the required resources are available, the presented plan is adequate to enable successful implementation of proton beam for IOTA.

4. Assess the Fermilab SRF R&D program. Is the progress on the SRF R&D program sound? Is this research program adequately setup to align with and to enable future national and international projects?

   The SRF R&D program is sound, world-leading, and has made steady progress with regular important breakthroughs. The research program could benefit by having a multi-year SRF technology development plan that aligns with the HEP roadmaps and includes Fermilab priorities and Quantum Information Science (QIS).

5. a. Suite of Accelerator Improvement Projects

   The work scope presented at AAC 2017 to deliver higher beam power to NOvA and prepare parts of the accelerator complex outside the PIP-II scope has been delayed, making prioritization and scheduling even more critical. Would the priorities of the revised work scope produce the maximum possible number of POT, given the constraints presented?

   The team has presented a sound plan with a revised and prioritized scope to increase beam power to 900 kW and to prepare the target systems for PIP-II. This will allow a maximum number of protons on target.

   b. Proposed Booster Space Charge Summer Studies

   Assess the proposed Fermilab-CERN Booster study program (2019). The proposed program will incur loss of beam up-time to the experimental program. Does the proposed program have a high impact potential for accelerator physics and future machines? Does the AAC support/endorse proposing the study program to the Fermilab Physics Advisory Committee?

   The proposed program does have a high potential impact. In fact, the committee was quite surprised that regular machine studies are not done. The AAC does endorse and support a proposal of the study program to the Fermilab Physics Advisory Committee.

6. a. NuMI Megawatt Upgrade

Assess and comment on the NuMI target upgrade plans. Is the suite of upgrades to the NuMI target station well-conceived to confidently be able to accommodate a proton beam power of up to one megawatt?

The planned upgrades to the NuMI target station is well suited to accommodate the 1 MW proton beam power.

b. High-Power Targetry R&D

Assess and comment on high-power target R&D plans and the roadmap. Has the recent campaign of irradiation and pulsed-beam experiments provided fruitful results for HEP? Are the "next steps" of the HPT program well-positioned to make an impact? Is the long-term plan (a.k.a. roadmap) of HPT research appropriate to enable future accelerator facilities?

The campaign of irradiation and pulsed-beam experiments provided fruitful information about the degradation of mechanical and thermal properties of target system's materials. The gain of data from irradiated samples or components will have an impact on the target reliability with an ultimate reduction of potential failures. The presented HPT R&D program is well suited to increase the performance of the target systems.

7. Assess and comment on the Fermilab Accelerator Control System (ACNET) Upgrade under consideration.

   A global strategy for the full complex is needed: hardware consolidation; controls system upgrade; PIP-II. The time-line for developing this strategy is demanding, but it is important that appropriate choices are made at this stage. Resources, both material and personnel, are limited.

   The controls infrastructure choices are critical and have far reaching consequences. Due time should be given to carefully evaluating the options and making sure that the chosen solution(s) fully meet the present and future needs of the complex. Requirements must be clearly established.

8. Assess and comment on the Illinois Accelerator Research Center (IARC) plans for R&D to commercialize technology developed at Fermilab.

   IARC is a good first step for developing a work-for-others (WFO) program for other federal agencies and a program for commercial activities. Over time, IARC should have no problem growing to and maintaining 5% of the total FNAL effort with externally funded activities. IARC will likely find growing the WFO portfolio easier than the commercial portfolio as the easiest niche for a national laboratory is to supply continued R&D. A single product focus is vulnerable to competition and IARC will find growth easier if it identifies a broader portfolio of accelerator technology solutions that is based on unique competitive strengths and that ranges from basic research to mature product development.

Charge 1: Recommendations AAC-2017 adequately addressed?

A document with the response to the recommendations has been compiled and was reviewed by the committee. All recommendations have been adequately addressed in writing, by the document available and by the presentations provided at the AAC2018 meeting.

Charge 2: High Field Magnet Program

Findings

The HL-LHC AUP project is a construction project only, and will cease in due time. Today there is no substantial successor magnet construction project planned.

The single running high-field magnet R&D project is the 15 T dipole, a 1 meter low-cost demonstrator magnet of which construction started in 2016. After about one-year accumulated delay, it is being assembled, where after a test will follow in 2 steps in 2019, first a low pre-stress run to see its limitations without risking damage, followed by a higher (nominal) pre-stress run to see eventually its ultimate performance.

FNAL executes their part of the US Magnet Development Program, in place since 2017, a coordinated effort of LBNL (leading), FNAL and ASC/HFML, closely monitored by DOE. Within this MDP, FNAL's primary focus is on minimizing margin, eliminating training and cost reduction studies.

However, there is a clear wish to do more, to run an in-house program complementary to the US MDP, a substantial parallel program with at least one major magnet construction program in accordance with its statue of being the main accelerator laboratory in the US.

To help developing a new high-field magnet program, an FNAL-internal multidisciplinary task force, also including non-magnet experts and many young scientists, was established in autumn 2017. This task force has presented a draft report now being edited for publication shortly (report not presented to the AAC).

An ambitious roadmap covering 2019-2025 was presented. This includes various magnet component optimization studies; advanced Nb_3Sn - high heat capacity conductor development for increasing conductor stability; instrumentation development; fast turn-around models; cost reduction; coil windings reinforcement studies; and magnet design studies for ultimately realizing an 18-T class Nb_3Sn dipole magnet as well as and a 5 T ReBCO based insert by 2024 and 2025, respectively.

