Summer Run 2019
Lesson Learned Meeting Summary

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1 Introduction

On September 10, 2019 members of Operations, Engineering, and Physics gathered to discuss lesson learned from the events of the Summer 2019 Run. This document will aim at capturing a summary of the meeting, to which we may develop into formal actions that we can assign. The goal is to roll the actions into the integrated start-up plan, as well as the run plan for the Fall Run that will start in November of 2019.

The thoughts and concepts that are presented here can later be formalized into a coherent list of items.

2 Meeting Minutes

2.1 Before the Meeting

Prior to all participants arriving in the Conference Room Riad S. had mentioned that Hall D has reported in the last few weeks that they have noticed while analyzing their data they have seen beam current modulation. Remember that Hall B had complained during this run about something similar. Riad did not think it was anywhere near the 100 kHz Modulation that Hall B has observed.

Mike Tiefenback and Riad discussed the possibility of the Chopping Apertures as being the culprit.

2.2 What Went Right

Leading this meeting, I felt that we should first discuss what went well during the run. The three things that I presented were:

1. We achieved beam delivery to three users during the run. Toward the end the machine ran well, with very little downtime and happy users.
2. The discovery and change of the 20 Amp Trim magnet cycling protocol. This change will certainly improve the reproduciblity of the machine, from cycle to cycle.
3. The discovery that our magnet field maps (for the large dipoles) were only mapped to 50% of their full operational max plus an additional 10 Amps. We were under this for the run.

I then opened the floor for additional achievements. Ken Bagget added that there had been a significant effort that went into the configuration of the cryo plant for the run. A number of people were involved in understanding how to operate the plant under the heat load, and the conditions that were present for this low energy run. This involved, designation of cryomodules that had to warmed up and a vigilant eye on head load to run the plant in a stable way.

Jay B., then asked the question: "Do we want to extend the magnet field maps?" In the event that we will need to be a lower than design energy again.

Mike Tiefenback, suggested that he had a method that he and a student devised some time ago, that could fit a polynomial to the existing data, and make educated predictions.

Ken Bagget, added that if this was done we need to flag in some way if we are using the measured values vs. the interpolated values.

I suggested that this should be a decision made by management to spend time and resources on this endeavor. It may be low over head to do such a thing.

Others, in follow-up emails suggested that it would be a good idea to go through back populating the field maps. With the possibility of lower energy summer runs and if one of the two CHL plants happen to break in some way during the run, it could be possible to do a one plant low energy run.

2.3 Review of current Repair Assessment Reports

We then took a look at the List of open Repair Assessment Reports (RAR) for this run. You can find the report here:
`https://accweb.acc.jlab.org/dtm/operability/root-cause?start=01-Jun-2019+00%3A00&end=09-Sep-2019+09%3A17&type=1&eventId=&incidentId=&smeUsername=&qualified=&offset=0&max=10&incidentMask=NONE&sort=DURATION&print=Y&fullscreen=Y`

The RAR process is defined at:
`http://opsntsrv.acc.jlab.org/ops_docs/online_document_files/ACC_online_files/repair_escalation_reporting.pdf`
Essentially, a certain amount of downtime will trigger different levels of reporting. All items in the list presented above caused significant downtime, and thus a level 3 or level 4 reporting level. In terms of lessons learned this list reflects items that have already been tagged as needing additional attention in order to understand underlying causes and implement actions. Many items on the list will get individual attention, but some open questions are:

1. Both Fast Valves (VBV1L27 and VBV2L27) failed during the run. In the report for the North Linac Valve the main action is to "Improve the C100 design to limit field emission". Is there anything else we can do (Shielding, valve radiation hardening....etc)? Are we deciding not to re-implement fast valve protection?

2. We lost about 2 days due to a power outage. Recovery was rather quick from the event it self, but the Cryo plant vented a bunch of helium. This in of itself took 2 days to recover from. Is there anything we could have learned here? Are we relying on Cryo to do all of the lessons learned? Could we in Ops have done anything to prevent the Helium loss?

3. Caroll Jones and Steve Suhring reported a number of Action items that were and are being implemented in for the LCW pump flange leak. One thing he mentioned is that are still losing water, and can't identify where, just may be something we need to keep in mind for the upcoming run.

