

Questions & Comments for A2/3 Template Analysis

Unblinding Request

Add any questions & comments on the analysis you may have to the end of the list below. Please make sure to include your name. Constructive feedback and questions from collaborators at all levels are encouraged.

The comment period for this unblinding request ends on Monday, December 4, 2023. The unblinding request slides can be found [here](#) (part 1), [here](#) (part 2), [here](#) (follow up), [here](#) (follow up part 2), and wiki page [here](#).

Questions

0. (Collaborator name) A really good question
 - a. (Respondent name) A satisfying answer
1. (Kaeli) Are we still including the 10-30% cut for SPiceCore angles we didn't trigger on for A2/A3? Or was this issue resolved/understood?
 - a. (Myoungchul) I added new [s/b cut, efficiencies, Aeff, and sensitivity](#) based on spice core angle
2. (Kaeli) Slide 24: Can you explain more about how the estimated shape of the thrown events impacts your effective area calculation? Does the simulation at least save the number of events thrown for each individual run? Otherwise, I'm not sure how you can know the effective area without this (but I could be missing something).
 - a. (Myoungchul) The sim at least saves the number of thrown events. That's why I can draw that level and calculate the effective area
 - b. (Kaeli) okay, great, thank you! This is resolved.
3. (Kaeli) Can you plot the elevation angle you get from the AraCorrelator and AraVertex formulas against the true simulated elevation angles for simulated events as a sanity check?
 - a. (Myoungchul) I added True vs Reco and True-Reco distribution in the [wiki](#). However, I didn't exactly separate the events based on their polarization.
4. (Kaeli) How are you estimating the surface background?
 - a. (Myoungchul) I will just remove the surface region by empirical cut. I tried to model the background before, and it was too unpredictable. An elevation angle of 35 degrees can cover the air/ice transition region that has no separation by shadow region and with our limited radius option in reconstruction, any surface event that has ray solution to the detector should be going through the ice, which has above 35 degree
5. (Kaeli) How are the background estimates combined in slide 63? (I realize you haven't gotten to this yet. I'm just not following what is happening here. But feel free to wait and answer this on Thursday)

- a. (Myoungchul) Since I'm using global cut now, I think I don't have to worry about the combining problem. But I added a new final background number in the [wiki](#)!
6. (Dave Seckel) In your table of configurations pg 6/7, can you include explicit dates?
 - a. (Myoungchul) I added the corresponding date in the [wiki](#)
 - b. thanks
7. (Dave Seckel) what do you do to account for the threshold adjustment period - typically about 15 mins at start of each run?
 - a. (Myoungchul) I'm using it for the analysis. I actually made a cut (it is still living in my code, but it does nothing) to check/remove the data in that period. The reason I decided to use it is 1) C_max (maximum correlation value from interferometry) from that period's event is matched with the thermal noise event's C_max distribution, and 2) it removes around 15 % of the live time from the station, especially A3. I added the detail in the [wiki](#)
8. (Dave Seckel) are the cases of 2 soft triggers just caused by when the clock offsets drift by each other? in which case its not so bad - although getting a proper dead time may take a bit of effort. - ok - i see this discussed in backup. its >2 forced trigs.
 - a. (Myoungchul) I'm sorry for my poorly organized backup slide. I also put in [wiki](#) Yes, the cut is based on >2. I also observed 2 soft triggers in a single second quite often. I added an example in the [wiki](#)
9. (Dave Seckel) are you really doing sims for each configuration? or reusing the same sims but with different configs? i.e. same set of events, calculating the same electric fields, but then applying those electric fields to different detector configs. i guess it might affect statistical uncertainty.
 - a. (Myoungchul) I simulated events for each configuration. So, all events have different electric fields
 - b. thanks
10. (Dave Seckel) in slide 23 there are a couple of bands that run parallel to the shadow line, but offset by 100m or so. Do you know what causes that? Also on slide 48, etc.
 - a. (Myoungchul) based on [plots](#) about each radius, especially the radii 4000, 5500, and 7000 m, I believe the bands are the location where events can easily have ray solutions. I tried to isolate the band by checking different radii, energy, flavor, and direct/reflected rays. But bands present all the parameters of data
11. (Kaeli) Do you have a plan written out describing what you'll do if any neutrino candidates pass your analysis?
 - a. (Myoungchul) Honestly, I'm getting an idea of what to do in that scenario. If you can recommend several options, I will carefully consider it!
12. (Kaeli) If I remember correctly, there were quite a few events that passed the A2/A3 analysis the first time, which were later sorted into background categories. 1) do you know if these events are likely to pass your analysis as well? And 2) is it possible that with the larger analysis livetime there will be new passing events in similar categories?
 - a. (Myoungchul) I believe the passed events are treated by the AraVertex method after unblinding. Since I'm using the same method before unblinding. If I see similar event after unblinding, I think high chance it can be dealt with the AraVertex

13. (Kaeli) have you thought about the possibility of weird passing events similar in structure to the one that passed the phased array analysis? That event came in at a zenith angle of 80 degrees which I think would be in your signal region (assuming it passed your other cuts)
- a. (Myoungchul) It might be filtered out by the AraVertex method. I couldn't see WF from other channels if the arrival time was obvious, It can point out to the surface since AraVertex is using only partial WF region
14. (dzb) Why not just filter the WB events and retain 4.5% more livetime?
- a. (Myoungchul) My lab and I decided to take a conservative approach by removing them. And we want to reduce the possibility that WB events (that failed to filter out) are appearing after all the cuts.
15. (dzb) This CP rate >0.85 Hz is a bit worrisome - do we understand why 1.4% of the time the deadtime is evidently anomalously high, or is this just either i) low statistics and/or a prior trigger occurred within 2 ms of the GPS second?
- a. (Myoungchul) I'm not sure why detectors generally have that amount of time that the calpulser is not functioning as we expected. Also, it is not the contribution from calpulser and software trigger that is interfered with each other. But a low calpulser rate is a sign of bad detector performance, and especially A3 has been observed to have a lots of unstable DDA voltage period
16. (dzb) Cut 4c - this is a minor effect, but when there are 2 software triggers within the same second, does the second trigger always show up at a specific time relative to the first?
- a. (Myoungchul) In my observation, yes. Usually, if there are 2 software triggers within the same second. The first one appears at the beginning of the second, and the second one is always at the end of the second. I added an example in the [wiki](#)
17. (dzb) I still don't get this Chiba in-situ VPol antenna model on slide 22, for a couple of reasons: i) to get these numbers absolutely, you had to apply the Friis equation, I think, which means you had to have the overall channel-by-channel gain (including the amplitude of the Tx signal as well as the amplification and losses in the system including your in-ice noise estimation) known to, like 0.5 dB,) Is that right? ii) the overall normalization doesn't seem right to me - all the points in the plot are 0 dB or larger. However, a monopole with VSWR=1 would have 0 dB over 4π . Unless there are some points in azimuth with negative gain, this antenna model therefore seems to violate energy conservation, no?
- a. (Myoungchul) Regarding the 'i)', yes, In order to get antenna gain from data, I used the Friis equation, Tx signal measured in the lab, signal chain gain, and in-ice noise estimation. [DB1591](#) and [DB1693](#) explain how I got antenna gain from data by following Thomas's method. Then, Simson used this data to construct the model. Regarding the 'ii)', It was completely my mistake. The unit of the gain model on the slide was actually linear (unitless), not dB. I uploaded proper plots on the [wiki](#). In the proper dB plot, the gain is going below 0 dB

18. (dzb) Also, as I understand, the actual data were only taken for positive elevation angles, and you've 'mirrored' those data. However, owing to the presence of the cable, there's almost certainly an asymmetry, no?
- a. (Myoungchul) [DB1838](#) is Simon's slide about model development. And I am not sure we take into account that effect during the antenna modeling (and I'm not sure other models considered that effect). The best we can do is (or I can think of) apply some corrections based on the cable effect in the future.
19. (dzb) Cal pulser veto box, slide 28 - you list losses 0.38% and 0.11% for this cut (A2/A3, respectively), although the boxes on this slide seem to constitute much more area than implied by those fractions.
- a. (Myoungchul) the box in the slide is not accurate. I'm sorry for the confusion. I added the correct box on the [wiki](#)
20. (dzb) To be sure, your 35-degree cut matches Kaeli's 55-degree cut previously, is that right?
- a. (Myoungchul) Yes, I described my cut as a '35 elevation angle'. So, in the zenith angle, it is 55 degrees, which is the same as Karli's cut
21. (dzb) I'd be interested in seeing a 2-dim plot of the Correlator-reconstructed elevation vs. AraVertex-reconstructed elevation, and a similar 2-d plot for z. It seems odd that they have the same angle cut, but different z values. For the original RICE code (which I think AraVertex is based), there was a tendency to pull vertices 'closer', but then the z cut should be dependent on r.
- a. (Myoungchul) I added the plots on the [wiki](#). If 'closer' means close to antennas (antenna center), yes, I saw that tendency. It is actually causing the mis-reconstruction of surface events. So, I limited the search range of the radius to 170 ~ 5000 m
22. (dzb) On slide 42, you seem to have two blobs for Software triggers - I would have thought there would be only one blob that should look just like your thermal triggers (most of your triggers) from data.
- a. (Myoungchul) It is caused by the normalization in AraCorrelator. As an equation that Mohammad showed in his slide, the C_max value can be changed based on WF length. Since a number of N_overlap values of the software triggered event is smaller than RF event, it stands out than RF distribution. But since RF WF has same wf length, if we just look at RF data (also simulate the event in RF WF length), there will be no problem
23. (dzb) Since Brian's data is a subset of your sample, have you compared your background separation from simulated neutrino signal events with his?
- a. (Myoungchul) please correct me if I'm wrong. Do you want me to 1) apply Brian's background separation method to my simulation set? Or 2) apply my background separation method to his simulation set?
24. (dzb) On slide 44, can you overlay the distribution of forced triggers with your simulated noise (and why not just use the forced triggers for your background estimate?)
- a. (Myoungchul) because of normalization process in AraCorrelator depends on WF length. We cannot use software trigger event for background estimation

25. (dzb) For effective area/volume, Alan recently showed us his nice work on the effect of birefringence on expected signals. It would be good to come to a consensus, evaluating the birefringence model vs. data, about whether we should be including birefringence in the $V_{\text{eff}}/A_{\text{eff}}$ estimates

- a. (Myoungchul) I think I can (maybe?) include birefringence effect in systematic uncertainty.

(dzb) Slide 57: We already talked about this a bit on Thursday - yes, it would be good to see a plot of the reconstructed vertex locations (I guess r vs. z) for your analysis, and also for the case where you enforce the efficiency(SNR) curve published in the previous analysis. Dave's idea that the increase in sensitivity is coming from getting a lot more neutrinos at the edge of the sensitive volume makes sense, but it would be good to see that verified in a plot.

- b. (Myoungchul) Regarding the ' r vs. z ' plot, It is in the [wiki](#). Efficiency vs Radius is in this [section](#)!

26. (Kaeli) copying over a question I asked during the call yesterday so it isn't lost: can you investigate how your p value changes depending on how much of the data you use in the tail?

- a. (Myoungchul) Thank you for putting it on here! I added in the [wiki](#) how fit line and p -value are changing based on how much of the data I used (let me know if it is hard to see)

27. (DS) A few comments after today's (12/4) call. Concerning the Spice elevation cut. I have A2 at 2400 horizontal. For Spice at 1200, that's $dz=1000$, and total R of 2600, which gives $el=22.6$. If I use 1100 m for when trig efficiency starts to drop, that's 20.5 deg. It seems 25 deg is a bit optimistic. I have A3 at horizontal distance of 3230, and eyeballing the plot on slide 2, it disappears some 50m later. That gives $\text{ArcTan}[1050/3230]$ which is about 18 deg. Again, 25 deg seems optimistic.

- a. (Myoungchul) I also got the same degrees when I only looked at the 4 runs in [follow up part 2](#) talk. Since I only looked at those 4 runs and A23/2020 analysis used 25 degrees, I decided to safely accept that number. In the future, if I can check all the SPICE measurements, I think I will get much more accurate cut-off degrees
- b. I don't understand. Your plot shows a line at 1200m depth. but that's only 1020 m below the station. $\text{ArcTan}[10.2/24] = 23$ deg. But that's for where there are NO triggers. reduction in trigger efficiency starts earlier, and shallower. A3 is worse.

28. (DS) It may be a small thing, but it confused me while considering Spice. Your (Z,R) plot on slide 3 today notes that $R = \sqrt{x^2 + y^2 + z^2}$. Most other ARA work I'm familiar with uses $r = r_{\text{horizontal}} = \sqrt{x^2 + y^2}$. Comparing the two took me a bit. In the usual presentation there are few triggered events at small r below the station, and this is because a vertex below the complement to the cerenkov cone doesn't illuminate the station. Using spherical R , there also are no events at small R and significant depth, but now it's because that space doesn't exist and there is a minimum value of $R_{\text{min}} = z - z_{\text{sta}}$. For consistency with other presentations is it possible to make the plots vs r instead of R .

- a. (Myoungchul) I totally agree about using ' $r_{\text{horizontal}}$.' I realized a bit later that the 'radius' AraSim gives us is ' R ', not " $r_{\text{horizontal}}$." I didn't have much time to

update the plot/slide before the phone call. I will make sure to update all of them.
I'm sorry for the confusion

29. (DS) In the effective area plot, I'm confused by the comparison to the a23/2020 result. I guess that result includes analysis efficiency, in which case comparing no cuts to a23/2020 is a bit of apples to oranges. Shouldn't the comparison be after cuts, e.g. an updated version of slide 52 from the Nov 16 presentation?
- a. (Myoungchul) Actually, the a23/2020 result is based on trigger level (no cuts in my wording) It is matched with [Fig.5](#) publication. (Well, I used $[m^2]$ unit instead of $[m^2sr]$. So maybe it is hard to compare with Fig 5) and I think I also confirmed with Brian
 - b. Ok. Will wait for an updated plot with apples to apples comparison
30. (DS) referring again to slide 52. It seems the template analysis actually has lower A_{eff} for $E < 10^9$. Is that right?
- a. (Myoungchul) Yes, but lower than A23/2020 trigger level
31. (DS) If so, relative to a23/2020 the template should get slightly worse at low energy. However, comparing to slide 58, the template analysis gets relatively better at low energy, i.e. the gap between a23/202 and template gets larger at low energy. How is this compatible with slide 52?
- a. (Myoungchul) I can calculate the A_{eff} of 23/2020 (including analysis efficiency) based on the number in GitHub. I believe that one can give us a clear comparison!

Comments

- 0. (Collaborator name) A very constructive comment
- 1. (dzb) Extremely impressive amount of work! Because the slides are so exhaustive, it's a big target. My comments above notwithstanding, we should make sure that this result gets out post haste.