The long-term goal is realizing a demonstrator for an accelerator quality Nb_3Sn/*Re*BCO hybrid magnet with relevance for FCC. A full 20+T HTS magnet is not considered realistic by the team.

Budget for the roadmap program, however, is not yet available and an estimated ≈3-4M$/y was said to be needed from 2020 onwards.

FNAL road map tasks are being integrated in the MDP program for maximizing overlap and synergy. Year 2019 tasks concern: finishing the 15 T dipole project; continue APC/Cp+ conductor R&D and trial production with Bruker OST; study Quench Current-boosting Device, mature plastic model 16+ T stress management design; study thermoplastic coil impregnation through testing various cable stacks; continue with ultrasonic quench diagnostic sensor, develop small coil test configurations; and start with a pre-conceptual design of 16+ and 20 T model magnets.

A small magnet model was made of a *Re*BCO tape-based quadrupole magnet using pancake solenoids to magnetize the quadrupole field making yoke. This design may eventually be used for 0.4-T class quads in linacs, but is of no significance for high-field accelerator magnets.

A plan was presented to realize at FNAL a new wide bore 14 to 15 T high-field conductor and magnet test facility called "HFVMTF" like FRESCAII at CERN, though its full funding still has to be found.

Comments

The 15 T magnet construction project is near to completion. This magnet has a few very interesting features like use of wide and stiff cables and a new coil support structure. Since magnet training is essentially determined by the mechanics of the cable stacks, pre-stress tuning and coil interface conditions to the support structure, and these three issues are very different than in other magnet models, the test may show pleasant surprises. It is therefore wise to perform the test in two steps as proposed and supported by the outcome of the test readiness review. As much as possible information shall be extracted from the test to evaluate this design.

The 15 T dipole magnet is the last magnet in the pipeline and no other full-size short demonstrator magnets are yet under construction nor planned and financed. This dead valley may have impact on the team's motivation and maintenance of the magnet construction facilities. This situation is further amplified by the completion of the HL-LHC quadrupole project AUP in the coming years for which at present no successor project is defined. It is considered important to anticipate this situation and define proper 5 and 10 year goals for initiating new model magnet construction projects in order to preserve hands-on skills, competence and tooling, as well as long-term prospects for personnel to participate to major magnet projects, and last but not least to keep FNAL at the frontier of high field magnet research.

The Task Force report was not disclosed to the committee; instead, some general conclusions were presented. The issues presented do not show surprises: more diversity; strengthen fundamental understanding; shift to more science - less technology; use fast turn-around systems;

magnet cost reduction; advanced and more instrumentation, keeping demonstrator magnet. They are in line with many earlier reviews; and in accordance with what is happening in other laboratories as well. The relative weight of the issues in terms of budget allocation was not addressed leaving all space for the management to prioritize.

A very ambitious roadmap for the magnet program of the next 6 years was developed showing important and less important tasks. Irrespective of the availability of budget, the roadmap is too optimistic as it includes small-scale studies and qualification test followed by two major magnet constructions, 18 T dipole with a bore and a 5 T HTS insert for a hybrid 20T magnet, to be completed within 6 years without having significant length of mature new conductors in hand today.

The roadmap is financially not yet covered in full (at some 30% today) and most likely needs to be scrutinized for this reason. Given the status of high field Nb_3Sn magnet developments in other labs, the committee is of the opinion that new technology directly targeting a "new" magnet design based on drastically enhanced thermal-electromagnetic stability in coil windings is first priority. This concerns in particular the development and demonstration in cables and small coils before starting to embark in a new costly full-size magnet demonstrator, of a new enhanced engineering critical current density and high-Cp material stabilized conductor. When the mechanical disturbance spectra in Nb_3Sn magnets cannot be substantially improved, the only route to stable reduced training high field Nb_3Sn magnets is to drastically improve conductor cooling (presently incompatible with transverse pressure load) or enormously increase the wire's enthalpy, in order to increase adiabatic single strand stability as well as collective strand stability in cables. This has to happen eventually, if still needed, in combination with imposing a higher temperature margin by reducing the working point on the load line. The new high Cp conductor program is exactly in line with this.

The cost reduction task apparently concerns solutions for cos-theta type windings implying difficult shapes of material and expensive tooling, and which are inherently more difficult to make than for example common coils using easy to wind and handle racetrack coils and easy to manufacture and assemble flat plate and block structures. Ideally the magnets should be made using robots to a high degree, but the design has to be adapted to this requirement. To reduce production cost of magnets the total cost of conductor, coil structures and automatic coil winding techniques and production failure risks have to be considered. It is not obvious at all that cos-theta type magnets will then be the best solution for making eventually 5000 units of 15m long magnets for an FCC or similar machine. It is recommended not to restrict the study to cos-theta.

Given the uncertainty in the outcome of the European strategy discussion regarding FCC-hh timing, the uncertainly in superconductor technology and magnetic field level in SppC as well, and absence of relevant high-field dipole magnet requests in the US, it is wise to target a more generic high field dipole magnet design and primarily aim at achieving a stable and no-training magnet using a comfortable margin instead of presently unrealistic minimum conductor designs. Value engineering and making the magnet 100% accelerator compliant is of secondary importance. The demonstration of a generic robust 16-T class dipole magnet with a relevant bore and that works, meaning at first cool down in 0 to 3 training steps to nominal, is of utmost importance, and would demonstrate a breakthrough technology and bringing FNAL back to the frontline.