4. An item not seen on the RAR list, but an had a report was the ZA magnet cable that overheated and caused a fire alarm. The corrective

actions in the report, have short term and long term solutions that we should look at and implement. There are certainly lesson, in HCO that we could learn from this incident.

5. Two other items that will be addressed with Spares from the CEBAF Performance Plan was the failing CPS units in the older RF Zones. We should expect to see more fail this upcoming run, as many are reaching end of life. The other is with the failure of the Arc 2 box supply, which a spare is now on-site for.

6. The additional items on the Downtime list are machine and hall beam line tuning and will be addressed in the next section.

3 Beam Transport

We had periods of significant beam setup time through out the run. There were several periods that were difficult to recover beam to the Halls. The following reflects issues that were discussed in the meeting.

3.1 Restoration After Power Event

Operations, along with CASA, spent significant time re-tuning the machine following the power outage. After beam was restored to the halls, we received many beam loss trips, specificity at 4E02 location which sits directly downstream of MYA4E02. The loss seemed to be current dependent. Due to the beam loss and the inability to send beam the request beam current to Hall C, operations cleaned up the steering and loss the best that could be achieved. The steering done as a result in the Spreaders and Recombiners was enough that it changed the optics enough that tuning again was required to reign in the Betatron Match. This was done, and we were still current limited. This was finally tracked down to 1L06-5 which was "silently" draining. This cavity was flagged sometime earlier as needing a tuner card, and we decided not to take the time to do so. The tuner card was replaced, and the cavity brought back on line. We then had no problems in delivering the requested beam current to Hall C.

3.2 ORFP

Yves R. discussed the fact that we used draft procedures for matching the Recombiners using the Differential orbits. This seemed to work, but may not have been consistent among crews. Other useful additions for the upcoming run will be measuring the Bucnhlength in Arc 1, and ensuring that we have an optimal upright bunch in Arc 1. Tuning procedures need to be updated for consistent setup of the machine.

3.3 Wien Angle Changes

Hall A Staff Member Cipran Gal was present at the meeting and discussed that the method used to change the electron spin was better than previously done. The two methods studied last year were, changing the solenoids downstream of the Wien magnets, or changing the wien magnets. Changing the Wien magnets gave better results for Hall A Parity Quality Beam, this was reported by Cipran.

After Wien angle changes the beam sizes in the Halls were in need of adjustment. After every change the subtle difference in the incoming beam made in hard to recover for various reasons in Hall B. Adjustments were also needed in Hall A, as well. Difficulties with both Hall A and B will be discussed below.

There were several periods where beam was unacceptable to one hall and adjustments made that affected the other hall, and had an impact on beam restoration to the other Hall.

3.4 Hall Specific

3.4.1 Hall A

Cipran from Hall A provided a list of challenges observed by Hall A, during the run. There are a number of constraints that should be considered for the upcoming Hall A experiment CREX, which is a Parity Experiment like PREX-II and will have all of the same requirements. I will briefly list them:

1. 2% difference in IHWP in/out of measured polarization
2. Compton background higher than desired, some issue with Hall B bleed through causing high rates.

3. Parity Quality very sensitive to interception on A2 and Master Slit.

4. FFB seemed to cause additional noise on the beam at some periods.

5. Optics Constraints:

   - Beam size at target should be greater than 80x80 μm in x and y
   - Beam size should be small at Compton with low halo.
   - Modulation coils at 1C08/1C10 must give slightly independent behavior at target BPM's 1H04A/E.

6. Hall A staff believes that we need a "recipe" for providing low background at the Compton.

Yves acting as the Hall A , Accelerator Physics Experimental Liasion (APEL), is aware of the constraints and has a plan to address some of these in the Optics design and by updating existing Operations Procedures.

3.4.2 Hall B

Hall B staff member Stepan Stepanyan was present for this meeting, also present was CASA APEL, Mike Tiefenback. Some issues that were discussed:

1. Long restore times after wien angle changes

2. The degradation in the ability to steer beam beyond the tagger, this also contributed to long restore times.

3. Bleed thorugh at times tripping the halo detectors.

3.4.3 Hall C

The Hall C beam run went smoothly. This is mostly due to the loosely based beam requirements, which was mentioned in the meeting by Javier Gomez.

