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.

Note: some minor changes requested in the highlighted thesis pdf have been made but not explicitly commented on in this document.

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?

From my count, there is one occasion where I used a stacked histogram instead of overlaid: Fig 5.14. I have turned the plot into an overlaid version instead of stacked. I also made a few other aesthetic changes – these were done because this plot was used in a NuPhys 2023 conference poster on the SMELLIE calibration system, and had to pass through Collaboration Approval.

* Great colour scheme for plots, though!

Thank you!

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"

Alex’s suggestion about the intro to this chapter has led to this phrase being removed.

* 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)

It is rather confusing – Alex also mentions how the longest absorption lengths (which are the ones the eye is most drawn to) are also the most susceptible to systematic errors. I have made the change to inverse lengths as you suggest, which I think helps to explain what is going on a lot more.

* 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

Understood. I have re-written the sentence to instead say “Subsequently, the scattering lengths were measured to be scaled down by a factor…”, which hopefully is more clear.

* p53 typo "N16" instead of "^16N"

Agreed; changed.

Chapter 3

* p61: typo - an "effect" that should be "affect"

Agreed; changed.

Chapter 4

* p86 typo: "as many calculations were tried to be put" I think is missing an "as possible

Agreed; changed.

* 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.

Understood; I have removed the claim.

* 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

Yeah it does – I have changed it to say “a positive sign, and hence bluer colours”.

* 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

I can see how this can easily be thought, when looking at the plot! I have added the SNO+ PMT transit time distribution, which shows the four main “bumps” of interest: the pre-pulse, direct pulse, and two late-pulse bumps. With this addition, I think it becomes more obvious why the bump at 19 ns cannot be just a shifted version of the 14 ns late pulse bump.

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.

Good point. I have now added a few sentences at the end of the “Mathematical Model” subsection that calls out this explicitly.

* 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"

Agreed; changed.

* 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.

I’m not quite sure what you mean by this?

* 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.

Agreed; changed.

* 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)

Agreed; changed.

* 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

I have now added to the legend the median value of each of the histograms; the results nicely show that the t\_med is much more stable as a function of intensity.

* 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)

Agreed; changed.

* 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?

I have now amended the sentence on the page before, to now say that the extinction lengths asymptote at a certain point, and beyond that become negative.

* 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?

I think we discussed in the viva how the correction that the black points show is a purely leftwards shift on the plot, because they only change the value of Rs. I have now added a sentence in the discussion of the black points in the text to note this fact, hopefully to prevent this issue.

* 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?

You’re right that this seems a bit odd. I’ve added a sentence or so about this disagreement at the end of the section, and noted that it could be due to underestimating the uncertainty of the measurements, in a manner associated with my new comments about the reduced chi2 (see my comments about this in Alex’s section)

* Fig 5.15 caption references "non-skipped events" - I don't know what that means

I never explained this in the thesis, but – SMELLIE events were skipped if there weren’t enough hits in the beamspot to be able to reconstruct the emission time. Because this is such a subtlety that I haven’t explained anywhere else in the thesis, I have now removed “non-skipped” from the caption.

* p137 typo: "with the using the same laser"

Agreed; fixed.

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.

Totally reasonable. I have now added in a plot at the end of the “Observational Principle” subsection, showing the expected reconstructed energy spectrum for the solar signal, assuming different combinations of oscillation parameters.

* 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

Fair enough; I can see how that implication could me made! I have changed the sentence to say “...which made an initial estimate of the sensitivity of SNO+.”

* 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

Oops yep you’re correct – changed now.

* 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

Good point. I have now added a sentence in the body of the text about how the global fit of th13 is sin^{2}theta\_{13} = 0.02220^{+0.00068}\_{-0.00062}, which has an uncertainty over an order of magnitude less the range scanned over in the plot.

* 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)

I have now added a sentence pointing out that the correlation between neutrino and electron energies is only weak. I now also point this out in a later paragraph when I talk about how the observed energy spectrum changes as a function of the oscillation parameters.

