

**A Precision Search for Exotic Scalar and Tensor Couplings in  
the Beta Decay of Polarized  $^{37}\text{K}$**

by

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# Abstract

Abstract Goes Here

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# Shit To Do

■ JB on the contents of Chapter 1:	
Move what you have in 1.1 and 1.3 to the first section of Chapter 2, and otherwise omit Chapter 1.	1
■ more from JB on the contents of Chapter 1:	
With three changes:	
...	
1)"and we shall be interested especially in scalar (S) and tensor (T) couplings." -> "our observable is mostly sensitive to scalar (S) and tensor (T) couplings."	
...	
2) "These couplings all refer to parameters in a Lagrangian that takes the relativistic inner product of a current for the lepton with a current for the proton or neutron. The resulting Lagrangian must be a scalar under Lorentz transformations, so these currents must have transformations like these V,A,S, and T and be combined into a scalar."	
...	
3) Add one reference to the latest review: Adam Falkowski, Martín González-Alonso, Oscar Naviliat-Cuncic. Comprehensive analysis of beta decays within and beyond the Standard Model. Journal of High Energy Physics, Springer, 2021, 04, pp.126. 10.1007/JHEP04(2021)126.	
...	
You have time for nothing else.	1

Did I even get this right? Is the phase angle really what makes it left-handed?

JB says:

• • •

Relative sign. look at the quark-lepton Lagrangian, which has  $(1 \pm \gamma_5)$  .

2

■ Cite a bunch of people here. (Who?) Might be nice to have a picture also.

2

- According to present limits, these couplings would have to be pretty small relative to the (V) and (A) couplings. . . . . . . . . . . . . . . . . . .

2

■ Need to figure out how the exotic couplings actually work, mathematically.

What the fuck does “( $V - A$ )” even \*mean\*? IIRC John wants a brief mention of  $\gamma_5$ ’s and  $\gamma_\mu$ ’s, and probably a brief mention of whatever mumble-mumble group is mumble-mumble represented or something.

•

JB says:

the current transforms like a Lorentz scalar or tensor – this does not refer to the angular momentum.

If you write down the Lagrangian for beta decay, that's enough. All these things refer to the structure of the Lagrangian. The theory considers all possible Lorentz transformations of the currents.

Please don't talk about  $SU(2) \times U(1)$  for electroweak unification. It's textbook material that's beyond the scope. . . . .

■ JB on intuitive concepts that are missing:	
The SM couples to left-handed neutrinos and right-handed antineutrinos. Since the neutrinos only have weak interactions, there are no right-handed nu's nor left-handed antinu's in nature. The neutrino asymmetry $B_\nu$ is a number with no energy dependence.	
Similarly, the SM weak interaction only couples to right-handed positrons and left-handed electrons. Since these are massive particles, the average helicity of positrons is not 1, but instead v/c. One can always boost to a frame where the positron keeps its circulation but is moving in the opposite direction. This is why the beta asymmetry is A v/c, not just A.	
The Fierz term's additional energy dependence of m/E also comes from helicity arguments, stemming from the fact that it still is coupling to SM nu's and antinu's only, so the beta's are generated with wrong handedness. The details are built at 4th-year undergrad level in Garcia's paper with his student and postdoc [3].	
The beta asymmetry dependence on the Fierz term only comes through the normalization of $W(\theta) = 1 + b_{\text{Fierz}}m/E + A_\beta \cos(\theta)$ .	
i.e.:	
$W'(\theta) = 1 + A_\beta/(1 + b_{\text{Fierz}}m/E) \cos(\theta)$ . (the angular distribution must be unity where cos(theta) vanishes, by definition).	2
Figure: I need that simulated picture of the different beta energy spectra, with different values of $b_{\text{Fierz}}$ .	3
■ JB on that missing figure: "A dependence of Abeta on beta energy is also introduced.	
UCNA fits energy spectrum and Abeta[Ebeta] simultaneously now."	3
■ Do I really even *want* to include a toy experiment? And would I want to do it here?? What even is the point? I think in the past I decided it was easier to build up a description of .... something .... starting this way. But why?? Possibly as I continue to add content, it will become obvious again why I originally wanted to do this.	4
■ JB says: "Your experiment is simple enough to describe without a toy." (He's said this twice now, so I should probably just kill this section.)	4
■ JB says Chapter 1 sections 1.1 and 1.3 should get moved here into Chapter 2.	5

█ JB on what (else) should change in Chapter 2:	
...	
cut "so we will simply provide the combined master equation here"	
Don't. The equation you have is all you need. . . . .	5
█ Is this even true? The pointlike thing? ...No. No it's not. . . . .	5
█ JB says: The title of Holstein's review addresses this "pointlike" issue, and he describes the "impulse approximation" in Section V. The interaction is not pointlike, because all constants are a form factor expansion in $q^2$ – finite size terms contribute to the Coulomb correction. . . . .	5
█ Do it! Do the master equation! . . . . .	7
█ JB: 2.3 Our decay: Just put the comments in. Keep the figure as-is.	
MJA: Pretty sure the comments are literally copy-pasted from somewhere I shouldn't just plagiarize from. Need to rephrase it at least. . . . .	7
█ "Here, we focus on the decay $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$ . The angular correlations between the emerging daughter particles provide a rich source of information about the type of interaction that produced the decay." . .	7
█ "Of particular interest is the decay process: $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$ . Among other useful properties, this is a 'mirror' decay, meaning that the nuclear wavefunctions of the parent and daughter are identical up to their isospin quantum number. This property allows us to place strong constraints on the size of the theoretical uncertainties for this decay process within the Standard Model. " . . . . .	7
Figure: This thing is going to need a nuclear level diagram for $^{37}\text{K}$ . Also, $^{37}\text{K}$ is a really nice isotope for this, because 98% + 2%, also because it's a mirror decay, also because it's an alkali. Also-also, its big $A_\beta$ value means we have a big thing to multiply any $b_{\text{Fierz}}$ value there might be when we construct the superratio asymmetry to eliminate systematics. . . . .	7

#### JB on 2.4 SOE:

Say what you did, with as little info as possible now. Georg is a theoretical chemist, so he may be very curious about this.

"Atomic electrons with kinetic energies 0 to 100 eV are produced as part of the beta decay process. If their energy is below a certain value, our detection process is not perturbed, so we provide physics about the energy spectrum here.

Levinger [4], assuming the sudden approximation, calculated the overlap between an electron in the initial atom with an outgoing electron or an electron in the final atom. Levinger approximated everything with hydrogenic wavefunctions, so his calculations become analytic. The collaboration has found that Levinger's formulae fit our measurements of the position and TOF info of our atomic electrons, so they are used in our simulations

QR

but the precise energy spectrum was found to be unimportant, so we used X in our simulations.

whichever is true.

This is then fine. You have most of the basics down. . . . . . . . . .

Really, just discuss the physics of what happens to cause SOEs to be a thing.

Should I talk about the distribution of how many SOEs come off in a decay?

I have measurements of the recoil charge distribution, which is related but not really the same thing. From a theoretical POV, I don't know how many get shaken off. Thankfully, it doesn't matter very much in the end.

█ First, I'm pretty sure you should combine Chs. 3 and 4. Ch 4 has good solid general description and details in situ about optical pumping and about the photoionization probe that are, unfortunately, wrong in Ch. 3. You could call the first half of such a chapter "General considerations of Atomic techniques used" and the 2nd half "Experimental Implementation of Atomic Techniques used" . . . . .	11
█ wtf could I possibly have gotten wrong in my Ch.3 description? It's so vague!	11
█ I need to organize the sections/subsections in this chapter better. . . . .	11
█ Have I defined TRINAT yet? . . . . .	11
█ Possibly most of the above paragraph is also written/paraphrased elsewhere. Like in Ch-4, unless I removed it... . . . . .	11
█ “In order to understand the mechanism by which a MOT is able to confine atoms, we must first introduce the Zeeman effect (Section 2.1) and a description of an optical molasses (Section 2.2). A functional MOT combines the forces resulting from these two mechanisms to trap and cool atoms.”	12
█ Needs a level diagram. Maybe. . . . .	13
█ When this is combined with a circularly polarized laser beam, the effect is to move the atomic resonance closer to- or farther from- the frequency of the laser. The circular polarization, combined with some selection rules, means a circularly polarized laser will only couple to one particular transition, w.r.t. angular momentum. ie, for a $\sigma_+$ polarized laser, the atom’s overall angular momentum projection (along some axis) will be incremented by +1. The Zeeman shift means that in a magnetic field, this transition ( $M=+1$ ) not be the same as the $M=1$ transition. So, if you have a magnetic field that changes linearly across space, you can make it so that in $+B_z$ regions, the laser beam with one certain polarization is closer to resonance and therefore more likely to be absorbed – and similarly, in $-B_z$ regions, a different laser with the opposite polarization will be more likely to be absorbed. Again, if the B-field is linear in space, you can do it so that as the atoms get further and further from the ‘centre’ region, the effect gets progressively stronger. So, if you’ve done this right, you can make it so that the atoms get a stronger “push” back towards the center the farther away they’ve drifted.	13
They still get the optical molasses cooling effect for free. . . . .	13

■ JB says: Chapter 3 (that's this chapter): Starting in Ch. 3 around p. 10, "Atom-Photon Interactions with a Laser" (that's this section): You need to do a careful pass through it and omit everything you don't understand and don't need.	13
Sorry, there is no longer time to resolve your questions unless you need them. . . . .	13
■ This is a stupid subsection title. . . . .	13
■ Also, if the photon is tuned close enough to atomic resonance, with absorp- tion being more likely the smaller the detuning is... . . . . .	13
■ Where have I even decided to put the photoionization section, anyway? . . .	14
■ Clunky phrasing. . . . .	14
■ Do I need saturation to make an optical molasses? I bet I do. . . . .	14
■ But yeah, it seems like I have to mention that saturation intensity is a thing that exists, before I proceed to neglect it forever. . . . .	14
■ JB says: "In practice, for the project that is the topic of this thesis, most lasers operate in the high intensity regime – the obvious exception being the photoionization laser (see Section whatever) However, one can still gain ..." . . . . .	14
change to -> "In practice, for the project that is the topic of this thesis, the MOT laser operates at about 1/2 of saturation intensity, while the optical pumping and photoionization lasers operate at considerably lower than saturation intensity. However, one can gain ..." . . . . .	14
■ ...and opposite polarization. Or something. I have to talk about the selection rules somewhere else. . . . .	15
■ Optical molasses equation? Maybe? . . . . .	15
■ "...This will slow the atom down, at least up to a limit related to the linewidth of the atomic transition and/or the laser. There's something to look up." . . . . .	16
■ JB: 3.1.4 Angular Momentum and Selection Rules (that's this section!): Sorry, this section is almost entirely wrong, and you don't need it. Omit it please. . . . .	16
■ JB: Agreed it's ok to ignore the repumper laser. . . . .	16
■ Fuck. What even *are* the other rules? Have to change F too? I think that's true.. Clearly I have to look this up. . . . .	16



█ JB says: "I would say you don't need an atomic level diagram. You could just describe in words the semiclassical picture of atoms absorbing photons until they are nearly fully polarized, then they stop absorbing. The optical pumping + photoionization is then an in situ probe of the polarization. ... You would need to add in words that quantum mechanical corrections to this picture are in the optical Bloch equation approach in B. Fenker et al. The depolarized states still have high nuclear polarization (1/2 for $F = 2, M_F = 1$ , 5/6 for $F = 1, M_F = 1$ ) and determining the ratio of those two populations provides most of the info we need – we model with the O.B.E, measure the optical pumping light polarization, and float an average transverse magnetic field. This is adequate to determine the depolarized fraction to 10% accuracy, which is all that is needed." . . .	17
█ Describe what's going on here! . . . . .	18
█ JB: ...Then you can refer to that (currently section 3.2, "Optical Pumping") in section 3.4, where you're trying to now but the phrasing is poor.	
3.3 Atom Trapping with a MOT and	
3.4 The AC-MOT The content and scope are ok, but the informal phrasing is going to confuse people. You have to pay some attention and rephrase these sections. . .	18
█ Do I *have* an equation I can put here? Surely there must be one somewhere, but I really don't want to dig it up, and I don't think it's really necessary.	18
█ Needs work. . . . .	18
█ Do I *really* need to bring up the repumper? I don't wanna. Maybe I can just mention it and move on or something. . . . .	19
█ Needs work. . . . .	19
█ How much do we lose? Have we quantified that somewhere? Probably. . .	19

■ JB on 3.5 'Photoionization as a probe' (that's this section!):

You describe this well in 4.4 "Photoionization laser"

So I recommend strongly you cut the whole 3.5 section, because it's incomplete, and what you have contains errors.