4 Appendix: Documents From Others

Appenddix 1: Run Time Line (Matt Bickley and Brian Freeman)

You can review the crew chief shift logs in time order using this URL:
https://logbooks.jlab.org/search/site/%22Crew%20Chief%20Shift%20Log%22

Some of the significant events that occurred during the run are listed below, to provide a framework for thinking about what's happened over the last 2+ months of beam operation and how we can improve. It's not intended to be a complete list of our successes and challenges:

May 30 : HCO – ZA cable fire (https://logbooks.jlab.org/entry/3684340)

June 16, beam to Hall B (https://logbooks.jlab.org/entry/3688963)

June 19, ARC2 box supply failure (https://logbooks.jlab.org/entry/3690033)

June 21, beam to Halls A, B and C (https://logbooks.jlab.org/entry/3692361)

June 25, VFV1L27 fast valve stuck open (https://logbooks.jlab.org/entry/3693549)

July 1, Power outage (https://logbooks.jlab.org/entry/3697568) - Power outage short but vented helium.

July 6, Helium levels sufficient; beam to 2R (https://logbooks.jlab.org/entry/3699188)

July 10, Beam to Halls A and C

July 11, ORFP restarted (https://logbooks.jlab.org/entry/3702704)

Tight apertures and not able to run higher current to Hall C prompts retune steering and optics tweaks.(https://logbooks.jlab.org/entry/3701829)

July 13, Beam to Halls A and C (https://logbooks.jlab.org/entry/3703518). No Hall B, Upper pass optics never finalized, so the remainder of Hall B setup was delayed until 5th pass setup for Hall C.

July 15, Found 1L06-5 was the cavity that was causing beam loading instabilities. (https://logbooks.jlab.org/entry/3704584)

July 16, Pass change and B tune-up (https://logbooks.jlab.org/entry/3705975)

Also found while setting up vertical split for B and C, that the field maps for the XX magnets did not go down to the energy that we were running. When "design" values were loaded magnet fields went to bottom of mapped region allowing for too much strength at present energy. (https://logbooks.jlab.org/entry/3705642)

July 18, Beam to three halls (https://logbooks.jlab.org/entry/3706394)

July 22, LCW gusher; conductivity too high (https://logbooks.jlab.org/entry/3708186)

July 23, LCW conductivity recovered (https://logbooks.jlab.org/entry/3708700)

August 5, Hall C beam delivery complete (https://logbooks.jlab.org/entry/3715640)

August 13, Work on Hall B FCup, target and SVT chiller (https://logbooks.jlab.org/entry/3718599)

August 14, Wien angle change (https://logbooks.jlab.org/entry/3719064)

August 16, B tuning and CHL trip (https://logbooks.jlab.org/entry/3719898)

August 17, Hall A waiting until B tuning complete (https://logbooks.jlab.org/entry/3720285)

August 18, Two happy halls (https://logbooks.jlab.org/entry/3720625)

Also of note:

Magnet hysteresis issue discovered for the 20 A trim system, reproducibility. (https://logbooks.jlab.org/files/2019/07/3702232/hysteresis.pdf)

Appendix 2: Beam Transport (Yves Roblin)

Lessons learned during spring2019 run

Machine setup

The ORFP process delivered a good machine setup (for the main machine). One significant addition this run period was paying particular attention to using the recombiners to match into the next spreader. There was guidance given to crews on how to do this. For this fall, we will formalize that into an OPS procedure.

A piece that is still missing in the machine setup is how to properly validate the injector setup regarding transverse matching into the NL and longitudinal matching. Both of these require having the beam to 1R at least. We are planning to modify the ORFP to explicitly stop at 1R and validate the injector setup. The transverse matching can be validated bymeasuring with raytrace initiated in the injector as well as measuring with qsUtility in ARC1 spreader.

The bunchlength can be measured in the ARC1 synch light monitor. Also, we can assess how the longitudinal phase space present itself. That is, does max energy gain coincide with min energy spread, etc.. We can use 0L04 to rotate the phase-space to minimize for either energy spread or bunch length. Finally, comparing the setup for all beams is something that should be done at that stage.