* p152 first paragraph: is Emin (and reconstructed energy in general) referring to neutrino or electron energy?

From here on out, “reconstructed energy” always corresponds to the reconstructed scattered electron’s kinetic energy. We generally never try to reconstruct the neutrino energy in SNO+; the weakness of the correlation in the energies of the neutrino and scattered electron means that there’s little point. I have now added “scattered electron” before the first instance of “reconstructed energy” in this first paragraph, to hopefully clarify this.

* 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

Fair. I have now re-jigged the sections/subsections a bit – removing the “Analysis Methodology” section title, and upgrading the “Observational Principle” and “Background Processes” subsections into sections. I have then grouped the remaining subsections of what was originally the “Analysis Methodology” section into a new section, “Statistical Approach”.

* p165 end: do we know what beta-gamma events are? I'm not sure I do

Fair enough! I never explain this background explicitly, even though I later talk about them in terms of handling energy systematics. They’re one of the dominant external backgrounds. I have now included in the subsection on external backgrounds a sentence explicitly pointing out them out, to help the reader.

* 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.

So as I explain in the subsection on including systematics within the fit, we can describe every systematic (as well as neutrino oscillations) as a matrix acting on a vector of a PDF’s bin values. This allows us to describe scaling, shifts, convolutions etc. However, in the special case of neutrino oscillations, the situation simplifies considerably. The value post-oscillations in a given bin is dependent only on the original value in that bin, multiplied by the survival probability for that bin. It is totally independent on what the original values in all the other bins were. As a result, when you cast this in linear algebra terms, the matrix which described the transformation from pre-oscillations to post-oscillations must be diagonal.

* Table 6.5 should be clear about what osc parameters were used to generated expected numbers of signal processes

Ah, there’s some confusion – the expected number of signal events are before any oscillations have been applied! I have now added “unoscillated” to the relevant parts of the table to make this clear.

* p179, end: typo "relative large" -> "relatively large"

Agreed; changed.

* 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

I do (briefly) explain marginalisation, in the subsection on the “Bayesian Statistical Approach”: “This is done by “marginalising” the posterior distribution, i.e. integrating over all parameters other than the one of interest.”

* p181 haven't explained autocorrelation

Good point; see next comment.

* 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?

You’re right. I’ve now removed all mentions of auto-correlation, which I only ever used as a post-hoc check out of curiosity, and replaced them with brief descriptions of the qualitative process I used.

* p184, fig 6.16: we can't tell from the given information which internal Tl slices are at smaller vs larger radii

Good point. I have now added a sentence describing the numbering in the text at the appropriate place.

* p188, fig 6.19 please label which lines go with which flux constraint

I have now added labels for the posterior densities to the legend.

* p192: "Those rates went from 6.06 events per hour..." that sentence I think should end with "for the 212Bi-Po"

Agreed; changed.

* p193: when you say "there is sufficient evidence to confidently reject" you should really say at what confidence level

Good point. For the 2 year Asimov dataset, the total posterior probability > 2e-4 eV^2 is only 0.1%. This corresponds to a rejection of 3.1 sigma (using a one-tailed Gaussian). I have now added this information to my statement.

* Fig 6.22 as with fig 6.19 please add solid lines to legend (and fig 6.24)

Agreed; changed like in previous figure.

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.

Understood; I have now re-written the first couple of paragraphs of the chapter to try and add this information in a sensible way.

28/52 – “within which”: alt “upon which”

Agreed; changed.

29/53 – “not expected to impact”: alt “not expected to directly impact”, given that you’ve just said that the goal of adding BHT is to prevent the degradation of the optics?

Agreed; changed.

30/54 – “α, β±, ɣ, p or n.” Not all of these directly ionise, so you should remove the ɣ and n, and replace them with two missing particles, μ and π. (Otherwise you should also include neutrinos!)

Understood; I have made the replacement as you suggest.

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

Agreed! I also think that absorption coefficient is a better thing to show next to emission spectra, and will lead to less confusion. I have made the change, and think it helps a lot.