Recommendations

- Given the limited resources, new advanced Nb_3Sn conductor development deserves the highest priority following the two promising routes already started, (1) increasing engineering current density by using the internal oxidation method for achieving enhancing pinning; (2) include in the matrix high Cp materials (like Gd_2O_3). Try much higher than the 1% today, investigate i.e. 5 and 10% as well.

- Include in the new Nb_3Sn conductor development program also cable development using the new strands, and prove enhanced cable stability by performing relevant Minimum Quench Energy (MQE) versus normalized critical current (I/Ic) studies at 15T level to demonstrate superior behavior (or not); followed by a small demonstrator coil.

- Develop further and set priorities in the new in-house magnet development program, still in coherence with the US Magnet Development Program, but supporting a main research line towards the next one or two magnet demonstrators, featuring new technology and not more of the same.

- Ensure that financial resources are sought to sustain the in-house Magnet Development Program, in order to preserve the lab's skills and expertise for participating in frontline magnet projects.

Charge 3: FAST & IOTA Progress & Research Program

Findings

The FAST and IOTA program has been progressing well according to the plan. IOTA commissioning with electron beam from the FAST e-linac went quite well, and subsequently developed an impressive capability of storing a single relativistic electron. This unique capability opens the door for research programs in accelerator science as well as quantum science.

The research program of FAST and IOTA in general can be categorized into two types. The IOTA based is at the moment mainly driven by Fermilab except the OSC, while the combination with the FAST e-linac draws more external users who are aiming for R&Ds that are tightly connected with projects beyond Fermilab, such as high repetition gamma-ray facility, EIC, etc.

The implementation of proton injector for IOTA is planned to be realized by 2020.

Comments

While the non-linear and space charge driven beam dynamics research program at IOTA is very impressive and will be highly interesting for the community, the research program using the FAST e-linac can draw a lot of interests from more broader background which can play an important role in establishing a vital user facility. The committee sees the value of establishing a FAST Science Advisory Committee to ensure the science program is vital and high quality.

Even though there are no technical challenges in implementing the proton injector for IOTA, the execution according to the plan still bears risk if the required resources are not available.

The mid-term proposed R&D programs overall are interesting and strategic, considering the high scientific impact vs. ongoing completion of the IOTA full scope in the frame work of other high priority tasks on campus.

The electron lens at IOTA R&D program is very interesting. It is quite impressive to see the rich potential academic knowledge one can gain from this program. Nevertheless, the committee would like to express its concerns over the technical details of implementing the electron beam so that the McMillan field can be achieved. Secondly, compensating the space charge dominated tune footprint using an electron lens at IOTA on one hand is promising, on the other hand, it is not clear to the committee how the team plans to measure the space charge induced incoherent tune spread. Perhaps a quadrupole pickup coupled with a spectrum analyzer to measure the quadrupole betatron Schottky spectrum would work?

Recommendations

- Quantify the error tolerance in implementing McMillan lens with the electron beam.
- Develop the required beam diagnostic instruments and techniques for measuring the impact of electron lens on the space change effect.

Charge 4: SRF R&D Progress and Research Program

Findings

The GARD RF roadmap was presented and is used to guide work in high gradient and high Q for future machines. A ‘magic’ bake at 50-75ºC was shown to produce record gradients at excellent Q values.

You have added dilution refrigerators to your test facilities to enable exciting work in QIS.

Base funding is ~$6M/yr for operating the SRF facilities and ~$2M/yr for R&D.

The SRF team is now incorporating world-class material scientists through the CAPST collaboration. Excellent results with Nb_3Sn coatings on single-cell cavities were shown, and work is underway to coat full cavities.

Three technology advances were described that have pushed cavity Q’s to record performances. A new furnace is on order to enable advanced coating research. Progress with the various types of cavities for PIP-II was described.

A 10-year plan for upgrading cryogenic systems across the FNAL campus was presented as well.

Comments

- The SRF groups should be commended for an excellent research program that covers a wide range of activities, including cavities and cryomodules, QIS and cryogenics.
- The recent improvements in cavity preparation techniques have enabled incredible gains in cavity Q and gradient reach. These improvements have had important impacts on projects around the country – LCLS-II, LCLS-II-HE and PIP-II.
- The progress for the amount of funding is outstanding.
- The SRF research plan has seemed somewhat Edisonian rather than relying on a systematic approach based on sound theoretical principles (e.g., the Advisory Committee did not see a technical development roadmap with specific activity steps). The SRF team has an opportunity to move to a more systematic approach with the increased involvement of materials scientists.
- The problem with shipping cryomodules to SLAC has been a difficult issue. This implies a possible weakness with skills in the overall cryomodule design, and inadequate quality control. Consider how to strengthen the expertise in CM mechanical design.
- The groups have demonstrated the importance of material science and surface characterization as an important part of understanding the basic physics of high gradient and high Q cavities. Your collaboration with Northwestern through the CAPST partnership is an excellent example of how a national lab can collaborate with a university to advance the basic understanding of SRF.