Hall A

Moller polarimeter setup

Beam is deflected by the Moller solenoid. Problem was compounded by the fact we were running with a low momentum beam. Measurements were made to attempt to predict how to realign the solenoid to mitigate the problem. The Moller procedure was modified to reduce the deflections by establishing a reference orbit This will be added to the operational procedures before the fall run. (Yves R. Eric F.)

Compton polarimeter setup

Relatively straightforward for this run. The Compton procedure yields a decent setup. However sometimes, it has to be optimized for low Compton rates. There is a specific set of steps that we go through for this to happen. Currently, the OPS procedure does not detail all these steps. We will revise this procedure to make it more explicit (Yves R. , Eric F.)

Fast feedback

Setting up the Hall A fast feedback requires going through the detailed procedure and has been shown to be effective. One systemic issue is that if the main machine is not well matched, we have to rematch in Hall A using the C04-C08 quadrupoles. Since this is where the FFB coils are located, we sometimes end up with suboptimal phase advances between these coils and the bpms. One possible solution is to add extra constraints in the Hall A matching process to ensure we have a good FFB configuration. This is currently being looked at.

Dithering for parity experiments

In order to measure the sensitivity of their detectors to beam induced position differences, Hall A dithers coils that are located in the 1C line. They look at the response on the last two bpms (H04A/H04E) to assess that. This imposes specific phase-advance constraints between their correctors and bpms. Hall A optics need to incorporate these constraints. This is currently being addressed.

Minimum spot size at target

For lead targets, there is a minimum intrinsic spot size that was required (>90 microns) even in the presence of fast rastering. This was discovered mid-run. For future Hall A experiments, we need to inquire in advance whether or not a minimum spot size is needed. If so, optics can be designed to provide that.

Hall B

Hall B had numerous issues with the setup. Part of it was the sensitivity of their HPS detector apparatus to beam halos. Their beamline was instrumented with many halo monitors. An early issue we had was inadequate communication regarding the location of these monitors. Also, steering past the tagger proved to be very challenging. We could not raise the current enough to see it in the bpms since it would trip the counters. The concern was that missteered beam would generate neutrons near their detectors and damage it. Even bleed thru from other halls was enough to impede the setup.

We had several instances of the HPS lock (which uses those last two bpms past the tagger) going erratic and missteering the beam. This could have been due to external factors in the main machine or a bpm sensitivity issue. We also have beam envelope reproducibility problems. Once setup, Hall B would monitor it and it would be stable. However, a machine cycling due to a failure elsewhere in the machine (like a power supply dropping) would result in losing the setup. Further reproducibility studies are needed to diagnose the root causes.

Flipping the Wien filter 90 degrees in the injector resulted in long restore times to Hall B. Both beam envelope and beam steering had to be adjusted.

1) bleed-through from other lasers resulted in uncontrollable (while the other experiments were running) beam current to Hall B. Attempting to "steer up" Hall B while respecting HPS "halo count" limits had the consequence of not only blinding the three (3) BPM systems downstream from the tagger, but also of tripping _all_ beam to all experiments at an intolerably high rate.

2) When passing beam through the Hall B "upstream collimator" adjacent to the tagger magnet, when that beam spilled onto the HPS collimator near the HPS target, the neutron yield was high enough and the source was in sufficiently close proximity to the HPS electronics systems that the electronics packages were readily damaged even with average beam current below "can't see it" levels. I'm inquiring about placing a low neutron yield stopper (borated poly?) for nA current to allow "seeing" on the 2H01 nAmp BPM while stopping the beam additional meters upstream from the HPS tungsten collimator. I suspect strongly that the absolute steering of the beam can be benchmarked and modeled well enough to allow accurate forecasting of the beam location downstream from the

tagger. This could potentially eliminate the hunt-for-it beam spill issue even without the low neutron yield beam stop.