32/56 – “ionisation of atomic electrons”: alt. “ionisation of atoms” (you cannot ionise an electron!)

Agreed; made change.

33/57 – “from LAB”: alt. “from LAB-PPO”

Agreed; made change.

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!)

This was just a misunderstanding of the photochemistry on my part, oops. I have re-written these paragraphs somewhat to get rid of the false claims about this stuff, and replaced them with something that I hope is more accurate.

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.

/If/ you assume Birks’ Law to be true for any dE/dx, then I’m pretty sure the limit does hold – by dividing the top and bottom of the fraction by dE/dx, you get the formula:

dL/dx = S/(1/(dE/dx) + kB),

which in the limit as dE/dx tends to infinity just equals S/kB. The units do work out – S has units photons/MeV, and kB has units mm/MeV, so S/kB has units photons/mm, which matches the units of dL/dx.  
As you say though, the above argument doesn’t hold if there are higher-order terms in the denominator O([dE/dx]^2). I have removed the discussion of this limit in the text, and just left it saying that “for $\alpha$-particles generated in radioactive decays, this denominator can become substantial.”

36/60 – “generating”: alt “generates”

Changed.

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.

I can see that this got a little confusing...the connection from one to the other is a bit complicated. As you suggest, I have re-worked the section to focus mainly on the density-fluctuation theory. I do keep a small mention of the original “scattering off a sphere” theory, because that was how Lord Rayleigh first worked came up with the initial theory, and therefore why it’s called “Rayleigh scattering”.

41/65 – “boundary of one medium to another”: alt. “between media”

Agreed; changed.

44/68 - “as well as the collection efficiency of a photoelectron onto the first dynode of the PMT” – the collection efficiency should be the subsequent probability that the pe gets recorded.

Understood. I have re-phrased this sentence to make this clear.

44/68 – “The dynamics of this cascade” – it’s the transit time of the photoelectron to the first dynode that generates the TTS mainly, not the subsequent cascade.

Understood; I have re-phrased the paragraph to correct this.

50/74 – “24/7” – too slangy?

Agreed; changed to “whenever”.

50/74 – “convert”: alt. “allow conversion of”

Agreed; changed.

51/75 – remove “in ns”

Removed.

51/75 – “Laserball” – has the Laserball been mentioned previously?

The Laserball is very briefly mentioned in the “Absorption and Re-emission” subsubsection, to explain where our current measurements of the extinction lengths of the UPW and acrylic come from. I then also briefly mention them later within “Energy and Optical Calibrations”. However, I never did explain what the Laserball actually was! I have added a fairly brief description of the Laserball, just before describing the hardware of TELLIE, as that seemed a sensible place to put it.

52/76 – “’Scattering Module’ for the ELLIE calibration system”: alt. “of” instead of “for”?

Agreed; changed.

53/77 - “The first is an americium-beryllium (AmBe) source inherited from SNO [128],

which contains 241Am that α-decays, which can be captured by the 9Be within

the source.” - This sentence has at least one dangling participle.

This sentence is a grammatical mess, indeed! I have lightly moved the punctuation around in this and the next few sentences to hopefully try and avoid this problem.

53/77 – “captured by hydrogen in the detector” – should add the caveat typically.

Agreed; added.

54/78 – “Using BiPo214 events in particular has been used…”: alt: “BiPo214 events in particular have been used”

Agreed; changed.

54/78 – Delete “cases of”

Agreed; deleted.

54/78 – “ScintFitter is”: alt: “ScintFitter are”

Agreed; changed.

56/80 – “At low energies, scintillator quenching becomes non-negligible” – even for electrons? And what about the non-linearity of Cherenkov production?

In a formal sense, low energy electrons do have a greater energy loss per unit length than higher energy ones; and that does lead to some amount of scintillator quenching from Birks’ Law. However, “non-negligible” might be taking it a bit too far.  
The non-linearity of Cherenkov production is also an effect, but I suspect the amount of light gets swamped in all the scintillation light, so the overall impact on energy reconstruction will be pretty small.  
All in all, to prevent people thinking that these are big effects for electrons, I have removed the sentence.

Chapter 3

60/84 – “This can be done by comparing the fraction of light emitted by the fibre that

gets observed on the far side of the detector” – comparing it with what?

I think the work “comparison” is causing some confusion - I was trying to say that I am comparing the amount of light emitted by the fibre to the amount observed on the far side of the detector; this is equivalent to measuring the fraction of light. I got a bit muddled between these two equivalent statements though, and ended up with something not quite right. I have now changed it to “observing the fraction of light”.

61/85 - “ If these lengths are systematically off within simulation”: alt “if incorrect values for these lengths are assumed in event reconstruction”

Agreed; changed.

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…

Yep, agreed. This becomes even more glaring as I do a version of this very energy scale tuning in my solar analysis! I have removed this sentence, and instead talked about how the position non-uniformity of the energy reconstruction can become incorrectly modelled.

62/86 – “the current contents of the calibration system” – what do you mean by this?

By “contents”, I just mean “hardware”; I just wanted to use a different word for some variety.

64/88 - “The emitted light needs to be intense enough to actually generate sufficient statistics for an analysis”: alt. “ The emitted light needs to be intense enough to generate sufficient PMT hit statistics for an analysis”

Agreed; changed.

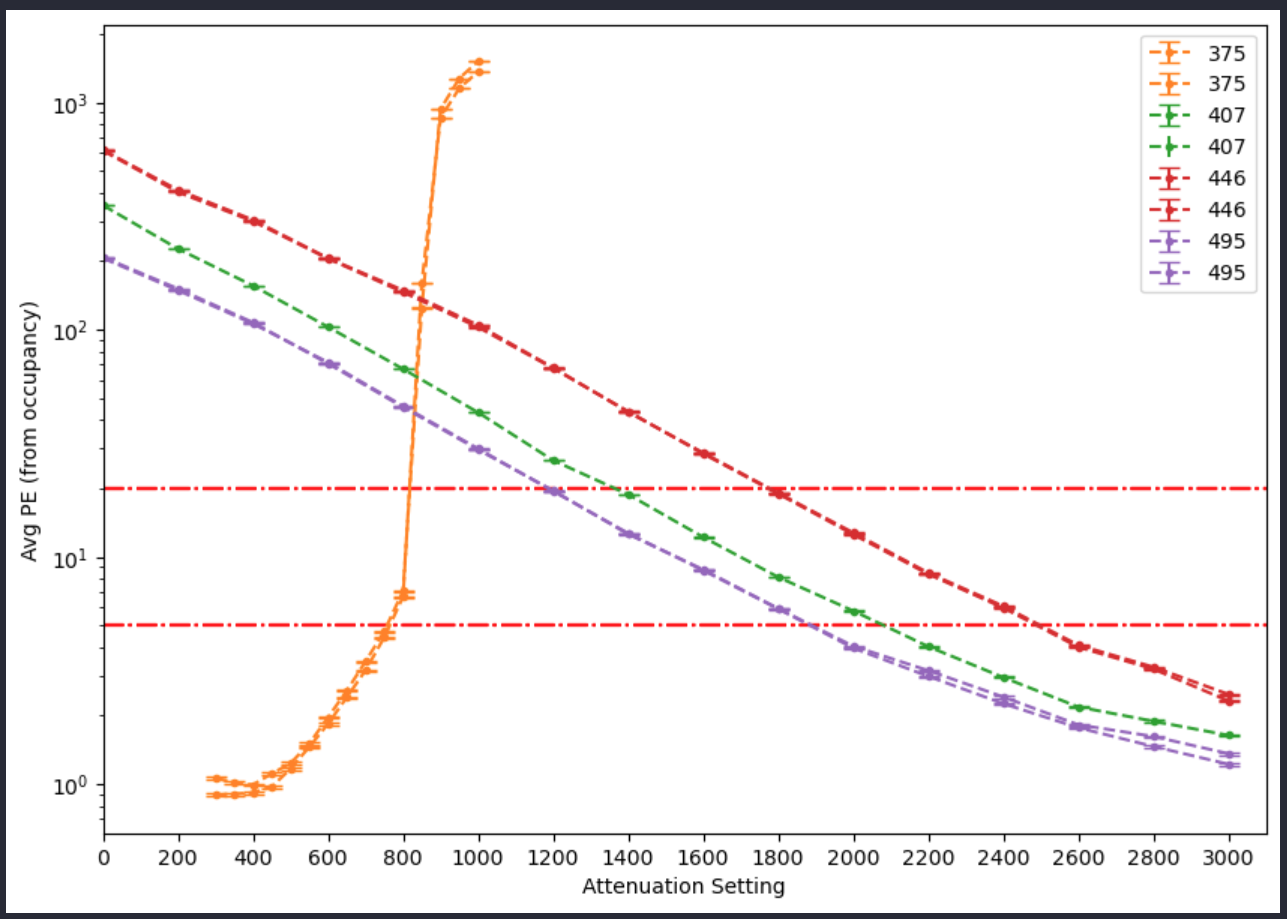
65/89 – Fig 3.4 – what is the timing spectrum with respect to? Does the 0-offset matter?

I am not too sure what the t=0 values actually correspond to here. It doesn’t matter for SMELLIE analyses currently, as we measure times relative to the reconstructed emission time from the fibre.

66/90 – “maximum PMT occupancy” – what does this mean?

This means the PMT occupancy which is largest in the detector for a given subrun. I have rewritten the sentence to clarify.

69/93 - “over multiple orders of magnitude of observed intensity” – I don’t think there is enough information in Fig 3.8 to support this claim; is it otherwise supported?

There is a version of this plot which shows total NPE in the detector vs Attenuation, instead of maximum PMT occupancy, and quite nicely shows the multiple orders of magnitude in intensity:  
However, I didn’t want to introduce the idea of calculating NPE until the next chapter, where it feels much more natural.  
So – the data in Fig 3.8 /does/ support the claim, but I agree that it’s not particularly obvious, given that I’ve boxed myself into using only maximum PMT occupancy in this chapter.

Chapter 4

81/105 – “By modelling the laser light emission into the detector correctly”: alt. “By modelling the laser light emission into, and propagation within the detector correctly”

Agreed; changed.

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

Fibre modes is very much the leading hypothesis for the speckle-pattern effect. I have added a sentence noting this.

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?

The simulation does assume perfectly specular reflections on the AV; whether this matches reality is another question! I also find this effect somewhat surprising.

* 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?

The MC shows a negligible amount of far AV reflections for FS155, in strong contrast to the data. I agree that it is strange! One possibility could be an increased amount of scattering within the acrylic, which would allow light that hit the far AV to come back at non-specular angles.

108/132 – “In this chapter, the simulation of SMELLIE events was updated in two ways.”: alt. “This chapter describes two updates to the simulation of SMELLIE events.”

Made the change, except using “described” instead of “describes” as this is the conclusions section of the chapter.

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.

Understood. I have now added the absorption length distribution for LABPPO into the plot, so that you can now see the expected dominance of the scattering at long wavelengths.

113/137 – “radially-opposite”: alt. “diametrically-opposite”?

Sure; changed.

114/138 – “length of path”: alt. “path length traversed”

Agreed; changed.

114/138 – “between two mediums”: alt. “between two media”

Agreed; changed.

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

Agreed; changed.

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”)?

I have now added the fit chi2 & ndof to the plots: a good idea. As suspected, the chi2/ndof ratio is somewhat larger than one in all cases, typically a value of ~10. This indicates that there remains some inconsistency between data and the fit model.

This is something I want to investigate more in the future. However, for the purposes of this thesis, I have written a paragraph in the results section describing the above.

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.

Agreed; I have added such a statement, linking it to the possible underestimation of the error described in the extinction length analysis.

143/167 – “5%”: you mean “50%”, right?

Nope, 5% is right (we discussed this in the viva). 5% is the percentage error on the extinction length of the /scintillator/, after propagating the 50% error on the extinction length of the water.

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.

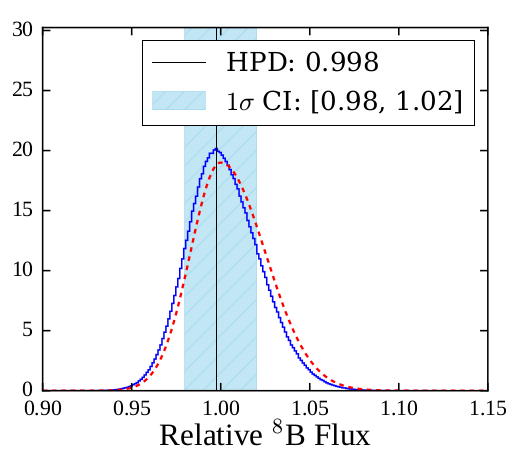
This is fair. I don’t have lots of space to go into detail, but I have now added a long paragraph and accompanying pair of figures on this topic. In particular, I show the NuFit 5.1 global fit contours, and also show the contours made by Javi Caravaca’s initial SNO+ sensitivity study.

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?

FV and energy both dominate the inefficiency. There is a third cut that has an important impact: fitValid. This cut removes over a third of triggered B8 events, and this is because it removes most of the re-trigger events. A large fraction of B8 events have at least one re-trigger event.

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.

I have this very plot in the thesis (prior in red, posterior in blue):  
As can be seen, there is a small change between the prior and posterior here, indicating that the data does provide a small amount of additional 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 within this allowed range (if true).

Agreed; I have now added this.

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.

Energy shift and non-linearity systematics are certainly possibilities, and could theoretically have impacts on the oscillation measurements, as you describe. This could be done through use of the AmBe calibration source, for example, which generates events with multiple distinct energies. Ideally, it would be nice to check our energy reconstruction at high energies (say ~10 MeV) somehow, but this cannot be done with most radioactive sources.

In general, you need:

- at least one calibration source to measure the energy scale

- at least two calibration sources to measure the energy offset

- at least three calibration sources to measure the energy non-linearity

I have now added a paragraph in the thesis mentioning these systematics.

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 have now removed my discussion of auto-correlation and replaced it with something that makes a bit more sense – see my comments responding to Kirsty.

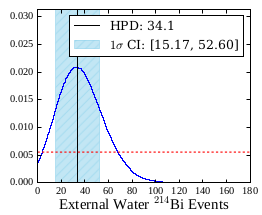
--I recommend at least mentioning that you did your Asimov tests to validate the fitter. Often, pull and bias distributions are shown to validate both the fit and the fit uncertainty.

Good point. I have now added a short paragraph on this matter. The marginalised distributions shown in the sensitivity study section can also be used as de facto pull/bias distribution plots, if you want to interpret them that way.

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?

Hmm now that you point this out, the posterior distribution for the external water Bi-214 event rate still has a relatively large posterior density at the water phase rate of 3.42 events:  
The Bayes factor between the water phase and HPD values appears to be from this plot ~4, which is not much. So despite the fact the HPD value is much larger, the width of the posterior density is quite wide, presumably because of the many correlations between it and other externals. I have now downgraded my claim from “good evidence” to merely “some evidence” in the text.

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

Okay. I have remade the plot, now showing both the values in both text & colours (the squares are still there, they’re just now all the same size).

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

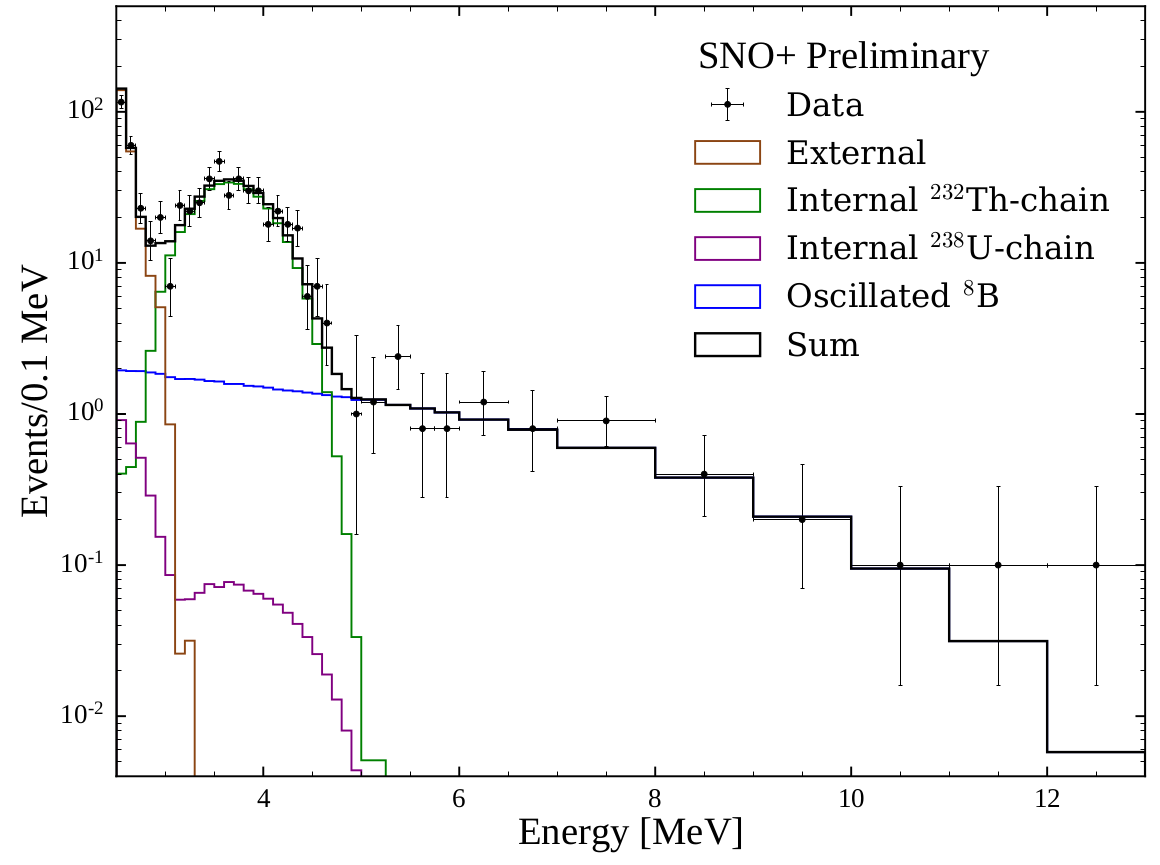
also has a reasonable explanation” – is it really above? It’s one sigma – I think it is surprising that it is so close.

That’s fair. I was just trying to point out that, by having chosen such a conservative prior on dmsq21, this leads to a shift in the 1D marginalised distribution for theta\_12. This becomes much less of a problem if one changes the prior, as I show. I have now removed the first sentence of this paragraph in the text.

...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?

In a sense, it already is! I float both oscillation parameters simultaneously in the fit, and there is very much a correlation between them: this can easily be seen in the 2D marginalised oscillation contour plots. This systematic effect manifests itself when looking at the 1D marginalised distribution for theta\_12 as a shift in the distribution. It also broadens the distribution, compared to the profile distribution for theta\_12 at the global fit value for dmsq21.  
In short, the oscillation parameter dmsq21 can be thought of as a “nuisance parameter” for the measurement of theta\_12, and my MCMC fit handles that appropriately.

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?

There are few statistics at high energy, so most bins only have at most a few events. Numerous bins are empty, pushing down the average. Here is a plot showing the same data and MC, but now summed over all the radial slices:

I think this shows qualitatively that the fit is fairly good. I am curious about the event in the highest energy bin, which according to the MC should be quite unlikely. One thing I would like to investigate in the future is whether I am missing a high-energy background component, possibly from atmospheric neutrinos.

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.

154 – The BiPo coincidence tag uses a 4 ms deltaT cut, not 4 us! Fixed now.