- You have a great staff of SRF scientists and engineers, both at junior and senior levels. You should be commended for promoting junior and mid-level researchers to leadership positions.
- QIS activities are new to FNAL and new to the world in general. The application of SRF expertise to QIS is an exciting new development. In addition, QIS R&D will be a great path to draw new students into SRF activities, and hopefully into accelerator jobs in the future.
- You outlined a long-term plan to improve and expand your cryogenic facilities and capacity. This will be important to allow for the increasing demands on your SRF test and production facilities in the future. The committee fully supports this long-term plan.
- The addition of dilution refrigerators to your cryogenic infrastructure has been an excellent investment and will enable additional R&D in QIS and perhaps other areas of SRF research.
- FNAL is clearly leading the community with advances in Nb_3Sn coatings for SRF cavities. This work will be important for future cost reduction and development of 'portable' industrial systems. Please continue with the excellent work in this area.
- The work on high Q and high gradients will be important for ILC cost reduction. At the next AAC, hopefully you can tell us about your plans for ILC contributions.
- Other countries involvement with PIP-II is commended; it is important that India was able to supply a good cavity.

Recommendations

- This problem with shipping cryomodules to SLAC has been a difficult issue. Consider how to strengthen your expertise in CM mechanical design and revisit your quality programs.

- Fermilab's SRF groups have made impressive contributions to projects around the country and the goals generally aligned with future machines. Develop a multi-year FNAL SRF technology development plan that aligns with the HEP roadmaps and includes FNAL priorities and QIS initiatives. This will help to clarify your priorities internally and externally.

Charge 5: Accelerator Facility Upgrades

Findings

The submitted proposal for NuMI Target System AIP and Intensity AIP has not been funded in FY2018. Due to changes in the rules, even those AIPs with activities over multiple years will have full funding supplied in the first year. Therefore, a new timeline for the upgrade projects has been developed with a clear plan to increase the beam power to 900 kW until FY2020. The plan comprises a reduced scope of the NuMI Target System AIP. The Recycler RF cavities have been added to the list of projects of the accelerator facility upgrades, following the needs of PIP-II.

The NuMI Target System AIP is funded for FY2019. According to the suggestion of the DOE, a "campaign" approach like PIP instead of AIPs is planned for the following topics, which are listed according to priorities:

1. Booster Intensity (needed for 900 kW)
2. Booster Magnets (helps to reduce beam losses at 900 kW)
3. Main Injector gamma-t jump (helps to reduce beam losses at 900 kW)
4. Booster RF cavities (helps with operational reliability at 900 kW and is needed for 20 Hz operation of PIP-II)
5. Recycler RF (needed for 20 Hz for PIP-II).

The challenges of the high beam power upgrade of the accelerator complex towards PIP-II in the future will be the intensity limits of Booster, Recycler and MI in terms of beam losses (~1 W/m).

It was stated that the booster upgrades needed for 900 kW were well understood and only the necessary funding was needed. In addition to the 900 kW upgrade there are plans to go to even higher currents, perhaps not in the existing booster but in a new rapid cycling synchrotron.

A set of generic experiments in the booster have been planned to better illumine the behavior of beams with large space charge and collective forces. The proposal is to run for the last few weeks before this summer shutdown. There is a wide range of interesting suggestions, probably too many to be done well in the time requested.

Comments

The team has presented a sound plan with a revised and prioritized scope to increase beam power to 900 kW and to prepare the target systems for PIP-II. If funding will become available, the "PIP-I+" activities will allow for the maximum possible number of protons on target and will continue the successful upgrade of beam intensity started with PIP-I.

Work forces for the projects are limited. However, the money from DOE will pay for personnel, which could be used to support critical succession by recruiting young people for the upgrade project tasks. The main risk on availability of work forces is PIP-II as it is not yet baselined.

In the Booster, the total permitted beam loss is ~500 W. The upgrade to 900 kW is going to push this envelope and the team should do their best to guarantee the upgrade will work. The dynamics associated with RF capture have been given a preliminary look, but beam loading and impedance effects did not appear to be estimated. The losses at injection still seem to be a mystery. Likewise, transition was modeled, but the simulations did not appear to include impedances. The laminations in the booster vacuum chamber are most likely the dominant impedance in the ring. This is bad because the impedance is rather large, but good because you can make a good estimate of the impedance.

The accelerator studies presented for the booster are a good idea. In fact, it would be a good idea to have regular beam studies designed to optimize performance. As far as the proposed studies it would be worthwhile to think of these as actual experiments aiming for quantitative results.

The transverse beam size in a synchrotron is difficult to measure. In ionization profile monitors the trajectories of the detected ions are influenced by the space charge potential of the beam. Left uncorrected high-density beams can be assigned emittances that are off by a factor of 10. A quadrupole pickup does not have this problem, nor do flags in transfer lines. By cross correlation one should be able to verify that space charge corrections are good. A quadrupole pickup has the additional benefit that space charge tune shifts are directly observable in the shift of the coherent quadrupole frequency.

Recommendations

- Consider prioritizing the list of Booster studies that has been presented, starting with the most impactful on future operations getting highest weight. Consider to distribute the studies over several years.
- For the 900 kW booster related upgrade calculate the impact of the machine impedance on injection and transition.
- For the proposed Booster experiments consider installing a quadrupole pickup and look into correcting IPM profiles for the beam space charge.
- Seriously consider adding regular beam studies to the operation schedule.