Appendix 3: PREX (Cipran Gal)

2019 PREX run lessons learned

Injector/Source

- **The 2% difference between the IHWP state IN/OUT has been confirmed by all 3 polarimeters and we should try to understand it before the beginning of the next run**
- **90Hz Xposition noise in the injector made PQB more sensitive to slight clipping on A2/Master slit**
- Bleedthrough from Hall B occasionally caused high Compton rates
    - Ramping up the Compton dipole with bleedthrough produces a large (1e6) rate in the Compton DS detectors leading to activation with a long half life. This makes tuning the Compton difficult and possibly damages equipment.
- The vertical Wien flip is far superior to the solenoid flip. The recovery procedure was difficult this run because of the Hall B requirements. It's not clear that with a solenoid flip things would have been removed.
- Compton background rates were adjusted (in the past) using the 0L04 phase or Hall A laser phase, but that produced significant Halo counter rates for Hall B (will this be a problem in the Fall/Spring -- wrt matching hall D-- ?)
- Injector HWP flips and charge feedback mechanism worked without a hitch during the run
    - We plan to use similar tools to feedback on hall C charge asymmetry during CREX
    -

Main machine

- For most of the run it seemed that adjusting the gang phase helped both Hall B and Compton rates, but there were situations where that was not the caseFast feedback/Feedforward
- FFB seemed to introduce additional noise in our measurements
    - The last Wien didn't allow us to use since the FFB would have engaged the slow locks (which would have produced a drift from the orbit that gave good Compton background rates)
- FF however seemed to improve things as long as it has updated data, but was not used when FFB was off

Dithering

- The dithering sensitivities can change significantly with small optics tweaks between their location (1C08/1C10) to the target (1H04A/E). Also these were not repeatable when quads were changed (hysteresis?)
    - Part of the struggle to get them running during the last Wien state was because changes to improve Compton rates produced a drop in sensitivities
- Getting different responses from at least two coils at two BPMs is vital; Mike T and Yves R plan to put a node close to one BPM and diverge the beam at the other
    - (MMD) Having more optics constraints seems undesirable. We already have (beam spot at target, waist at compton, variations to minimize HC backgrounds in Compton). What about having more coils so that we span the space under more circumstances?
- Our energy monitor 1C11X 1C12X were really well separated from the position sensitivity in the 1H line.
    - Yves plans to revert back to the 4m dispersion to minimize the effect of BPM resolution

Compton

- **An orbit through the Compton can be obtained as long as we have a small enough beam on the table with less than 25Hz / uA background rates**
- (MMD) Compton tuning is expensive and operator dependent. During PREX it felt to me like we had to "get lucky". We need a recipe.
- (MMD) Large laser-off asymmetries in Compton suggest HC halo (a la Qweak). Is this Compton data usable? What implications does this have for the main experiment? I.e. Charge asymmetry in the halo implies charge asymmetry in the core via feedback. What diagnostics can we implement for this? Harp and SAMs?
- The 1C20H corrector doesn't seem to have enough power to set the position where we needed it (frequent railing) -- requiring US orbit tweaks
    - (MMD) Why do we sometimes need bizarre orbits to get backgrounds down? Why are they not necessarily repeatable?
    - CREX will be 2x energy
    - We will work with survey and alignment to improve chicane
- For CREX we hope to have an **electron detector** -- bleedthrough may be a significant problem; sync light should not be a major issue.

Moller

- Lock that fixes the trajectory in the Moller spectrometer is critical (solenoid and quad offsets)

- Setting the quads and correctors US of the Moller to fixed values allows us to get a reproducible measurements with a fast setup

Target

- **Designing for an appropriate spot size is important**
    - For CREX we are looking for spot sizes larger than 80X80um (but preferable around 100X100)
- Raster at the target should not be a large concern since one set of coils should be able to drive to the desired size (4x4mm in CREX) -- during PREX we used about 20% of the coil power
    - Well aligned target ladder will be crucial to get low radiation from the collimator
- We noticed the positions at the target (4E) would be stable at a value not set in the lock!?

Dump/Radiation

- The radiation levels in the hall were about what one expected beforehand
    - Increased doses were seem at the end of life for a target (this won't be an issue for CREX)
    - The total levels seem to scale linearly with beam current (for example a new target at 85uA seem to produce equivalent levels to a half damaged Pb target at 70uA)
    - Single event upsets were consistent with expectations and didn't produce large downtimes (required reboots)
- The shielding seems to have worked for the most part allowing us to operate the HRSes continuously (the long downs were due to other issues unrelated to radiation - afaik)
- The total boundary rad dose was significantly lower than the lab limit. Will have to compare with simulation predictions.

For the collaboration: