Thesis Comments & Responses

Main examiner comments have been left in black; additional comments made by Alex Wright highlighted on the thesis pdf itself added in blue; responses by Daniel Cookman have been made in red.

Kirsty Duffy Comments

General comments

* I found it confusing a few times throughout the thesis to work out whether a plot was stacked or overlaid because you use the same plot style for both. Perhaps try filling in the distributions for stacked?
* Great colour scheme for plots, though!

Chapter 1

* p11 towards the end says SK is sensitive to neutrinos through nu-e scattering. That's true, but could imply it's the \*only\* way they measure neutrinos, which isn't. Maybe "sensitive to solar neutrinos" (if they're under Cherenkov threshold for nu-O interactions)? Or just hedge a little in some other way

Agreed; added “sensitive to solar neutrinos” as suggested.

* p12: clarify the rate of \*electron\* neutrino interactions was too low?

Only some of the experiments I described in the previous paragraphs of the text could observe explicitly the rate of electron neutrino interactions (Homestake, SAGE, GALLEX/GNO). The Kamiokande & Super-Kamiokande experiments measured solar neutrinos via elastic scattering, which can only measure an ad-mixture of neutrino flavours. I felt that saying that the “measured rate of neutrino interactions” being too low was more accurate. Keeping.

* p13: consistent values of the flux of 8B neutrinos \*from each flavour\* or from all flavours combined? More importantly, do you want to make explicit the conclusion here that the total flux was correct but the nue flux was low, which could be explained by nue oscillating into other flavours? I didn't feel that that was quite clear.

Understood; I have replaced “consistent with the SSM” with “consistent with the total flux expected from the SSM”. I have also added a sentence at the end of the paragraph, saying “Because the measured $\nu\_{e}$ flux was low, but the total flux neutrino flux was correct, this was strong evidence for some form of neutrino oscillations.”

* p17 no-zero -> non-zero

Agreed; changed.

* p21 Fig 1.5 (or in caption) make sure to label what is data and what is expectation

I have now added “...as grey and pink bands, respectively.” to the end of the caption, which should clarify this.

Chapter 2

* p27 typo: "It are these PMTs"
* Fig 2.2: I was confused by this plot. In the viva we discussed changing the y axis from absorption length to 1/(absorption length)
* p39 I don't think "divisive scaling factor" has been defined. In the viva we talked about alternatives like "inverse scaling factor" or even defining a variable and saying it scales as 1 over that variable
* p53 typo "N16" instead of "^16N"
* p61: typo - an "effect" that should be "affect"

Chapter 4

* p86 typo: "as many calculations were tried to be put" I think is missing an "as possible
* p99: I was not convinced that it is more likely that the fibre emission points are simulated wrong than that the rope positions are wrong - might be better to hedge this a little.
* Fig 4.9 caption says "a negative sign, and hence bluer colours" but that seems to be opposite to the z axis on the plot
* Fig 4.10 I didn't understand the discussion about the peak at 19ns. Could be worth expanding on the explanation a little, as to why it's \*not\* equivalent to the 14ns bump in the MC
* p117 typo - in last paragraph says "the 20 PMTs closest to the fibre were chosen" but I think it should be "closest to the intersection point"

Chapter 5

* General: the start of this chapter promised to detail the analysis choices made to negate the systematic effects mentioned in the previous chapter. I didn't feel like those were explicitly called out in a super clear way - you could perhaps do that a bit more.
* p118: "A tres window of [-30,-10]ns was used for the isolation of backscattered light in each subrun". Around this point would be good to state what this is for.
* p118: the description of the noise correction is in the paragraph about backscattered light only. In the viva you clarified that it is also applied to "far" light npe, so should move out of this paragraph to make that clear.
* Table 5.1: suggest using something other than commas to separate run numbers (that's done for some of the rows, but not May 2021)
* Fig 5.5 would be easier to interpret if you had something like the mean of that peak near 0, or the fitted mean using a Gaussian fit in that region only
* p123: I don't think this sentence makes sense -- "The proportion of direct light signal to background components in the [-5,+5]ns window is substantial" (I think you want to say the background is substantial compared to the signal, but I didn't get that from this sentence)
* p126: I didn't follow how FS125 corresponds to negative extinction lengths. The page before the plot told us that extinction lengths increase going top-left to bottom-right, so naively this would seem like it's even higher. Can that be explained more?
* Fig 5.8: the post-correction points in black are sometimes ambiguous (e.g. there are at least 3 black squares on each plot). How do I know which corresponds to which pre-correction point?
* p128: It feels a bit weird to have previously pointed out twice that May 2022 and June 2023 don't really agree, and then just end this section with the conclusion that generally they are consistent without any further discussion. Can you expand on this, or reframe how you talk about it earlier?
* Fig 5.15 caption references "non-skipped events" - I don't know what that means
* p137 typo: "with the using the same laser"

Chapter 6

* General: I think it would be really helpful to see scattered e energy spectrum with and without oscillation. I don't have intuition for what I should expect oscillation effects to look like in the measured variable, and I felt like that made it hard to understand some parts of this chapter.
  + For example, bottom of p174 discusses binning but it's hard to understand how that will impact the analysis without knowing the distribution of events we expect.
* p147 "An initial background-free study was performed by Javi Caravaca [155], which demonstrated that it was indeed possible to make such a measurement in the detector " -- this struck me as strange because you said it was one of the main aims of the scintillator phase, so I wasn't expecting that it might not be possible
* p148 "Fig. 6.1 shows the dependence of each of these oscillation parameters on Pee(E)" I think you mean the dependence of Pee(E) on each of the oscillation parameters
* p148, fig 6.1: clearly dm31 has no impact on Pee, but th13 does a little. It might help to make this argument if you could comment on the values you have shown here and how they compare to the global uncertainty on th13
* p 150: "a higher energy neutrino will generate a higher energy scattered electron, on average" -> This is true but may imply a stronger connection between electron energy and neutrino energy than is really there (as discussed in viva)
* p152 first paragraph: is Emin (and reconstructed energy in general) referring to neutrino or electron energy?
* I would make 6.1.3 the start of a new subsection - it's jarring to go straight from reconstruction-level cuts to osc fitting methodology
* p165 end: do we know what beta-gamma events are? I'm not sure I do
* p167 "the resulting matrices are diagonal": not sure if I understand this, and it's probably because I don't understand what the matrix binning is. Showing us an oscillation matrix would likely help, and/or giving that information in text.
* Table 6.5 should be clear about what osc parameters were used to generated expected numbers of signal processes
* p179, end: typo "relative large" -> "relatively large"
* Do you ever explain marginalisation? Not sure if it's necessary but could be useful for a reader unfamiliar with MCMC and multi-dimensional fits
* p181 haven't explained autocorrelation
* Auto-correlation is not the argument I'm used to for burn-in. Wouldn't it be reasonable to expect autocorrelation to change as a function of step number during burn-in?
* p184, fig 6.16: we can't tell from the given information which internal Tl slices are at smaller vs larger radii
* p188, fig 6.19 please label which lines go with which flux constraint
* p192: "Those rates went from 6.06 events per hour..." that sentence I think should end with "for the 212Bi-Po"
* p193: when you say "there is sufficient evidence to confidently reject" you should really say at what confidence level
* Fig 6.22 as with fig 6.19 please add solid lines to legend (and fig 6.24)

Alex Wright Comments

Frontmatter:

Title page – “Trinity Term 2023” – doesn’t this end in June?

Trinity Term at the University of Oxford does end in June, but what follows is the Summer Vacation, which is not really a term, and then Michaelmas Term in October. Because I handed in my thesis (just!) before the start of Michaelmas Term 2023, from what I understand I am still allowed to label my thesis as having been submitted in Trinity Term 2023. Keeping.

Abstract – “result is capable of improving” – alt. “can be improved”?

Agreed; changed to alternative.

Introduction:

2/26 – “List of Acronyms” in header of page 2 should not be there

Agreed; changed so that the header in page 2 now says “Introduction”.

2/26 – remove “still” from “the remaining discrepancies that still exist”

Agreed; changed to alternative.

2/26 – “data is taken is also investigated” – alt. “data is taken are also investigated”

Agreed; changed to alternative.

Chapt 1:

5/29 – “No evidence of neutrinos with positive helicities (or equally, anti-neutrinos with negative helicities) exists” – does this test the existence of positive helicity or the chiral-selectivity of the weak interaction?

As we discussed during the viva, I agree that what I wrote ends up being just a vacuous statement: even if there were neutrinos with positive helicities, the chirality of the weak interaction prevents us from seeing them. I have added the phrase “Because the weak interactions are chiral,” to the start of this sentence to clarify why there is a lack of evidence.

12/36 – “D\_2O” – D isn’t an official abbreviation; we typically define it as ^2H\_2O the first time it is used.

Agreed; in fact I never use D\_2O ever again in the thesis. Changed to ^2H\_2O.

12/36 - “\nu\_{\mu,\tau}” – It might be worth emphasizing that without neutrino oscillations, there would be no \nu\_{\mu,\tau} solar neutrinos.

Agreed; added a sentence afterwards saying “Without a flavour-changing process like neutrino oscillations, no $\nu\_{\mu,\tau}$ solar neutrinos should be observed.”

13/37 – double use of “consistently” in one sentence

Agreed; second “consistently” removed.

16/40 – “P(\nu\_\alpha \to \nu\_\beta)” should be “P(\nu\_\alpha \to \nu\_\alpha)”

Agreed; corrected.

17/41 – “If instead Xij ≫ 1, then as detectors have limited distance and energy resolutions, what can only be observed is the average effect over many oscillations” – I think this is dominated more by the size of the neutrino production region in the Sun.

I wrote this section to be about neutrino oscillations in general, not just solar neutrinos. But you are correct about the case of solar neutrinos. I have added an extra clause to the sentence, saying that “neutrino sources generate them over a region of space”.

17/41 – “give” – alt. “gave” or “gives”

Agreed; changed to “gave”.

19/43 – “However, If the” – alt. “However, if the”

Agreed; changed.

20/44 - MSW discussion

* “Under this approximation, states that are in a given effective mass eigenstate smoothly transform into one another as the neutrinos propagate.” (Adiabatic approximation) – I found this to be a confusing description. I understand it to mean that the neutrinos do not change mass eigenstates during the transition, just the PMNS matrix.

You are right. I have changed this to “Under this approximation, there are no transitions between effective mass eigenstates as the neutrinos propagate.”

* Eg “driven into” -> “produced in”

Agreed; changed.

* “a neutrino generated closer to the core of Sun will have to travel through regions of greater electron density, driving the effective mixing angle θM larger.” – what about the neutrino produced behind the core of the sun that propagates through it?

Good point! I have re-worded the sentence to say “a neutrino that has to travel through regions of greater electron density will drive the effective mixing angle $\theta\_{12}^{M}$ larger.”

22/46 – “The only known means by which sterile neutrinos could have any contact with the rest of the SM is via” gravity. What about EM (via a neutrino magnetic moment)?

I hadn’t thought of this! I have changed the sentence to read “Sterile neutrinos would only be able to interact with the rest of the SM either via perturbative loops (creating a non-zero neutrino magnetic moment, for example), or the above mass term of the Lagrangian; this implies that neutrinos could oscillate into a sterile neutrino state.”

23/47 – “This is only possible in the subset of isotopes for which 2νββ is energetically allowed, but the usual single β-decay is not” – I think this is not true – isotopes which have two allowed B decays can BB decay, the branching ratio is just tiny.

Chapter 2

General comment: I wonder if it would help the non-expert to follow this chapter if it started with a few sentence description of event detection and reconstruction (“SNO+ is an optical detector, meaning events produce photons, which are detected by an array of photosensors. The properties of events are estimated based on the number of photons detected, and their relative times of arrival. This means that the detector must be optimized to achieve a high, stable, and well understood efficiency for creating and detecting photons, and the times of arrival of a photon at a PMT must be known at the ~1ns level.” Etc). That might help to motivate the discussion of optics, PMT triggers, etc, below.

32/56 – Fig 2.2 – I don’t believe any attenuation measurement over ~20m…. Probably best to show attenuation coefficient.

33/57 – “When using just a single scintillating compound, the very same energy levels

that can generate scintillation light are those that can absorb it” I think this is incorrect – recall Stokes’ shift. The fluor does help to suppress self-absorption, but I think the main benefit is that it increases the primary scintillation yield (otherwise we could just use BisMSB and not the PPO – which would have saved a lot of time and money!)

35/59 “in the limiting case we have merely dL/dx ≈ S/kBirks” This can’t be right somehow. kB<<1, for one thing. Plus, I’m not sure it works dimensionally, and doesn’t the LY have to depend on the dE somehow? I think maybe Birks’ linear approximation breaks down for large (kB\*dE/dx), so you can’t take this limit.

38/62 – Eg 2.5. How are you using “scattering off a sphere” theory to get a scattering length in a liquid? The 1/V suggests that the assumption is that the fluid is composed of tightly packed particles? It may be cleaner to just go with the density fluctuation derivation.

Chapter 3

61-85 – “because energy reconstruction is strongly dependent on the number of PMT hits observed in an event, the energy of events will be systematically under-estimated” - don't we tune the light yield? I think it is more the position-dependence of the light yield that would be wrong...

65/89 – Fig 3.4 – what is the timing spectrum with respect to? Does the 0-offset matter?

Chapter 4

83/107

* “One might even assume this distribution is Gaussian.” The angular distribution of light guided by a fibre is known, I think (normally light less than 8 degrees, or something, off axis is guided and otherwise it isn’t). Have you considered fibre modes? “a distinct speckle-pattern can be observed within the beamspot that is not uniform in φ” sounds like a mode-dependent effect

105/129 – Fig 4.11.

* Hmm… It seems odd that the MC would predict that the magnitude of reflected light from the near AV surface and the far AV surface should be so similar in magnitude – I guess if it is perfectly specular and the light was truly “beamed,” but I’m not sure it is?
* Does the MC show any far AV reflection for FS115? It doesn’t look like it, and that peak is certainly seen in the data. That is strange. Could it be deviations of the AV from a perfect sphere?

Chapter 5

112/136 (Fig 5.1) – This was the one that confused me in comparing scattering and attenuation. It might be better to actually calculate the absorption and plot that to make the desired comparison.

117/141 (Fig 5.3) I think the y-axis units are wrong.

125/149 (Fig 5.8, etc) – is the data consistent with the fitted line? What is the fit chisquare? How big would the uncertainties be to make things agree (i.e. add “systematic” term to each error bar and increase it until the chisquare=1”)?

141/165 – “Data taken

one year later, in June 2023, shows no indication of any further change to the

scintillator’s extinction length distribution.” I thought the PQ data on pg 126 does show a shortening? Maybe state explicitly that you trust the PQ data less and are making conclusions discounting that.

Chapter 6

151/175 – I would have liked to have seen some discussion in 6.1.1 of how well the parameters are currently measured, what precision a SNO+ analysis might be expected to yield, and what we add to the global analysis.

175/199 – Table 6.4. For the 8B what sets the inefficiency? Is this just FV & energy, or does reconstruction or something fail for some events that “should” pass?

177/201 “In this analysis, the strongest constraint on the rate of events comes from the global fit constraint of Φ8B. Because of this, the fit will also be run with the looser SSM constraint on

the signal flux as a means of comparison.” You could also just compare the posterior distribution to the constraint. If they are identical then the data hasn’t added any information.

178/202 “this allows for the energy scale to float in the range

13/14 ≤ α ≤ 2.6/2.5 .” Maybe emphasize that this is just a practical constraint from the width of the buffer bin, and that practically the energy was constrained well withing this allowed range (if true).

179/203 – You checked energy at a single point. What about linearity systematic? Also, you have compared data and MC. What about an absolute energy offset systematic (i.e. how well is 5 MeV really 5 MeV)? Presumably that effects the oscillation probabilities, scaling to full energy spectra, etc.

183/207 “parameter space in the same direction, so that the autocorrelation of the chain” – what is “the same direction” and what is correlated?

--I recommend at least mentioning that you did your Asimov tests to validate the fitter. Oftern, pull and bias distributions are shown to validate both the fit and the fit uncertainty.

184/208 “This is 10 times larger than the expected rate

shown in Table 6.5, providing good evidence for a change in the level of this

particular background relative to the water phase.” (34 +/- 18) – What level of evidence would you call this?

186/210 Fig 6.17 – I think this would be easier to read if it had numbers rather than squares.

188/212 – “The fact that the measured value of θ12 is somewhat above the global fit value

also has a reasonable explanation” – is it really above? It’s one sigma – I think it is surprising that it is so close…. I note that in Tab 6.6 changing from HPD to maximum likelihood does move the fit by almost 1 sigma…. That seems like it should be included as a systematic?

191/215 Fig 6.20 – it seems strange that all the high energy/8B data points are above the fit line. I guess the empty bins pull it down? Is this a good fit to the data? What happens if you fit only above 6 MeV?

Other Changes

11 – Moved footnote 2 to a more appropriate part of the sentence, so it no longer ‘hangs’ beyond the end of the sentence.