Charge 6: NuMI 1-MW Target System Design

Findings

The NuMi 1-MW target must accept $6.5 \cdot 10^{13}$ protons per spill, which is an increase of about 33% compared to present target requirement. To achieve the 1 MW goal, the cycle time for the proton pulse must also be reduced from 1.33 to 1.2 seconds. The path toward this goal require investigations of temperatures and mechanical stresses produced by beam heating. The target handling need to take the higher radioactivity into account that is created in the targets. Finally, the aging infrastructure need to be refurbished to cope with the 1MW operation in the future.

The NuMI 1-MW Target System AIP is conducted in phases. The feasibility and the design have been completed. The Target Systems Department (TSD) started the implementation and prepares for the target facility and beamline component upgrades. The completion of the NuMI 1-MW Target System AIP is planned for 2021.

The system improvements are driven by operational experience in the past and an analysis has been conducted with MARS to explore the required improvements of components. Extensive analysis and redesign work has been performed on the crucial elements, like the pre-target window, the graphite target core and the target baffle. The magnetic horn conductors have been analyzed for 1 MW operation. The new design relies on cooling improvements by an increase of the cooling spray nozzle pressure that will avoid hot spots on the inner conductor.

The target modules are corroded over the years and the motion control on target and horn is failing. The drives and links will require a rebuild with corrosion resistant materials that are successfully tested. In addition, and upgrade of the chase cooling capabilities has been already started and will be beneficial for future operations. The shielding upgrade as well as the enhanced Tritium handling has been addressed and the environmental assessment is complete.

The high-power target (HPT) R&D is essential to overcome the limitations imposed by the target on the neutrino production in the future. A broad program has been launched at Fermilab to achieve a systematic understanding of the material behavior under intense irradiation conditions. The program has been broken down into the main topics, material behavior and targetry technologies. The material behavior research addresses mainly radiation damage and thermal “shock” response. Targetry technologies comprise remote handling, heat removal, radiation protection and radiation induced corrosion issues for instance.

To explore the radiation damage and thermal shock/fatigue regimes, alternative testing methods to reduce costs and risks (like Methods of Irradiated Material Characterization - MIMiC) are explored. Thermal shock testing using electron beams, lasers, or other techniques will not only reduce the cost but also the length of R&D cycles compared to proton beam-line tests. Electron beam induced thermal fatigue could allow the unique combination of proton beam-like thermal shock cycling at relatively high frequency while simultaneously creating radiation damage. X-ray diffraction is used to investigate irradiation damages, for instance swelling and amorphization of the graphite target core.

Comments

The committee commends the team for the careful planning and preparation of the 1 MW upgrade. A project manager for this important upgrade project has been appointed, who is driving the project together with the TSD. This is the right step to guarantee the success of this essential AIP project with completion foreseen in 2021. The resources loaded schedule for the NuMI 1 MW upgrade is sound as well as the budget planning.

The NuMI AIP 1 MW upgrade will accelerate the physics goals of NOvA. It's well based on the developments that led to the success of the 700kW operation and is a result of a well planned and executed development plan of the TSD. The planned upgrades to the NuMI target station is well suited to accommodate the 1 MW proton beam power in the future.

The committee commends that Fermilab has strengthened its leading role in high power target R&D and according collaborative activities that includes the coordination of the RaDIATE collaboration program. TSD establishes the material science of targets as a core competency at Fermilab and is doing the right move in proposing the Targetry Materials Science and Technology Lab Initiative. This will provide the opportunity to develop new Targetry Technologies such as advanced robotic remote handling.

An essential tool of target material investigations is the post irradiation analysis of target samples. According hot cell infrastructure within the proposed Targetry Materials Science and Technology Lab that combines analysis and handling of irradiated target samples is crucial.

The HPT R&D program is partnering with universities to involve students in targetry research projects. The committee acknowledges that the TSD followed the recommendation and established a strong graduate student thesis program together with universities national and international.

The campaign of irradiation and pulsed-beam experiments provided insight in degradation of mechanical and thermal properties of the graphite target core material. It also provided a better understanding of the mechanical properties of the Be window after irradiation. The gain of data from irradiated samples or components will have an impact on the target reliability with an ultimate reduction of the risk of failure. The presented HPT R&D program is well suited to secure the top performance of the target systems midterm and the future increase of the beam power the targets will withstand, which is a prerequisite for a leading neutrino physics program.

Recommendations

- None

Charge 7: Modernization Program

Findings

The functionality and high reliability of the existing control system has served the complex well over the past years. An important new client, PIP-II, is incoming.

The 3-tier system has:

- Aging and heterogeneous front-end hardware – different frameworks, network protocols;
- Middle level ACNET 1980's era network services, central database and a collection of generic functionalities;
- Top level is a mix of high-level software (C/C++/X windows, Java, Web, ACL, Python/Matlab APIs) some of which is using obsolete frameworks.

Recent targeted modernization has included: Erlang based FE framework, VME based ramp generators, Raspberry Pi based crate controller, database migration from Sybase to PostgreSQL. These are rather specific, targeted initiatives.

Major issues include: lots of old hardware; lots of old software; an aging and declining in strength work force (no software development related hires since 2001 for instance).

PIP-II represents a major new development with the need for, inter alia, new timing, machine protection. A preference for EPICS was expressed by the PIP-II management team, and there is a clear interest in exploring the possibility of basing the entire complex's future control system on EPICS.

Strategy options presented: Evolve existing; Work with two solutions; All EPICS; Re-think. Either way a large effort and additional resources will be required. EPICS likely to be modest effect on PIP-II cost to completion but a large effect on modernization cost for rest of complex.

A modernization workshop has been held. A task force set-up, charged, and at work. There is a nascent wish list. The identified highest priorities list is a mixed bag and includes urgent hardware upgrades.

Comments

As noted above, the functionality and high reliability of the existing control system has served the complex well over the past years. However, there is lots of old hardware, lots of old software, a mature and limited work force.

A global strategy for the full complex is needed: hardware consolidation, control system upgrade, PIP-II. The time-line for developing this strategy is demanding, but it is critical that appropriate choices are made this stage. Resources, both material and personnel, are limited.

There has been a quiet revolution in accelerator controls over the last 10 years driven by the exploitation of increasingly complex machines and the availability of modern technology. This has impacted: the front-end environment; the communication layer; and the high-level software stack. The required consolidation, rejuvenation and PIP-II initiative provide Fermilab with an opportunity to harness, where appropriate, at least some of these developments.

Strategic options considered are:

1. Evolve existing,
2. Support both ACNET and EPICS,
3. All EPICS,
4. Re-think.

Comments on these:

1. It is definitely time to move on.
2. Supporting both architectures in the long term would place too much of a strain on the limited resources.
3. EPICS, after careful evaluation by the Fermilab team, would appear a reasonable choice.
4. A re-think at some level should be performed, but given the resources and time-scale, an in-house development is ruled out. But it would certainly be instructive to at least canvas other possibilities.

An all-at-once conversion to a new system across the complex is ruled out. Thus, a staged deployment machine-by-machine, gaining experience on the way, would seem a reasonable approach.

In the meantime, a consolidation plan for old hardware (including the choice of FE framework) has to be drawn up. Before starting on the software, a coherent approach has first to be established. Be very careful with the (ad hoc) technology choices – consider support, size of community, available of programmers for hire etc.

Fermilab management foresees that controls be included as part of the Lab Modernization Plan to be submitted to the DOE in early 2019. At present the controls section contains a prioritized list of items with cost estimate. PIP-II estimate to date has assumed evolved ACNET. This submission is a recognition that something needs to be done rather than a strategy. A firm strategy by CD-2 baseline review next June is required.

At SLAC, a 5 year ‘Mission Readiness’ program was carried out to address aging controls and infrastructure. Please contact SLAC if you are interested in the details of how this was planned and carried out.

Recommendations

- The controls infrastructure choices are critical and have far reaching consequences. Due time should be given to carefully evaluating the options and making sure that the chosen solution(s) fully meet the present and future needs of the complex.

- Requirements must be clearly established. To this end, (re)establish operation models and the required functionality for the existing complex and PIP-II.

- Carefully evaluate EPICS functionality given the requirements of operations across the complex and evaluate solutions other than EPICS deployed elsewhere, preferable coupled with visits and discussions with the labs concerned.

- Start to develop a resource loaded schedule for the consolidation/upgrade deployment and establish an obsolete hardware eradication plan.

- At an appropriate point, organize an external review of the upgrade strategy.

Charge 8: IARC – R&D to Commercialize Fermilab Technology

Findings

Fermilab is considering growing non-HEP efforts to up to 5% of the total Fermilab activities. This non-HEP effort can be a combination of WFO for other federal agencies and commercial activities. Fermilab's Office of Partnerships and Technology Transfer (OPTT) is largely responsible for this sector of Fermilab activities. IARC has a different purpose and objective than Fermilab's OPTT organization and is a separate organization. IARC is a special pilot line organization at the discretion of the Lab Director.

Fermilab provides IARC with indirect funding as its base funding. Fermilab wants IARC to grow with outside funds, either with federal Work For Others (WFO) or commercial funds (including CRADAs), at least to the point the tax on the funds coming in covers the indirect that constitutes IARC's base funding.

Ideally, IARC can serve as a buffer for the roll-off of Fermilab employees working on LCLS-II and other funding fluctuations. If successful, IARC may become aligned with another Fermilab Division or spin off as its own division. The goal of IARC is to mature a limited range of accelerator technologies from Technology Readiness Level (TRL) 3 to 6 – as a focused technology push.

For WFO and Tech Transfer, IARC is focusing on a 10-MeV compact SRF accelerator using cryocoolers and a future field-emitter cathode under development by NIU.

IARC has had about sixteen externally funded projects, five of which are ongoing, plus another pending. Eight of these projects are commercial while ten are for federal agencies, including eight for various parts of DOE. Most of these projects appear to support the IARC compact SRF accelerator concept. The IARC compact SRF accelerator concept is especially closely aligned with HEP/Stewardship in the areas of high-average power accelerators for energy and environment applications.

Comments

The IARC team is technically excellent and the compact 10-MeV accelerator concept they developed is actually quite compelling.

Comparable successful WFO/Tech Transfer activities at other DOE laboratories target a broader portfolio of technology areas at all TRL levels (foundational research through prototype development) rather than a single point design.

The natural national laboratory niche is technology development rather than selling products.

Successful WFO/Tech Transfer at other DOE laboratories leverages technologies and/or capabilities developed for that laboratory's core mission (such as SRF for Fermilab) and uses small amounts of targeted discretionary laboratory funding (e.g., LDRD and other indirect opportunities) to keep the activity sustainable.

Having a commercial partner (e.g., Niowave or RadiaBeam) may have advantages (i.e., IARC does continuing research while the commercial partner sells matured products based on that research and builds the need for the continued research).

There are off-the-shelf commercial products that appear to directly compete with the IARC 10-MeV SRF accelerator.

The HEP/Stewardship technology focus areas are not necessarily the right focus areas for WFO or Tech Transfer at Fermilab.

Recommendation

- Conduct a SWOT (Strengths, Weaknesses, Opportunities, and Threats) study for the 10-MeV point design, including both the commercial/federal market and the competition from commercial devices such as the IBA Rhodotron.

- Identify a broader set of accelerator technologies Fermilab developed for its core mission that can address technology gaps for federal sponsor mission areas.

- Evaluate the benefits/threats of partnering with a commercial entity.

Appendix 1: Agenda

Tuesday, December 4, 2018

08:30 - 09:15 Executive Session

09:15 - 09:30 Welcome and Introduction to AAC Meeting

7.5 minute presentation

Convener: Dr. Sergei Nagaitsev (FNAL)

Material: slides

09:30 - 10:10 Accelerator Division (AD) Introduction and Status

Including Muon g-2, PIP-700, ASTC Building
20 minute presentation

Convener: Michael Lindgren (Fermilab)

Material: slides

10:10 - 10:25 Break

15 minutes

10:25 - 11:05 Accelerator Plan for 900 KW to NOvA and Needs beyond scope of PIP-II Project - Q5a

20 minute presentation

Convener: Mary Convery (Fermilab)

Material: slides

11:05 - 12:05 Applied Physics and Superconducting Technology Division (APS-TD) Introduction and Status

including LCLS-II and HL-LHC
30 minute presentation

Convener: Dr. Sergey Belomestnykh (Fermilab)

Material: slides

12:05 - 13:05 Lunch for AAC Members, Sergei Nagaitsev, Sergey Belomestnykh, and Mike Lindgren

Small Dining Room - WH1SW
1 hour

13:05 - 13:50 Student Presentations

3(10 minute presentation + 5 minute discussion)*

Daniel Bafia (Illinois Institute of Technology) - SRF Cavities with Modified High Temperature Bake
Ihar Lobach (University of Chicago) - Quantum Effects in Undulator Radiation
Sebastian Szustkowski (Northern Illinois University) - Gas Sheet Beam Profile Monitor for IOTA

Conveners: Daniel Bafia, Ihar Lobach, Ihar Lobach (The University of Chicago), Sebastian Szustkowski (Northern Illinois University)

Material: Daniel Bafia | Ihar_Lobach | Sebastian Szustkowski v2

13:50 - 15:05 Superconducting RF R&D - Charge Question 4

37.5 minute presentation

Convener: Sam Posen (Fermilab)
Material: **slides**

15:05 - 15:20 break
15 minutes

15:20 - 15:50 Proposed Booster Beam Studies (with CERN) - Charge Question 5b
15 minute presentation

Conveners: Dr. Vladimir Shiltsev (FNAL), Dr. Cheng-Yang Tan (Fermilab)
Material: **slides**

15:50 - 16:20 Illinois Accelerator Research Center (IARC) Activities and Plans - Question 8
15 minute presentation

Convener: Jayakar Thangaraj (Fermilab)
Material: **slides**

16:20 - 17:00 Questions, Answers, and Discussion

17:00 - 18:30 AAC Executive Session

18:30 - 20:30 Drinks at Users Center and Dinner at Chez Leon

Wednesday, December 5, 2018

08:30 - 09:00 Meet with Fermilab Director Nigel Lockyer

09:00 - 10:15 FAST and IOTA - Status and Plans - Charge Question 3
Sasha Valishev - IOTA/FAST Status - 20 min presentation
Aleksandr Romanov - Nonlinear Integrable Optics Experiment - 7 min presentation
Jonathan Jarvis - Optical Stochastic Cooling - 7 min presenation
Giulio Stancari - Electron Lens Experiments - 7 min presentation

Conveners: Alexander Valishev (Fermilab), Aleksandr Romanov (Fermilab/AD/IOTA), Giulio Stancari (Fermilab), Dr. Jonathan Jarvis (Fermilab)
Material: **slides**

10:15 - 10:55 Uprgrade of the Accelerator Controls System - Charge Question 7
20 minute presentation

Convener: James Patrick (Fermilab)
Material: **slides**

10:55 - 11:10 Break
15 minutes

11:10 - 12:25 Target Systems - Charge Question 6a and 6b
15 minute presentation - Cory Crowley - NuMI Megawatt Upgrade
25 minute presentation - Patrick Hurh - High Power Targetry R&D

Conveners: Dr. Bob Zwaska (Fermilab), Mr. Cory Crowley (Fermi National Accelerator Lab), Mr. Patrick Hurh (FNAL)
Material: **NuMI AIP - Megawatt Upgrade** **alt path to HP Targetry R&D** **slides**

12:25 - 13:25 Lunch
1 hour

13:25 - 13:55 Answers to Tuesdays Questions
Material: **T-4 Booster PPT with animation** **T-4 Booster pdf** **T-5 IARC** **Tuesday AAC Questions and Assignments**

13:55 - 15:10 Fermilab High Field Magnet Program
37.5 minute presentation

Convener: Dr. Gueorgui Velev (FNAL TD/MS)
Material: **slides**

15:10 - 15:25 Break
15 minutes

15:25 - 16:00 Questions and Answers and Discussions
35 minutes

16:00 - 17:30 Tours: FAST/IOTA (and SRF?)
1.5 hour

17:30 - 19:00 AAC Executive Session

19:00 - 21:00	AAC Dinner on their own

Thursday, December 6, 2018

08:30 - 09:00	Answers to Wednesday's Questions
09:00 - 11:30	AAC Preparation for Closeout *coffee available 10:15*
11:30 - 12:30	Closeout Presentation and Discussion
12:30 - 12:45	Box Lunches available for AAC Members

Appendix 2: Fermilab Accelerator Advisory Committee Members, December 4-6, 2018

- Oliver Kester (Chairperson) – TRIUMF, Canada
- Mei Bai – GSI Darmstadt, Germany (Wednesday and Thursday)
- Herman ten Kate – CERN, Switzerland
- Bruce Dunham – SLAC, USA (Tuesday and Wednesday)
- Mike Lamont – CERN, Switzerland
- Bruce Carlston – LANL, USA
- Michael Blaskiewicz – BNL, USA
- Roland Garoby – European Spallation Source, Sweden (not attending this meeting)

Appendix 3: Charge to ACC-2018

Fermilab strives to be the best laboratory for neutrino and muon research in the world, as well as a lead lab in the world in accelerator technologies, such as SRF, SC magnets, beam targets, and high-intensity beams. Its particle accelerator complex produces high-power proton beams, muon beams, and the world's most intense beams of high-energy neutrinos. At present, the laboratory operates several neutrino experiments and the Muon g-2 experiment. It is also involved in construction and execution of several high-priority DOE projects: LBNF, LCLS-II, PIP-II, HL LHC-AU, and Mu2e.

In the near term, Fermilab is planning and executing a series of continuous accelerator upgrades in order to increase the neutrino flux for the current NOvA and the future DUNE experiments. The current components of these upgrades include increasing the proton flux up to 900 kW to an upgraded NuMI target system and, in the future, PIP-II to deliver proton beams of 1.2 MW to the

LBNF target, including preparing the current accelerator complex to fully utilize the PIP-II beam intensities.

Research and development continue in the areas of beam dynamics, beam cooling, high-intensity beams, high-power targets, High Energy LCLS-II, the LHC Accelerator Upgrade Program, the High Field Magnet R&D Program, the superconducting RF (SRF) program, and technology commercialization.

The Fermilab Accelerator Advisory Committee is asked to assess and provide advice on the following topics with a concentration on the accelerator physics and engineering:

1. Recommendations from prior AAC 2017

Have all the recommendations by AAC 2017 been adequately addressed?

2. High Field Magnet Program

a. Assess the readiness for the test of the 15 T, 1 meter dipole;
b. Assess the Magnet Task Force reformulation of the Fermilab HFM R&D program;
c. Evaluate integration of the Fermilab program into US Magnet Development Program.

3. FAST & IOTA Progress & Research Program

a. Fermilab has finished the construction of the IOTA ring and began the IOTA and FAST accelerator commissioning, followed by accelerator science experiments. Assess the IOTA and FAST operations and the near-term research program, and comment on the plans and issues. Are the operations plans adequate to enable success of the FAST/IOTA research program?
b. The next major step in the development of the FAST facility is the installation and commissioning of the IOTA proton injector. Assess and comment on the plan and schedule of the IOTA proton injector construction, installation and commissioning.

4. SRF R&D Progress and Research Program

Assess the Fermilab SRF R&D program. Is the progress on the SRF R&D program sound? Is this research program adequately setup to align with and to enable future national and international projects?

5. Accelerator Facility Upgrades

a. Suite of Accelerator Improvement Projects

The work scope presented at AAC 2017 to deliver higher beam power to NOvA and prepare parts of the accelerator complex outside the PIP-II scope has been delayed, making prioritization and scheduling even more critical. Would the priorities of the revised work scope produce the maximum possible number of POT, given the constraints presented?

b. Proposed Booster Space Charge Summer Studies

Assess the proposed Fermilab-CERN Booster study program (2019). The proposed program will incur loss of beam up-time to the experimental program. Does the proposed program have a high impact potential for accelerator physics and future machines? Does the AAC support/endorse proposing the study program to the Fermilab Physics Advisory Committee?

6. NuMI 1-MW Target System Design

a. NuMI Megawatt Upgrade

Assess and comment on the NuMI target upgrade plans. Is the suite of upgrades to the NuMI target station well-conceived to confidently be able to accommodate a proton beam power of up to one megawatt?

b. High-Power Targetry R&D

Assess and comment on high-power target R&D plans and the roadmap. Has the recent campaign of irradiation and pulsed-beam experiments provided fruitful results for HEP? Are the "next steps" of the HPT program well-positioned to make an impact? Is the long-term plan (a.k.a. roadmap) of HPT research appropriate to enable future accelerator facilities?

7. Modernization Program

Assess and comment on the Fermilab Accelerator Control System (ACNET) Upgrade under consideration.

8. IARC – R&D to Commercialize Fermilab Technology

Assess and comment on the Illinois Accelerator Research Center (IARC) plans for R&D to commercialize technology developed at Fermilab.

The Fermilab Director would welcome any other comments the AAC has on any of the topics presented, or on other issues beyond the topics presented.

In addition to a verbal closeout with the management of the Accelerator Division and the Applied Physics and Superconducting Technology Division on the final day of the meeting, the AAC is requested to submit a written report of their findings, comments, and recommendations to the Fermilab Director (cc Sergei Nagaitsev) by February 1, 2019.