...

Here are things there are not time to understand:

...

about 1/3 of the MOT atoms are excited, not 'a large fraction'

...

yes, we've intentionally polarized the OP laser to be circular so the atoms will be polarized. We've also picked the S 1/2 to P1/2 so that the fully stretched S1/2  $F = 2$   $m_F = 2$  state is dark. (Unlike the MOT, where we need the P3/2 state so that atoms never go dark, or the force would stop.) But we agreed above not to talk about this.

...

"We have a photodiode(?) to observe what fraction of things get photoionized overall."

...

no, we look at the photoionization rate, for which we have the ion vs. photodiode TOF ('photodiode' looks at when the bursts of light occur)

21

■ \*Have\* we even polarized the photoionization laser in any particular way?

I'm not sure we even have. Why doesn't it absorb? When it's stretched, it's not just all in the excited state! It's...stretched. Do angular momentum selection rules even apply if the electron is going into the continuum? . . .

22

■ Include a picture of the trap. Possibly on both the rMCP and the eMCP.

Also, maybe a thing for the polarization as a function of time? But I don't really need to do that, because it's in the paper. . . . .

22

■ But it's worse for  $A_\beta$  and  $b_{\text{Fierz}}$ , because .... reasons that probably shouldn't

go in this chapter. . . . .

22

■ This also doesn't go in this section. . . . .

22

█ JB: First, I'm pretty sure you should combine Chs. 3 and 4. Ch 4 has good solid general description and details in situ about optical pumping and about the photoionization probe that are, unfortunately, wrong in Ch. 3. You could call the first half of such a chapter "General considerations of Atomic techniques used" and the 2nd half "Experimental Implementation of Atomic Techniques used" . . . . .	23
█ Remember the pulser LED! To evaluate the stability of the scintillator gain!	23
█ JB says: chapter 4.1 (this section) is really good, and in good shape for the committee . . . . .	23
█ We obtain a sample of neutral, cold, nuclear spin-polarized $^{37}\text{K}$ atoms with a known spatial position, via the TRIUMF accelerator facility, by intermittently running a magneto-optical trap (MOT) to confine and cool the atoms, then cycling the trap off to polarize the atoms. With $\beta$ detectors placed opposite each other along the axis of polarization, we are able to directly observe the momenta of $\beta^+$ particles emitted into 1.4% of the total solid angle nearest this axis. We also are able to extract a great deal of information about the momentum of the recoiling $^{37}\text{Ar}$ daughters by measuring their times of flight and hit positions on a microchannel plate detector with a delay line. Because the nuclear polarization is known to within < 0.1% [1], and we are able to account for many systematic effects by periodically reversing the polarization and by collecting unpolarized decay data while the atoms are trapped within the MOT, we expect to be well equipped to implement a test of 'handedness' within the nuclear weak force. . . . .	23
█ Surely most of this paragraph goes in an intro chapter somewhere. . . . .	24
█ Cite a bunch of papers here. . . . .	24
█ Figure was originally created by Alexandre, modified by ... someone else? Or Alexandre? And I got it from ... probably an experimental proposal? I should figure out how to cite a proposal. . . . .	24
█ defined 'MOT' in Ch.3. . . . .	25
█ Mumble mumble UHV. Mumble mumble tail end of the Boltzmann distribution. . . . .	25
█ discussed ... idk, somewhere else. . . . .	25
█ I *do* discuss this, right? Right?? . . . . .	25

█ How to segue here? Do I... Cite myself? Cite Harvey+Murray? Reference the next section? Reference the previous chapter? This is clunky and stupid.	26
█ JB on Chapter 4.2 "The AC-MOT and Polarization Setup" (that's this section!):	
...	
"We use only the polarized portion of the duty cycle in order to minimize other systematic errors, such as the scintillator energy calibration and overall trap position."	
...	
We need to fix this. We use the polarized portion of the cycle because the atoms are almost totally polarized during it, and almost totally unpolarized outside of it. You leave the impression we could measure $b_{\text{Fierz}}$ during the rest of the cycle, during unpolarized time ?	
...	
MJA: Ugh, no, that's really not what I meant. Oh well, time to rephrase.	27
█ Probably document things about the waveform and frequency used for the beamtime, since I don't think it's in my MSc.	27
█ Some of the contents of this section should be removed from here and moved over to Ch-3. Probably.	27
█ have I defined the beta asymmetry yet in some previous chapter? otherwise I have to do it here..	27
█ have I defined $b_{\text{Fierz}}$ yet?	27
█ Plus, it barely makes sense to talk about measuring $b_{\text{Fierz}}$ if you don't know $A_\beta$ .	27
█ I could move this (above) whole paragraph over to Ch.5, where I surely discuss this in more detail. It provides nice context here though.	27
█ Should I move a bunch of this section's content over there, to live in Ch.3?? Should I move a bunch of content from Ch.3 to live here? Current thinking: It seems best to split the content up into a general picture of the MOT (Ch.3), and stuff that's unique to this experiment (here).	27
█ Removed: stuff about how a MOT works. It's in Ch.3. It lives there now.	27
█ Is my photoionization description adequate? ... in light of John's feedback: no.	28

█ JB says: "Since you worked hard on the logic triggers, a photoion spectrum with duty cycle would be appropriate if you want." . . . . .	28
█ Need to describe how polarization works. . . . .	28
█ JB says: all polarization details could be deferred to [1]. (be sure to list all authors including [me]). ) . . . . .	28
█ Note that because the atoms within a MOT can be treated as following a thermal distribution, some fraction of the fastest atoms continuously escape from the trap's potential well. Even with the most carefully-tuned apparatus, the AC-MOT cannot quite match a similar standard MOT in terms of retaining atoms. The TRINAT AC-MOT has a 'trapping half-life' of around 6 seconds, and although that may not be particularly impressive by the standards of other MOTs, it is more than adequate for our purposes. $^{37}\text{K}$ itself has a radioactive half-life of only 1.6 seconds (cite someone), so our dominant loss mechanism is radioactive decay rather than thermal escape. . . . .	28
█ At some point I have to decide if that's going to be a section or a subsection.	29
█ JB says of Chapter 4.3 (this section: first few paragraphs are poorly phrased. You have to fix those. Most of the rest looks really good.) . . . . .	29
█ This section is really disorganized. . . . .	29
█ Possibly needs more diagrams? Possibly just needs more words. . . . .	29
█ ... (shown in Figure 4.4) . . . . .	31
█ There's gotta be a better way to describe it . . . . .	31
█ what's the open area of the detector? how big is each pixel? . . . . .	31
█ Wait. Am I repeating myself? . . . . .	32
█ Some of this stuff is in Ch.3 (see Section 3.5) and/or Ch.6 (see Section 6.1). Some of it *should* be. . . . .	32
█ Chapter 4.4 (this section!) is very good and complete, showing you understand what is needed about photoionization. A good reason to omit 3.5 'photoionization as a probe' as I said above. . . . .	32
█ Probably worth mentioning that we test this stuff offline on stable $^{41}\text{K}$ . . . . .	33
█ JB: "you could reference the letter for the value of the field 150V/cm." . . . . .	33

■ As a check: the camera measurements for photons from de-excitation. It's aimed 35 degrees from vertical, with its horizontal axis the same as ..... one of the other axes. I think it's the TOF axis. I can check this when my computer comes back. Also, there's an unknown additional delay between some of our DAQ channels that can't be explained by accounting for cable lengths, so we really like having the check there. . . . .	33
■ JB says: "yes, camera x-axis is tof axis." . . . . .	33
■ JB: "I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now." . . . . .	36
■ Possibly this can be combined with the "Background and Motivation" or "Theory" chapters? Why do I even *have* two of those chapters, if not for this? Anyway, surely I don't need *three* of them. . . . .	36
■ The point is, the presence of either scalar or tensor interactions will produce a $b_{\text{Fierz}}$ term in the decay PDF. It has other effects on the PDF, but those come in at higher-order in the tiny scalar and tensor couplings. So, the Fierz term would be by far the biggest thing that changes in the PDF. The PDF describes the energy and momentum of the outgoing beta w.r.t. a variety of other things. Notably, we can write an elegant-ish description of beta momentum w.r.t. nuclear polarization direction, and ignore the neutrino completely after integrating over it. We have a PDF in beta <i>direction</i> (w.r.t. polarization), and beta <i>energy</i> . To lowest order (and lowest order is best order) the distribution w.r.t. polarization direction doesn't change, but the distribution w.r.t. energy does change. Or ... something? The point is, it makes a change in the beta energy spectrum. This change is most pronounced at low energies, because the Fierz term is scaled by $(1/E_\beta)$ . However, the asymmetry is also a function of $E_\beta$ . A different function of $E_\beta$ . In fact, it is scaled by $(p_\beta/E_\beta)$ within the PDF, which is distinctly different than $b_{\text{Fierz}}$ . So, one might ask what effect a $b_{\text{Fierz}}$ term would produce on a constructed asymmetry spectrum. ....This explanation has gone way off track. . . . .	36
■ JB: You need to at some point say that the supersum is the beta energy spectrum. There are experiments trying to do this method better, but they are very difficult. UCNA published a combined energy spectrum and Abeta[Ebeta] analysis on the neutron in March 2020 [6]. . . . .	37

█ I can't help but also notice the follow-up article from September 2020 [7]. Ugh. . . . .	37
Figure: Show individual beta energy spectra. ...with a variety of different cuts, perhaps? . . . . .	37
Figure: Show simulated spectra separated by scattering category. . . . .	37
Figure: Show SimpleMC spectra, show the supersum, show the superratio, show the superratio asymmetry. Maybe do some simple fits to show how much better the superratio asymmetry is than <i>not</i> the superratio asymmetry.	37
█ I have some tables summarizing which types of data were measured. Presently, that's in Ch-4, but probably I should move it here. . . . .	40
█ Content that previously was in this subsection is now in Section 4.4. But also, see Section 3.5. Probably in this section here, I need to just jump right in assuming everyone understands the physical principle of how the data works. Then, I can talk about what the data <i>*is*</i> , and how it gets interpreted. . . . .	40
█ Trap position – Measured using the same dataset that was used to quan- tify the polarization. The trap drifts slightly over the course of our data collection. Describe the rMCP calibration needed to extract this info. . .	40
█ Polarization measurement was conducted on a different set of data, collected in between the measurements used for $A_\beta$ and $b_{\text{Fierz}}$ , and at a higher electric field, because we were unable to run both our MCP detectors simultane- ously. . . . .	40
Figure: Need pictures of the cloud. Possibly need projections of the cloud as a function of time, for AC/OP cycles. . . . .	40
█ Also, we noticed the trap drifting after one of the runs, because one of the batteries on one of the thingies adjusting the laser frequency (I think) was failing. . . . .	40
█ Sig figs here need work. . . . .	41
█ JB: "If we rejected the data with the MOT moving (indeed a battery deter- mining the voltage controlled oscillator frequency offset between absorp- tion in stable $^{41}\text{K}$ cell and the $^{37}\text{K}$ resonance) then that's all you need to say." . . . . .	41
█ describe how you'd turn this into a physical description of the cloud, with like a temperature and a sail velocity and shit. with equations. . . . .	41

█ JB: eMCP. You need to describe the timing information obtained. You also need a statement of whether or not you used the position information in your cuts. . . . .	41
Figure: Needs an SOE timing spectrum. At least one of them. Experimental and simulated. Also, I have to describe how I did the simulating, and how I check that it's OK despite the fact that the simulated spectrum looks nothing like the experimental spectrum. . . . .	41
█ JB says: "ch 7 looks fine to me." ...even in spite of the giant paragraph suggestions below, I guess. . . . .	46
█ Do I actually want to combine this with Chapter 6? . . . . .	46
█ from JB: you ask at the start if you should combine it (Ch.7, 'Analysis') with Ch.6 ('Calibrations'). I see no special reason either way. I would say the topics seem distinct. . . . .	46

JB: Here is what I suggest for the intro the Analysis section: (n.b.: at the time of this comment, that was Ch.7)

• • •

7.0 Analysis is critical to a precision measurement, as most of the research is in determining systematic uncertainties by self-consistent analysis and simulations. Each detector in this experiment is critical and has independent calibrations and cuts. Full explanation is here (and in two following chapters) justifying choice of the deterministic cuts, because in the analysis in this thesis the data was not blinded. The main goal of blinding was nevertheless achieved— to make sure all analysis is done completely with full redundancy of checks wherever possible— so the discipline entailed must be described in full detail. Here there are details of detectors: eMCP rMCP beta DSSD scintillator.

• • •

The collaboration has done an independent analysis fixing bFierz to zero. Differences with that analysis are interleaved in this section. Critical physics improvements concern an emcp-beta timing walk correction which enabled an improved cut against background, also incorporating a more complete modelling of decay backgrounds from untrapped atoms. Technical corrections include a correct treatment of the polarization cycle. An arbitrary change in the deltaE radius cut is kept self-consistent.

• • •

Then Section 7.1... . . . . .	46
■ Also, the background was too noisy in Set A I think. Also, pretty sure there isn't really that much data in Set A at all. Also-also, Set A has $E=66.7$ , so can't use it for more stats on the other backgrounds. . . . .	47
■ MUST check to make sure I didn't use Run 420 in "good runs" in the end!!!	48
■ I have more (really early) runs classed in Set A than Ben had. In the end, we didn't use them so it doesn't matter. But...what? . . . . .	48
■ Ugh. My categorization system is slightly different than Ben's on the later recoils. That's annoying. . . . .	48
■ Other things I could list here: Electric field strengths, total runtime. . . . .	48
■ also discussed: how we decide what counts as an eMCP hit at all . . . . .	48
■ Awkward stupid phrasing. . . . .	48

█ Also, we claim that it doesn't bias the data. Much. Didn't I try to evaluate how much it biased the data at one point? . . . . .	48
█ change by 0.2% of its value vs change by 0.5% of its value, according to Ben's thesis pg 143. . . . .	49
█ Pretty sure I'm repeating myself from Chapter ?? . . . . .	49
█ JB says: To repeat a comment, since the Appendix on analysis changes is being dispersed throughout, when you state somewhere (I think it's in Ch. 6) make sure you state clearly that the only change for BB1 cuts is the radius cut, e.g. that you took the same T and E from the waveforms (I'm not even sure whether the waveforms are recorded anyway). You ask 'how can I state this' but there's no reason to be subtle. Just say upfront that Ben and Spencer's theses did all the groundwork on the BB1, and here you include selected details needed to understand the present analysis. If you need to include some redundant material, don't worry too much about that. . . . .	49
█ How do I *say* that Ben was the one who did most of the DSSD calibration stuff? I maybe don't need to describe all of it here, but I *could*, and maybe it's needed in order to understand like 4 rows in my error budget.	50
█ JB: "You can describe anything you did differently or improved, but you can and should otherwise defer all details of the scintillator calibration and DSSD calibration to Ben's paper and his thesis and Spencer's. E.g. Section 9.2 "statistical agreement between BB1 X and Y detectors' energies only makes a small effect on results" does not need the technical details beyond that statement." . . . . .	50
█ JB: "If you have some way of documenting the coding you used, that would be great." ... yeah, it would, wouldn't it? . . . . .	51
█ I think Ben might have selected 60 keV? That's maybe something for the appendix. . . . .	51
█ Did I even mention the collimator? Like, in the previous chapter or something..? . . . . .	51
█ I described the HEX-75 somewhere in a previous chapter, right?? . . . . .	51
█ Do I need to describe MCPs and delay lines somewhere? Maybe not... . . . .	51
█ Um. Did I for sure get the labels correct on this??? It seems really wrong.	53
█ This goes in that one appendix, if I haven't already put it there. . . . .	54

█	Okay. This needs more description. . . . .	54
█	JB: "I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now." . . . . .	58
█	Do I want this in a separate chapter? Or maybe I want it to be combined with the stuff on data selection and analysis. Surely that chapter is getting too big even without this stuff. . . . .	58
█	JB: "I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now.  I've tried to email you the paragraphs on "collaboration determination of uncertainties" for Ch. 9.  My intent of all that other advice was to keep your time spent on Chs 1-4 concise, so you could concentrate on these real jobs. (n.b.: the advice he's already sent was almost all about chapters 1-4, which are the various intros/background info and experimental setup stuff)  ...  I can only say that if you have an equal choice between including a detail or not, pick "not." "	60
█	JB: I will try to schedule meeting with Dan for you to show us the final version of Ch. 9 Estimating systematic effects soon. . . . .	60
█	Do I want this chapter combined with the analysis chapter? If that chapter includes all the stuff about G4, it's going to be unwieldy and huge. . . .	60

JB says:

A simple estimate from the collaboration that builds intuition for this result: Scatter in the SiC mirrors and DSSD actually produces an efficiency change at low beta energy. Energy loss is not minimally ionizing in these structures, and instead will have a long Landau tail that can take events below energy threshold in the scintillator. The collaboration has modelled explicitly the false asymmetry as a function of Kbeta between 600 and 1300 keV, producing roughly  $(K-0.6 \text{ MeV})/(0.7 \text{ MeV})$ , i.e. 50% at  $K\beta=0.95 \text{ MeV}$ . This efficiency degradation would be distributed roughly equally between the SiC and DSSD. If completely ignored, this would introduce by inspection a false bFierz of approximately 0.5. Scattering effects will vary between linear and  $\sqrt{\text{thickness}}$ , so assuming worst case of linear, the mechanical thickness uncertainty of 5 micron/300 micron and 6 micron/275 micron, an average of 2%, making a random contribution of order 0.01 each. The Be window has larger mechanical thickness uncertainty of 23micron/229 micron, but energy loss and scattering in this material is 5x smaller, so the net effect would be similar.

• • •

To minimize this systematic for future experiments, the collaboration has implemented pellicle mirrors of negligible thickness, 100 nm Au on 4 micron kapton. The collaboration is also implementing Be-windowed wire chambers in place of the DSSD.

•

It's actually not nearly as big as I'd originally expected. It's huge in the lineshape thing, but pretty tiny in everything else. . . . . . . . . . .

from John: "I used Ben's threshold when determining the uncertainty from the lineshape tail (UFTLT). If you're saying the UFTLT depends on the threshold used, ok, of course it does. But if you're claiming that UFTLT depends on the \*\*uncertainty\*\* of the threshold, that's manifestly smaller than the UFTLT itself, and I'm going to assert it isn't worth evaluating."

■ JB: I hope the discussion is clear in your head. Any effect that relies on scattering computation in G4 should have an uncertainty on order 10% of the correction – hopefully you are keeping a distinction here between the finite geometry acceptance (which I guess is exact) scattering off the collimator. . . . .	61
■ As per JB's comment in section 7.2: "statistical agreement between BB1 X and Y detectors' energies only makes a small effect on results" does not need the technical details beyond that statement." . . . . .	62
Figure: Surely this requires at *least* one image of the pixelated BB1 data. Maybe some of a few waveforms and energy distributions too. ....Feels like cheating to include some of that stuff, since Ben was the one who actually used it mostly. . . . .	62
■ JB on missing figure: "if you used such an image as part of your uncertainty estimate, yes [include it]" . . . . .	62
■ Remember: There's noise applied to simulated BB1s, matching some spectrum.	62
■ JB: The simulations of course include it event-by-event, not just a minimally ionizing average loss. . . . .	63
■ JB on figures that might go here: Figure 6.4 (currently that picture of the TOF spectrum) could either be here, or you could reference it from here. The TOF histogram is a great start. Adding the asymmetry[TOF] indeed would be vital. . . . .	63
Figure: Show the "average asymmetry" (all energies) as a function of TOF, with real data, best model normalization, and extrema of model normalizations. Show our cut. Turns out, it's a lot of work for a really tiny correction. Oh well. . . . .	63
■ JB on the *actual* figure I had been planning to put here, and my remarks about it: Indeed it will be critical to show a clear compelling version of this figure in thesis and in a paper. It was vital to minimize and determine this background to avoid fitting a polynomial to it from the wings, even more so for the energy dependence of A than for its average – you should say so. ...	63
The reason the correction is small is because of all your hard work. . . . .	63

■ JB: "I wouldn't call these "scattered" events... that's very misleading."

...

Yeah, I should really stop doing that.

...

Wait no, scattered is what I mean! But fine, my phrasing is really unclear. 63

■ JB: Please comment on whether or not it was important to have this energy distribution. . . . . 63

■ Oh god. Have I even *tried* to quantify the combined systematic that comes out of the TOF cut? Do I need to, or is it double-counting? Ugh, it would be such a headache to do this. Maybe I can at least do it at the end – because I might never get my code back to the way it was. . . . . 64

■ JB says: Please discuss this at the next meeting. (ETA: Done!)

Indeed this is why you should avoid calling the events originating not from the trap 'scattered events.' More importantly, why it was so critical that you reduced the size of the correction by timing bad events out. I would say you have a well-determined TOF cut to minimize this error– a cut that could not have been done blind without an unreasonably perfect simulation. Thus the exact spot of the cut should not be considered to introduce a systematic. . . . . 64

■ This section should reference Clifford. [8]. . . . . 64

■ JB: so it's still critical to write down more of the lineshape work. . . . . 66

■ JB: yes, brems strahlung is 'braking radiation' so gets 2 ss's. the lineshape tail in any scintillator also includes backscattered events – we are not claiming the 2-pixel cut is complete . . . . . 66

■ JB: "I will write this up better soon." (I think he already did that) . . . . . 67

■ Here is Subsection 9.5.5 "The low-energy tail uncertainty, and what it does" complete. There should be no figure. You cite the Clifford paper now in other sections– please check if my [] is right. (I cc: everyone as my summary of the situation that I showed Dec 1, 2021 at trinat.triumf.ca) 67



\*\*\*\*\*

This subsection has the collaboration’s evaluation of the uncertainty from the scintillator detector’s lineshape tail. The energy from a monoenergetic beta is not always fully absorbed in a plastic scintillator. Although most backscattered betas are vetoed by the DSSD, some produce bremsstrahlung photons, and these frequently escape low-Z plastic scintillator— all cross-sections are known to high accuracy, but there is always uncertainty entailed in the MC implementation. This lineshape tail will then effectively move events from higher to lower measured energy, artificially altering the lower-energy asymmetries and mimicking the effects of a Fierz term.

...

Since this detector effect is difficult to disentangle from the other scattering effects off volumes, the collaboration adds a linear function down to zero for the tail to a Gaussian for the peak, with linewidth varying by photon statistics [? ]. The convolution of this simple detector response function with v/c then scales the centroid MC, with the lineshape tail varied by  $\pm 10\%$  of its value, a generic uncertainty accepted by the community for MC electromagnetic simulations. The fit  $b_{\text{Fierz}}$  centroid changes by  $\pm 0.0076$ , summarized as the 0.008 “Low Energy Tail” in the systematics table at the start of this chapter. Compared to other uncertainties of the present data set, this is small enough that the accuracy of this estimate is adequate.

\*\*\*\*\*

67



JB: Dan and I independently discussed Ch 10 yesterday, and he has suggestions to help. So I will also schedule a meeting with Dan and you to discuss Ch 10 Results and whether the S,T part must be deleted and left to a paper. You don’t have enough time, and although this should be quite straightforward, it is not your critical result and it’s the only thing that can go. . . . .

69

JB says: To put your work in context, please add at the end of that minimal S,T section, or at the end of "Our Decay" section

• • •

The best existing measurement of  $b_{\text{Fierz}}$  is in the decay of the neutron [9],  $b_{\text{Fierz}} = 0.017 \pm 0.021$ , consistent with the Standard Model prediction of zero. Our measurement is strongly related, yet complementary. In terms on non-Standard Model Lorentz current structures, to lowest order in the non-SM currents the same equation applies:

$$b_{\text{Fierz}} = \pm (C_S + C'_S + (C_T - C'_T)\lambda^2)/(1 + \lambda^2)$$

(the plus is for  $\beta^-$  decay and the - for  $\beta^+$  decay) [10]. [to be continued...] 69

[...continued from prev.]

In our  $^{37}\text{K}$  case,  $\lambda^2 = |M_{\text{GT}}|^2/|M_F|^2$  is close to  $3/5$  (the expected value  $j/(j+1)$  for a single  $j=3/2$  d $3/2$  nucleon) [11], while for the neutron  $\lambda^2$  is close to  $3$  (the expected value for an  $(j+1)/j$   $j=1/2$  s $1/2$  nucleon).  $|M_F|$ , the Fermi matrix element, is nearly the same for both of these isospin =  $1/2$  decays (the largest correction is the larger isospin mixing of  $\sim 0.01$  in  $^{37}\text{K}$ ). So our observable is relatively less sensitive to Lorentz tensor currents, and will predominantly constrain or discover Lorentz scalar currents.

• • •

Full considerations would require a weighted fit of  $b_{\text{Fierz}}$  experiments and similar observables [12], and are beyond the scope of this thesis. The info from this thesis, values of  $A_\beta$  and  $b_{\text{Fierz}}$  with their uncertainties, can together with the known  $fT$  value (lifetime and branching ratio) allow the community and/or the collaboration to include the results in a future constraint or discovery of scalar and tensor Lorentz currents contributing to  $\beta$  decay.

■ JB says: Appendix A (ie, *this appendix*) is very important, and should at least be a subsection in the Analysis chapter.	
...	
You could condense the Appendix into a set of bullet points at the end of the intro to the Analysis section (which you still need, badly!), and then its content could be interleaved in the Analysis chapter. E.g. you already have redundancy in the LE and TE discussion vs. the Appendix, and the discussion is more complete in the Analysis chapter, which is good. . . .	77
■ Somebody will surely ask for a justification for why I did this differently, and I don't have one beyond "this seemed more reasonable to me", which is of course nobody will ever accept as a reason. . . . .	78
■ John says to keep this appendix, because it's great now. . . . .	79
■ JB: Appendix B is in good shape. "someone other than me" please change to "the collaboration." . . . . .	79
■ We have already specialized to $\beta^+$ decay. . . . .	79
■ Also, $\xi = G_v^2 \cos \theta_C f_1(E)$ . . . . .	81
■ There was something wrong with this assumption. Something circular. I forgot. Blah. . . . .	84
■ Or will I? . . . . .	84
■ Somewhere I have to define $q^2$ and $\Delta$ are. . . . .	84
■ Somewhere, just list out the goddamn values of things that I inherited from Ian Towner's personal communication that one time, over multiple generations of grad students. . . . .	84
■ and also, I think something like that the weak charge is the same distribution as the electric charge . . . . .	85
■ What is less clear, given the context in the paper, is whether or not when Holstein writes out his simplified expressions for $\Delta F_x(E_\beta, u, v, s)$ he actually means $F_{\mp}(Z, E_\beta)\Delta F_i(E_\beta, u, v, s)$ . These terms are pretty small, so it probably doesn't *really* matter, but it would still be really nice to *know*, damn it. . . . .	85
■ Also, pretty sure one of those never gets used. Which one was it? idk. . . .	86
■ Note: It's not the case that $ \vec{J}  == J$ . It's actually super fucking infuriating notation. . . . .	87

█ JB on how to triage Appendix C: keep after:	
C.1: keep 1st two sentences, discard next few.	
C.2 Remove subsection title, replace by one sentence 'This section includes handwritten notes.' without further apology. We can't pay you anymore to make nice Latex. If the committee or Gerald objects to complex handwritten notes, App. C -> internal. . . . .	88
█ JB: Appendix C has some redundancies with B. You will have to sort that out. (n.b.: from context, it's less clear which appendices he's actually talking about, but whatever, there's certainly redundancies all around. . . . .	88
█ Must find a better way to smush this table with display style math font type-setting. At present, it's somehow both too smooshed and not smooshed enough. . . . .	88
█ Appendix D -> internal document. . . . .	98
█ JB: Appendix C has some redundancies with B. You will have to sort that out. The page-long shaggy dog story of f7 is distracting and needs to be truncated to the insight gained: "f7 is a recoil order term, and there are no recoil order terms in JTW." There may be many more things to do of that sort.	
...	
MJA: There's fuck all mention of f7 in Appendix C at the time of this comment, so he probably meant Appendix D. Probably there are redundancies everywhere though. . . . .	98
█ JB: Keep Appendix E iff you can clean it up in one (1) hour. Otherwise -> internal. . . . .	103
█ In fact,	
$\xi = G_v^2 \cos \theta_C f_1(E)$	(0.1)
...	103
█ JB says: Appendix F -> internal . . . . .	107

- According to insight that Alexandre thought was very obvious, because it was, JTW-style notation only happened in the first place because the lab frame matters. it's measured w.r.t. polarization or alignment or something. The integration over the two leptons is really an integration over the lab frame + one lepton. . . . . 107
- See "jtw\_integration\_scratch3.nb" for more-other details. Of things. Things I've deleted from the document now. ...That version of the mathematica notebook, I believe, has finally got all the thingies right. . . . . 108
- Is that ↑ even true?? Because I'm really not sure it is. Via Kofoedhansen,  $(E_0 - E_e) = E_\nu$ . So there. . . . . 108
- A thing that's worth noting is that (I think!) recoil-order corrections have been implicitly excluded at some point here. ...Is this even true?? . . . . . 108
- JB says: Appendix G -> internal (needed for understanding for later ST analysis). . . . . 110
- JB: You're describing a consequence of CVC, and are not stating the actual hypothesis. . . . . 112
- JB: I think you don't have time to explain the CBC hypothesis. You'll just have to assume it. I personally found that the technical derivation was the only way to see what was going on. . . . . 112
- JB: A good sketch of CVC is done in Commins' notes and his book with Bucksbaum [? ]. Ian towner did it in his notes. I reproduce this in my own course notes for Phys 505 (p.21-28 of the thing John attached to that email). First you show in Dirac notation that the E&M current for pointlike particles is conserved—this just needs one use of the Dirac Equation and conservation of electric charge. Then look what happens for finite nucleons. Then construct the analogous weak interaction current for the nucleon, including three possible current terms that transform like Lorentz vectors, and note the consequences if that current is still conserved. 112
- Figure out how to cite somebody's unpublished/semi-unpublished notes. . . 112



■ JB: Something else beyond the thesis scope, sorry: a nonzero $b_{\text{Fierz}}$ does not necessarily break CVC, as the vector part of the SM weak interaction could still be conserved whether or not there are other quark-lepton currents with different Lorentz structure. You could make a nonzero $b_{\text{Fierz}}$ from a 2nd-class scalar in the nucleon-electron weak current which would break CVC, but that could not be distinguished from a quark-electron extra scalar current. . . . .	113
■ JB says: Appendix H -> internal (critical for later ST analysis, but only then)	114
■ John and Dan say this paper is widely accepted to be wrong about some of the things. (Some assumption was wrong, I guess?) So then presumably what I've written here can't be trusted either. . . . .	114
■ JB: The paper that fixes the known mistakes in Severijns et al. [14] is L. Hayen and N. Severijns, 2019 [15]. It's conveniently located on the arXiv, and I should definitely go read it. The known mistake was using the ratio of integrals of the lepton momentum $f_A/f_V$ more than once— there is a more subtle radiative correction for the Gamow-Teller piece. A paper using the formalism is D. Combs et al [16] (which is <i>also</i> conveniently located on the arXiv) with $^{19}\text{Ne}$ and $^{37}\text{K}$ results— their extraction is so close to Ben and Dan's that we conclude we are doing the formalism well. i.e. we used the $f_A/f_V$ ratio correctly. . . . .	114
■ Appendix I keep, it's excellent. It should be moved as is to Conclusions under "Future Experiment for the collaboration"! so people know you worked so hard on it!! . . . . .	118
■ JB: Appendix J -> internal . . . . .	122
■ JB: "You don't need anything about the atomic hyperfine structure in this thesis."	

...

Me: yeah, that's fair. I probably should cut this whole section actually. .	122
■ Appendix KLM you have to pick what you want— I hope that's Appendix K— and remove the rest as you say they're "old". Appendix K could be moved to the end of Experimental Methods because it's absolutely critical and helpful!! but if you want to reference it there and leave it as an Appendix, it's up to you. . . . .	124

■ We could also use this with the Holstein formulation, at least some of it.	
The point is, we can put *anything* that only depends on beta energy into $W(E_\beta)$ . It doesn't matter, because it's already only integrable through numerical methods anyway – so we can't possibly make it worse. . . . .	125
■ Not quite true. Some strips are missing. . . . .	125
■ This is only true if we neglect (back-)scatter. This is not actually a good approximation. But we have pretty good simulations to give us the real numbers, anyway. . . . .	125
■ Is that definitely true, or is it only true to lowest order? . . . . .	126
■ See content at Appendix (K). After reading that stuff, continue here. . . .	127
■ The trap is *not* centered, but the polarizations in the two states are equal, to a very high level of precision. More than we need for $b_{\text{Fierz}}$ anyway, and probably more than we'd need for $A_\beta$ . . . . .	127

# Chapter 1

## Introduction

JB on the contents of Chapter 1:

Move what you have in 1.1 and 1.3 to the first section of Chapter 2, and otherwise omit Chapter 1.

more from JB on the contents of Chapter 1:

With three changes:

...

1)"and we shall be interested especially in scalar (S) and tensor (T) couplings." -> "our observable is mostly sensitive to scalar (S) and tensor (T) couplings."

...

2) "These couplings all refer to parameters in a Lagrangian that takes the relativistic inner product of a current for the lepton with a current for the proton or neutron. The resulting Lagrangian must be a scalar under Lorentz transformations, so these currents must have transformations like these V,A,S, and T and be combined into a scalar."

...

3) Add one reference to the latest review:

Adam Falkowski, Martín González-Alonso, Oscar Naviliat-Cuncic. Comprehensive analysis of beta decays within and beyond the Standard Model. Journal of High Energy Physics, Springer, 2021, 04, pp.126. 10.1007/JHEP04(2021)126.

...

You have time for nothing else.

### 1.1 Background and Motivation

The nuclear weak force is one of four fundamental forces described within physics. It mediates the process of beta decay, which is of particular interest to us here.

Although beta decay is generally well understood, it presents a unique opportunity to search for physics beyond the Standard Model within the behaviour of the weak coupling. By observing the kinematics and angular correlations involved in the decay process, one gains access to a wealth of information about the precise form of the operators mediating the decay.

According to the predictions of the Standard Model, beta decay involves only so-called vector (V) and axial-vector (A) couplings, with a relative phase angle producing the left-handed “(V–A)” form of the interaction. There exists an extensive body of experimental evidence to demonstrate that this is overall a very good description of the beta decay process. Despite the success of the (V–A) model, the additional presence of certain other “exotic” couplings cannot be entirely ruled out, and we shall be interested especially in scalar (S) and tensor (T) couplings.

According to present limits, these couplings would have to be pretty small relative to the (V) and (A) couplings.

Need to figure out how the exotic couplings actually work, mathematically. What the fuck does “(V – A)” even \*mean\*? IIRC John wants a brief mention of  $\gamma_5$ 's and  $\gamma_\mu$ 's, and probably a brief mention of whatever mumble-mumble group is mumble-mumble represented or something.

...

JB says:

the current transforms like a Lorentz scalar or tensor – this does not refer to the angular momentum.

If you write down the Lagrangian for beta decay, that's eough. All these things refer to the structure of the Lagrangian. The theory considers all possible Lorentz transformations of the currents.

Please don't talk about  $SU(2)\times U(1)$  for electroweak unification. It's textbook material that's beyond the scope.

Did I even get this right? Is the phase angle really what makes it left-handed?  
JB says:

...  
Relative sign. look at the quark-lepton Lagrangian, which has  $(1 \pm \gamma_5)$

Cite a bunch of people here. (Who?) Might be nice to have a picture also.

JB on intuitive concepts that are missing:

The SM couples to left-handed neutrinos and right-handed antineutrinos. Since the neutrinos only have weak interactions, there are no right-handed nu's nor left-handed antinu's in nature. The neutrino asymmetry  $B_\nu$  is a number with no energy dependence.

Similarly, the SM weak interaction only couples to right-handed positrons and left-handed electrons. Since these are massive particles, the average helicity of positrons is not 1, but instead  $v/c$ . One can always boost to a frame where the positron keeps its circulation but is moving in the opposite direction. This is why the beta asymmetry is  $A v/c$ , not just  $A$ .

The Fierz term's additional energy dependence of  $m/E$  also comes from helicity arguments, stemming from the fact that it still is coupling to SM nu's and antinu's only, so the beta's are generated with wrong handedness.

The details are built at 4th-year undergrad level in Garcia's paper with his student and post-doc [3].

The beta asymmetry dependence on the Fierz term only comes through the normalization of  $W(\theta) = 1 + b_{\text{Fierz}} m/E + A_\beta \cos(\theta)$ .

i.e.:

$W'(\theta) = 1 + A_\beta / (1 + b_{\text{Fierz}} m/E) \cos(\theta)$ . (the angular distribution must be unity where  $\cos(\theta)$  vanishes, by definition).

## 1.2 Exotic Couplings

## 1.3 Fierz Interference – The Physical Signature

The physical effects resulting from the presence of scalar or tensor couplings include a small perturbation to the energy spectrum of betas produced by radioactive decay.

Missing figure

I need that simulated picture of the different beta energy spectra, with different values of  $b_{\text{Fierz}}$ .

JB on that missing figure: "A dependence of  $A_{\beta}$  on beta energy is also introduced.

UCNA fits energy spectrum and  $A_{\beta}[\text{E}_{\beta}]$  simultaneously now."

## 1.4 Present Limits

A bit about other people's physics.

## 1.5 A Toy Experiment

A quick overview of how an experiment like this one would be set up to extract the physics of interest, to keep the reader from getting too lost in the rest of the thesis.

Do I really even \*want\* to include a toy experiment? And would I want to do it here?? What even is the point? I think in the past I decided it was easier to build up a description of .... something .... starting this way. But why?? Possibly as I continue to add content, it will become obvious again why I originally wanted to do this.

JB says: "Your experiment is simple enough to describe without a toy." (He's said this twice now, so I should probably just kill this section.)

# Chapter 2

## Nuclear Physics and Beta Decay Overview

JB says Chapter 1 sections 1.1 and 1.3 should get moved here into Chapter 2.

JB on what (else) should change in Chapter 2:

...

cut "so we will simply provide the combined master equation here"

Don't. The equation you have is all you need.

### 2.1 The Basics of Beta Decay

Standard Model beta decay is well understood. The Fermi description of beta decay can be found in any nuclear physics textbook, but you have to dig slightly harder to understand Gamow-Teller or mixed decays, all of which are relevant here.

via Krane [17] Under the Allowed Approximation, we require that a beta decay may not carry away any orbital angular momentum, because we treat the nucleus as pointlike and work in the CM frame. An Allowed decay can, however, change the total nuclear angular momentum, because the outgoing leptons have spin= 1/2 and therefore carry angular momentum. Therefore, in an allowed decay, the total nuclear angular momentum must always change by either 0 or 1.

Is this even true? The pointlike thing? ...No.  
No it's not.

JB says: The title of Holstein's review addresses this "pointlike" issue, and he describes the "impulse approximation" in Section V. The interaction is not pointlike, because all constants are a form factor expansion in  $q^2$  – finite size terms contribute to the Coulomb correction.

From a 2006 paper by Severijns et al [18], the selection rules for an allowed

transition are:

$$\Delta I = I_f - I_i = \{0, \pm 1\} \quad (2.1)$$

$$\hat{\Pi}_i \hat{\Pi}_f = +1 \quad (2.2)$$

Then, you can separate the allowed transitions into singlet (anti-parallel lepton spins,  $S = 0$  – a Fermi transition) and triplet states (parallel lepton spins,  $S = 1$  – a Gamow-Teller transition).

Fermi decays are so-called “vector” interactions, and happen when the spin of the two leptons involved are antiparallel, so there can be no change in angular momentum (at least in the case of the Allowed approximation).

Gamow-Teller decays involve two leptons with parallel spins, so the decay must change the projection of the nuclear angular momentum,  $M_I$ , by exactly one unit (in the case of the Allowed approximation). They transition may or may not simultaneously change the total nuclear spin,  $I$ , by one unit. These are “axial-vector” interactions. (Note that  $I = 0 \rightarrow I = 0$  interactions are never Gamow-Teller decays.

Probably everything in this section is yoinked from [19], pg 212.

## 2.2 Mathematical Formalism

In order to proceed with a measurement, we must find a master equation to describe the probability of beta decay events with any given distribution of energy and momenta among the daughter particles, as a function of the strength of the specific couplings of interest to us. To do this, two sets of formalisms are combined – the older formalism from Jackson, Treiman, and Wylde (JTW) [10], [20], which describes the effects of all types of Standard Model and exotic couplings of interest to us here, but which truncates its expression at first order in the (small) parameter of recoil energy, and a newer formalism from Holstein [21], which includes terms up to several orders higher in recoil energy, but which does not include any description of the exotic couplings of particular interest to us. We note that because any exotic couplings present in nature have already been determined to be either small or nonexistent, it is sufficient to describe these parameters with expressions truncated at first order, despite the fact that it is still necessary to describe the larger Standard Model couplings with

higher-order terms.

The procedure for combining the two formalisms is described in detail in Appendix B, so we will simply provide the combined master equation here:

In the mean time, here's what happens when we integrate JTW over neutrino direction:

$$\begin{aligned} d^3\Gamma dE_\beta d^3\hat{\Omega}_\beta &= \frac{2}{(2\pi)^4} F_{\mp}(Z, E_\beta) p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d^3\hat{\Omega}_\beta \xi \\ &\times \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \left( \frac{\vec{J}}{J} \cdot \frac{\vec{p}_\beta}{E_\beta} \right) \right], \end{aligned} \quad (2.3)$$

where

$$\xi = G_v^2 \cos \theta_C f_1(E). \quad (2.4)$$

## 2.3 Our Decay

JB: 2.3 Our decay: Just put the comments in. Keep the figure as-is.

MJA: Pretty sure the comments are literally copy-pasted from somewhere I shouldn't just plagiarize from. Need to rephrase it at least.

“Here, we focus on the decay  $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$ . The angular correlations between the emerging daughter particles provide a rich source of information about the type of interaction that produced the decay.”

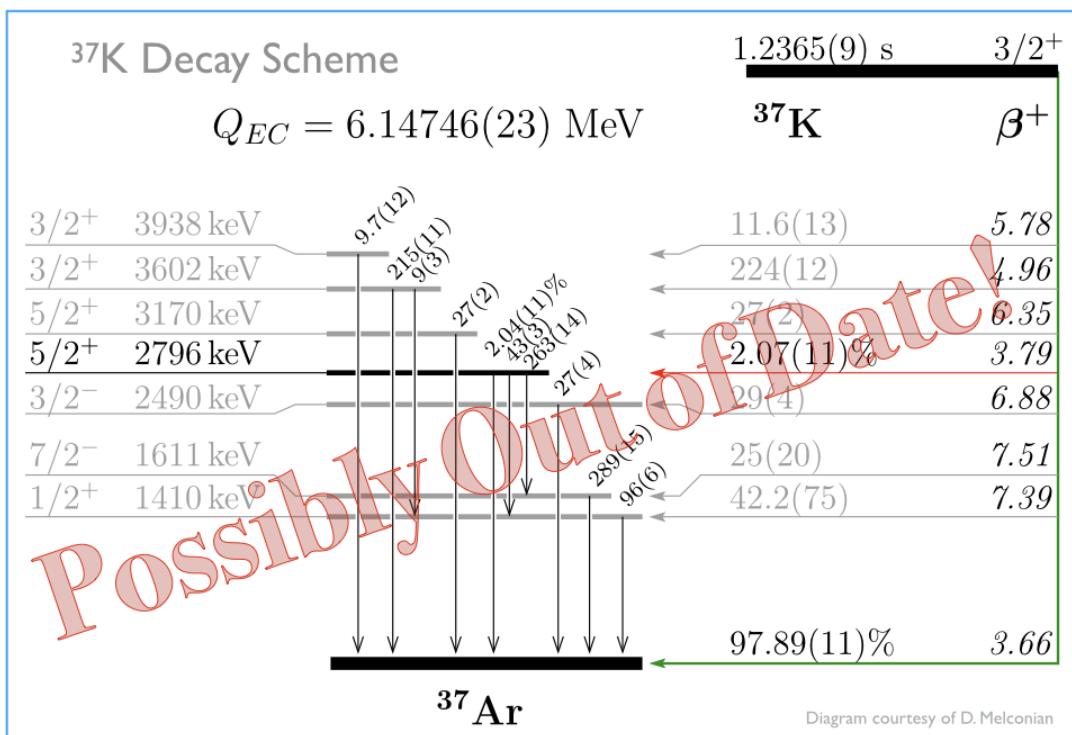
“Of particular interest is the decay process:  $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$ . Among other useful properties, this is a ‘mirror’ decay, meaning that the nuclear wavefunctions of the parent and daughter are identical up to their isospin quantum number. This property allows us to place strong constraints on the size of the theoretical uncertainties for this decay process within the Standard Model.”

Talk about how great  $^{37}\text{K}$  is for what we're doing with it. Also, drop all the math-numbers to support those assertions.

Do it! Do the master equation!

Missing  
figure

This thing is going to need a nuclear level diagram for  $^{37}\text{K}$ . Also,  $^{37}\text{K}$  is a really nice isotope for this, because  $98\% + 2\%$ , also because it's a mirror decay, also because it's an alkali. Also-also, its big  $A_\beta$  value means we have a big thing to multiply any  $b_{\text{Fierz}}$  value there might be when we construct the superratio asymmetry to eliminate systematics.



2

Figure 2.1: A level diagram for the decay of  $^{37}\text{K}$ .

## 2.4 Shake-off Electron Spectrum

JB on 2.4 SOE:

Say what you did, with as little info as possible now. Georg is a theoretical chemist, so he may be very curious about this.

...

"Atomic electrons with kinetic energies 0 to 100 eV are produced as part of the beta decay process. If their energy is below a certain value, our detection process is not perturbed, so we provide physics about the energy spectrum here.

...

Levinger [4], assuming the sudden approximation, calculated the overlap between an electron in the initial atom with an outgoing electron or an electron in the final atom. Levinger approximated everything with hydrogenic wavefunctions, so his calculations become analytic. The collaboration has found that Levinger's formulae fit our measurements of the position and TOF info of our atomic electrons, so they are used in our simulations

OR

but the precise energy spectrum was found to be unimportant, so we used X in our simulations. whichever is true.

...

This is then fine. You have most of the basics down.

Shake-off electrons: where do they come from, and where do they go? [4].

Really, just discuss the physics of what happens to cause SOEs to be a thing. Talk about \*our\* SOE spectrum in some other chapter later. Should this go in the atomic physics chapter? I can't decide whether it's more atomic or more nuclear.

John made some nice plots of these from the eMCP data. I did \*not\* use it to make a cut on eMCP hit position in the end, despite the fact that it makes the spectrum more clean, because a lot of good events don't have full hit position information, and you lose an awful lot of statistics by making the cut. I used this for modeling the background spectrum, but in the end it wasn't as elegant a result as I might have hoped. Also, it's still an open question exactly which fraction of SOEs come from which atomic shell, but it doesn't change the resulting spectrum very much.

Should I talk about the distribution of how many SOEs come off in a decay? I have measurements of the recoil charge distribution, which is related but not really the same thing. From a theoretical POV, I don't know how many get shaken off. Thankfully, it doesn't matter very much in the end.

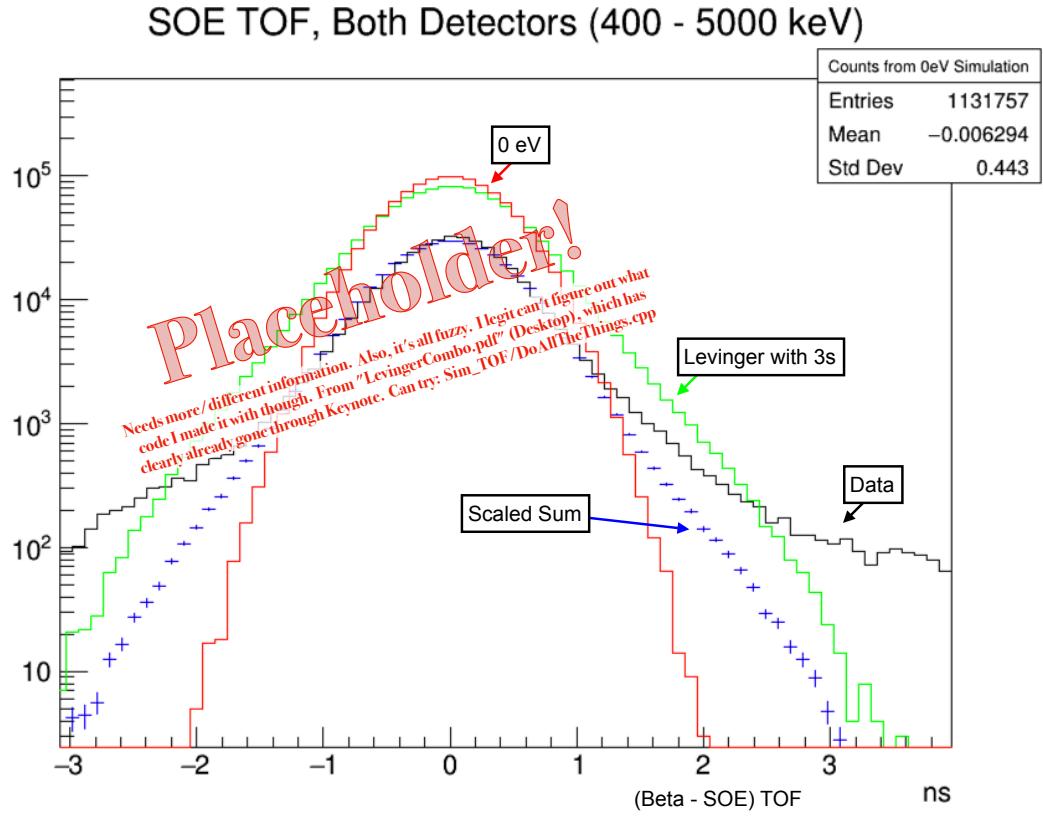


Figure 2.2: Shake-off electron TOF (w.r.t. beta TOA) spectrum, showing how the spectrum is different if one includes different sets of initial electrons to be shaken off. I forget why some of them have 0 eV. Maybe those are the ones from the  $^{37}\text{Ar}^+$ . ... Levinger TOF spectra for some different sets of SOE initial orbitals before shake-off. (At least that's what it's supposed to be, after I fix the picture). It's reconstructed event-by-event with beta times-of-flight that would pass some basic 'good event' cuts. Anyway, it turns out, it doesn't much matter what orbitals you lose SOEs from. That's nice. In the end, I used 85+15. (Need to re-plot this.)

# Chapter 3

## Atomic Physics Overview

First, I'm pretty sure you should combine Chs. 3 and 4. Ch 4 has good solid general description and details in situ about optical pumping and about the photoionization probe that are, unfortunately, wrong in Ch. 3. You could call the first half of such a chapter "General considerations of Atomic techniques used" and the 2nd half "Experimental Implementation of Atomic Techniques used"

wtf could I possibly have gotten wrong in my Ch.3 description? It's so vague!

### 3.1 Magneto-Optical Traps

I need to organize the sections/subsections in this chapter better...

Since its initial description by Raab et. al. in 1987 [22], the magneto-optical trap (MOT) has become a widely used technique in many atomic physics laboratories. The MOT produces confined samples of cold, electrically neutral and isotopically pure atoms confined within a small spatial region. It is these properties that make the MOT a valuable tool not only in atomic physics, but for precision measurements in nuclear physics as well, and the TRINAT lab has adopted the technique wholeheartedly.

The technique is used predominantly with alkalis due to their simple orbital electron structure, and once set up it is quite robust. The MOT's trapping force is specific to the isotope for which the trap has been tuned. This feature makes it ideal for use in precision radioactive decay experiments, since the daughters are unaffected by the trapping forces keeping the parent confined.

Have I defined TRI-NAT yet?

Possibly most of the above paragraph is also written/paraphrased elsewhere. Like in Ch-4, unless I removed it...

A typical MOT can be created from relatively simple components: a quadrupole-shaped magnetic field, typically generated by two current-carrying coils of wire, and a circularly polarized laser tuned to match one or more atomic transitions in the isotope of interest. Because a MOT is easily disrupted by interactions with untrapped atoms, the trap must be created within a vacuum system. Finally, a source of atoms to be trapped is required. [See Fig. 3.2.]

The laser, which must be circularly polarized in the appropriate directions and tuned slightly to the red of an atomic resonance, is split into three perpendicular retroreflected beams, doppler cooling the atoms and (with the appropriate magnetic field) confining them in all three dimensions (see Figure 3.2).

“In order to understand the mechanism by which a MOT is able to confine atoms, we must first introduce the Zeeman effect (Section 2.1) and a description of an optical molasses (Section 2.2). A functional MOT combines the forces resulting from these two mechanisms to trap and cool atoms.”

### 3.1.1 Zeeman Splitting

In the presence of an external magnetic field  $\vec{B}$ , the Hamiltonian associated with an atom’s orbital electrons will acquire an additional “Zeeman Shift” term, given by [23]

$$H_{\text{Zeeman}} = -\vec{\mu} \cdot \vec{B}, \quad (3.1)$$

where  $\vec{\mu}$  is the magnetic moment associated with the orbital under consideration. In the limit where the magnetic field is too weak to significantly disrupt the coupling between the electron’s spin- and orbital angular momenta,  $\vec{\mu}$  may be treated as being fixed with respect to changes in the magnetic field. It is this weak field regime which will be primarily of interest to us in work with magneto-optical traps.

With  $\vec{\mu}$  fixed, it is clear that the magnitude of the energy shift must scale linearly with the strength of the magnetic field. In considering the perturbation to the energy of a particular *transition*, the perturbations to the initial and final states must of

course be subtracted:

$$\Delta E_{\text{transition}} = -(\vec{\mu}_f - \vec{\mu}_i) \cdot \vec{B}. \quad (3.2)$$

Needs a level diagram. Maybe.

When this is combined with a circularly polarized laser beam, the effect is to move the atomic resonance closer to- or farther from- the frequency of the laser. The circular polarization, combined with some selection rules, means a circularly polarized laser will only couple to one particular transition, w.r.t. angular momentum. ie, for a  $\sigma_+$  polarized laser, the atom's overall angular momentum projection (along some axis) will be incremented by +1. The Zeeman shift means that in a magnetic field, this transition ( $M=+1$ ) not be the same as the  $M=-1$  transition. So, if you have a magnetic field that changes linearly across space, you can make it so that in  $+B_z$  regions, the laser beam with one certain polarization is closer to resonance and therefore more likely to be absorbed – and similarly, in  $-B_z$  regions, a different laser with the opposite polarization will be more likely to be absorbed. Again, if the B-field is linear in space, you can do it so that as the atoms get further and further from the 'centre' region, the effect gets progressively stronger. So, if you've done this right, you can make it so that the atoms get a stronger "push" back towards the center the farther away they've drifted.

They still get the optical molasses cooling effect for free.

### 3.1.2 Atom-Photon Interactions within a Laser

JB says: Chapter 3 (that's this chapter): Starting in Ch. 3 around p. 10, "Atom-Photon Interactions with a Laser" (that's this section):

You need to do a careful pass through it and omit everything you don't understand and don't need.

Sorry, there is no longer time to resolve your questions unless you need them.

This is a stupid subsection title.

Consider a single two-level atom in its ground state interacting with a single photon with the same energy as that of the atomic transition between the ground and excited states. If angular momentum selection rules allow it, the photon will be absorbed and the atom will receive a "push" from the incident photon's momentum. After a time, the atom will spontaneously de-excite by emitting a photon in a random direction. Conservation of momentum results in the atom receiving a second push from the emitted photon.

Also, if the photon is tuned close enough to atomic resonance, with absorption being more likely the smaller the detuning is...

If one considers a cloud of many such atoms within the path of a correctly-tuned (low intensity) laser beam, then provided the atoms are constrained to remain within

the beam path, the result of many such interactions is a biased random walk in physical space for each individual atom, and a net velocity change for the cloud as a whole.

In contrast to the above description of absorption followed by spontaneous emission, a photon interacting with an atom in the excited state may cause the atom to de-excite by emitting a second photon. This mechanism is negligible in the case of a sufficiently low intensity laser beam. However, as the laser intensity increases, so too will the fraction of atoms in the excited state at any given time. As it is only the excited state atoms that are able undergo stimulated emission, this effect becomes increasingly dominant as the intensity of the incident laser increases. In the limit of infinite intensity, stimulated emission and absorption are equally likely to occur, and therefore the population is split evenly between the ground and excited states.

To describe the regime change between atomic interactions with low intensity- and high intensity lasers, we introduce the (on-resonance) saturation intensity,  $I_{\text{sat}}$ , where

$$I_{\text{sat}} = \frac{\hbar\omega_0^3\gamma}{12\pi c^2} \quad (3.3)$$

is the laser intensity at which the rates of decay by stimulated- and spontaneous emission are equal,  $\hbar\omega_0$  is the energy of both the atomic transition and  $\gamma$  describes the linewidth of the atomic transition and equivalently its rate of spontaneous decay from its excited state. We may further define the on-resonance saturation parameter  $s_0$ , for a laser of intensity  $I$  as,

$$s_0 := \frac{I}{I_{\text{sat}}} \quad (3.4)$$

In practice, for the project that is the topic of this thesis, most lasers operate in the high intensity regime – the obvious exception being the photoionization laser (see Section whatever). However, one can still gain a qualitative understanding of many aspects of atom-laser interaction while neglecting saturation concerns.

But yeah, it seems like I have to mention that saturation intensity is a thing that exists, before I proceed to neglect it forever.

Where have I even decided to put the photoionization section, anyway?

Clunky phrasing.

Do I need saturation to make an optical molasses? I bet I do.

JB says: "In practice, for the project that is the topic of this thesis, most lasers operate in the high intensity regime – the obvious exception being the photoionization laser (see Section whatever) However, one can still gain ..." change to ->

"In practice, for the project that is the topic of this thesis, the MOT laser operates at about 1/2 of saturation intensity, while the optical pumping and photoionization lasers operate at considerably lower than saturation intensity. However, one can gain ..."

### 3.1.3 Doppler Cooling

We now consider a somewhat more general case in which a cloud of two-level atoms lies along the path of two counter-propagating laser beams, both detuned slightly to the red of resonance. For simplicity, this cloud will be treated as being constrained in the other two dimensions such that it must within the laser's path. With two counter-propagating laser beams of equal intensity and detuning, the “push” from interaction with one beam is exactly counteracted by the push from the opposite-propagating beam, and there is no net velocity transfer to the cloud.

Detuning the laser from resonance will of course decrease absorption upon interacting with an atom at rest – however the atoms within the cloud are not at rest, but rather are undergoing thermal motion. As such, within the rest frame of each individual atom, the two laser beams will appear to be Doppler shifted in opposite directions, with the sign dependent on atomic motion. In particular, atoms moving against a laser's direction of propagation will see that laser beam as being blueshifted. Since the laser has been red-detuned within the lab frame, the blueshift moves the laser frequency as seen by the atom back toward resonance, and makes its photons more likely to be absorbed. Similarly, for an atom moving in the same direction as a red-detuned laser beam, this laser will be seen as further red-shifted, and absorption likelihood is decreased. Because of the difference in absorption, such an atom becomes more likely to receive a push back against its (lab frame) direction of motion, slowing it down, and less likely to receive a push to increase its lab frame speed.

The overall effect on a one-dimensional cloud of atoms in the path of two counter-propagating red-detuned lasers is that the atoms will be slowed and cooled. Such a setup is sometimes referred to as a one-dimensional “optical molasses” due to the viscous drag force induced on atomic motion. It is straightforward to extend this model to three dimensions. Although this setup will decrease atomic velocity, it does

...and opposite polarization. Or something. I have to talk about the selection rules somewhere else.

Optical molasses equation? Maybe?

not include a confining force, so the atoms are still free to move out of the lasers' path, albeit at a decreased speed.

"...This will slow the atom down, at least up to a limit related to the linewidth of the atomic transition and/or the laser. There's something to look up."

### 3.1.4 Angular Momentum and Selection Rules

JB: 3.1.4 Angular Momentum and Selection Rules (that's this section!):

Sorry, this section is almost entirely wrong, and you don't need it. Omit it please.

JB: Agreed it's ok to ignore the repumper laser.

As in any other interaction, when a photon and an atom interact with one another, angular momentum must be conserved. An individual photon has spin 1, and therefore when we consider an atom-photon interaction where the photon is absorbed, the atom's total angular momentum projection (*I think on any axis???*) must also change by one unit.

Fuck. What even \*are\* the other rules? Have to change F too? I think that's true.. Clearly I have to look this up.

On a macroscopic level, the polarization of a laser can be used as a stand-in to describe, in a probabilistic way, the spin orientation of the individual photons of which the beam consists.

I mean. I probably don't need to go too in-depth here. But I haven't mentioned it at all yet, and it's a relevant thing.

Polarization matters. You have to conserve angular momentum. In fact, you have to conserve both total angular momentum \*and\* its projection on whatever axis. Photons have spin-1, but because they literally go at the speed of light, they can't do  $0 \rightarrow 0$  transitions.

This is related to why you need repumpers for your MOT, but also, I don't really want to get into that. But also-also, why even \*would\* you? You don't in the description I've already given. Pretty sure it's because you can sometimes de-excite to a \*stupid\* set of energy levels. And then, I don't think they really fall back down or something? I've largely forgotten how it works. Ugh.

## 3.2 Optical Pumping

3.2 (that's this section!):

You cover what is needed for optical pumping well in "4.2 the AC MOT and Polarization setup" Just say here indeed "The optical pumping process is described in detail in our collaboration's Ref. [? ]. The main detail described here is that the optical pumping is disturbed by any component of magnetic field not along the quantization axis. (Ours is the vertical axis, defined by the direction of the optical pumping light, and along which the detectors are placed.) This required sophistication with an AC MOT described below.

...

Then you can refer to that in section 3.4, where you're trying to now but the phrasing is poor."

Needs work.

"Until recently, one limitation of such samples was the necessity for the presence of a relatively large magnetic field, which is expected to partially destroy atomic polarization, limiting the precision of many types of measurements. Here we discuss the construction of a newer type of MOT, the AC-MOT, which minimizes residual magnetic fields. The guys in [5] came up with the idea of the AC-MOT. They made it work and did some stuff with it. Good for them."

Need a nice, uniform, constant magnetic field for your polarization to larmor precess around. Then, however depolarized (from the axis of the magnetic field) you were to start out, you don't like precess in a way that changes the projection you care about.

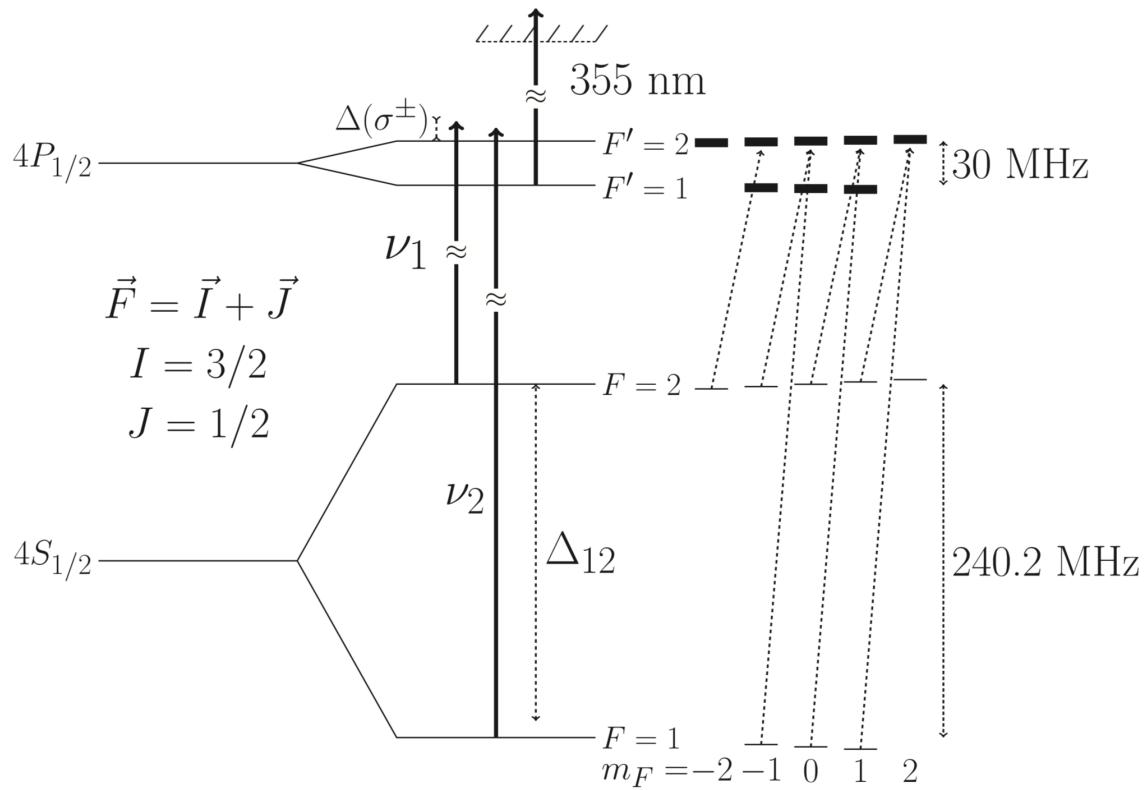
But also, I should actually describe the optical pumping, too. And point at Ben's OP paper that we did [1].

Somewhere later, I can talk about photoionization? Or should it be here, in this chapter?

Surely it should at least get a different section though.

Do I need a MOT level diagram too?

JB says: "I would say you don't need an atomic level diagram. You could just describe in words the semiclassical picture of atoms absorbing photons until they are nearly fully polarized, then they stop absorbing. The optical pumping + photoionization is then an in situ probe of the polarization. ... You would need to add in words that quantum mechanical corrections to this picture are in the optical Bloch equation approach in B. Fenker et al. The depolarized states still have high nuclear polarization (1/2 for  $F = 2, M_F = 1$ , 5/6 for  $F = 1, M_F = 1$ ) and determining the ratio of those two populations provides most of the info we need – we model with the O.B.E, measure the optical pumping light polarization, and float an average transverse magnetic field. This is adequate to determine the depolarized fraction to 10% accuracy, which is all that is needed."



Describe what's going on here!

Figure 3.1: An atomic level diagram for the optical pumping of  $^{37}\text{K}$ , taken from [1].

### 3.3 Atom Trapping with a MOT

JB:

...Then you can refer to that (currently section 3.2, "Optical Pumping") in section 3.4, where you're trying to now but the phrasing is poor.

3.3 Atom Trapping with a MOT

and

3.4 The AC-MOT

The content and scope are ok, but the informal phrasing is going to confuse people. You have to pay some attention and rephrase these sections.

Do I \*have\* an equation I can put here? Surely there must be one somewhere, but I really don't want to dig it up, and I don't think it's really necessary.

Needs work.

Really, at the end of the previous section, I described the MOT's trapping mech-

anism. That's literally what it is. You just need to do it in 3 dimensions, rather than only one. Fortunately, an anti-helmholz(sp?) coil gives us a quadrupole-shaped magnetic field, which \*actually\* has a magnetic field that changes linearly along any axis in the region near the center.

Optical molasses + zeeman splitting = magneto-optical trap. Anyway, see Fig. 3.2.

Do I \*really\* need to bring up the repumper? I don't wanna. Maybe I can just mention it and move on or something.

## 3.4 The AC-MOT

Citation for Harvey and Murray goes here [5]. Also, myself [2].

Needs work.

Here's a diagram of our AC-MOT running one AC-MOT/OP cycle, in Fig. 3.3.

Normal MOTs are DC-MOTs. They just sort-of go. It's continuous. We used an AC-MOT though! The point of an AC-MOT is to shut off the magnetic field as quickly as possible. With a well-controlled and uniform magnetic field, we can optically pump the atoms, which I think I'm going to describe in the upcoming section (3.2).

Sadly, this also removes our trapping mechanism. We could keep the optical molasses after the field is off if we wanted to, but we don't, because we wouldn't be able to optically pump the atoms then. But at least the atoms are cold-ish (we can measure! I think it's done indirectly in that one table, or for realsies in Ben's thesis), so we can let them just chill for a little while before we have to re-trap them. Don't lose too much.

Anyway, the idea of the AC-MOT is to run a sinusoidal current through your anti-Helmholtz coils. You'll get eddy currents in your nearby metal \*stuff\* when you have a changing current, and those will \*also\* make a magnetic field. So, the idea with an AC-MOT is that with a sinusoid, you have clear control over what the eddy currents are actually doing, and you can just shut the current off when the eddy currents are zero (current in the anti-Helmholtz coils will be close to zero at this time too, depending on frequency of the sinusoid...), so you can reduce the size of the eddy currents by like an order of magnitude. Eddy currents in general take a while to die away, so it's good to make them as small as possible. ...Also, eddy currents making a magnetic field will screw up your optical pumping, because .... um, reasons? I think

How much do we lose? Have we quantified that somewhere? Probably.

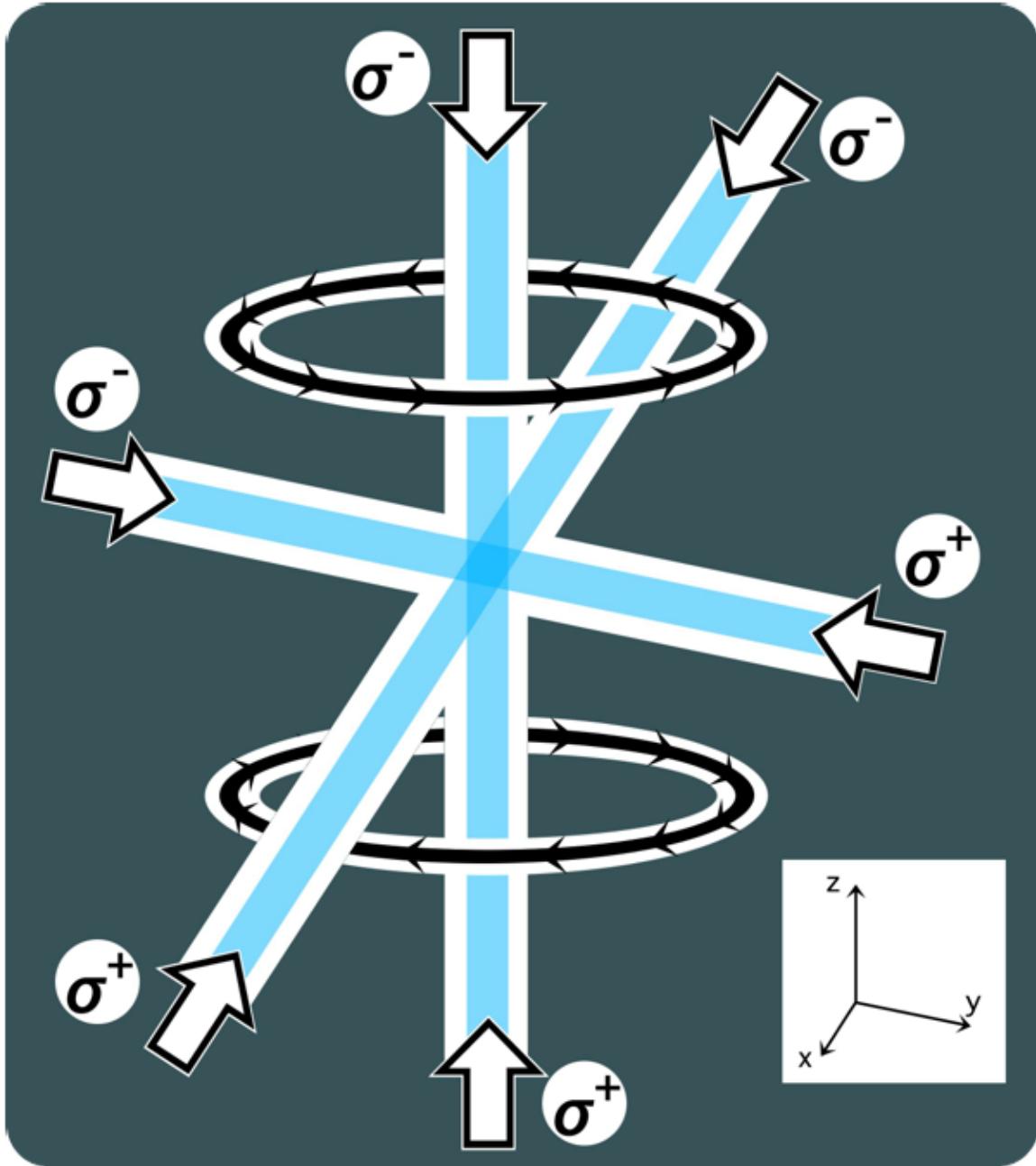


Figure 3.2: Components of a magneto-optical trap, including current-carrying magnetic field coils and counterpropagating circularly polarized laser beams. Diagram taken from [2]

it's not just the detuning, it's also something about the Larmor precession.

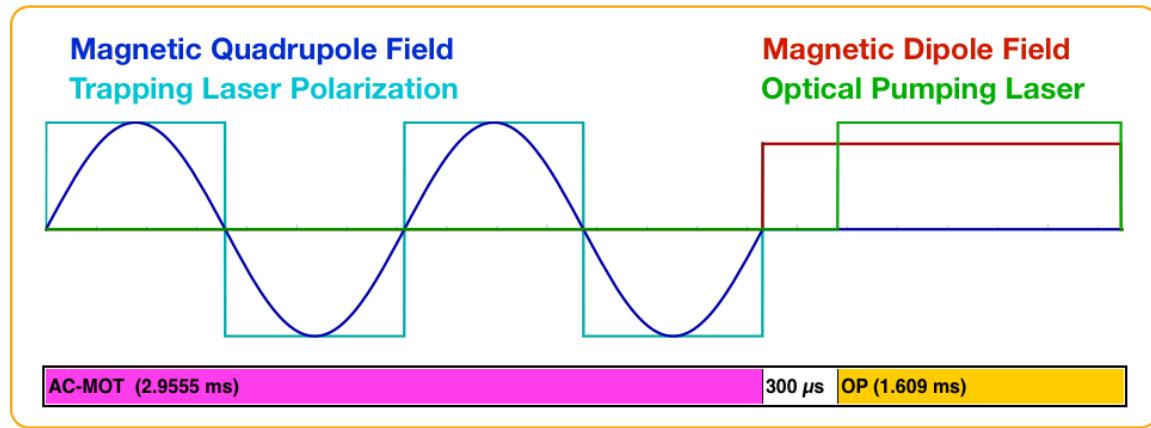


Figure 3.3: One cycle of trapping with the AC-MOT, followed by optical pumping to spin-polarize the atoms. After atoms are transferred into the science chamber, this cycle is repeated 100 times before the next transfer. The magnetic dipole field is created by running parallel (rather than anti-parallel as is needed for the MOT) currents through the two coils.

### 3.5 Photoionization as a Probe

JB on 3.5 'Photoionization as a probe' (that's this section!):

You describe this well in 4.4 "Photoionization laser"

So I recommend strongly you cut the whole 3.5 section, because it's incomplete, and what you have contains errors.

...

Here are things there are not time to understand:

...

about 1/3 of the MOT atoms are excited, not 'a large fraction'

...

yes, we've intentionally polarized the OP laser to be circular so the atoms will be polarized.

We've also picked the S 1/2 to P1/2 so that the fully stretched S1/2  $F = 2$   $m_F = 2$  state is dark. (Unlike the MOT, where we need the P3/2 state so that atoms never go dark, or the force would stop.) But we agreed above not to talk about this.

...

"We have a photodiode(?) to observe what fraction of things get photoionized overall."

...

no, we look at the photoionization rate, for which we have the ion vs. photodiode TOF ('photodiode' looks at when the bursts of light occur)

So, the photoionization laser. It ionizes only  $\approx 1\%$  of the atoms, so it's mostly non-destructive.

For atoms in a MOT, it can give a projection of the trap position. At least, it can if you add some MCPs with delay lines, and an electric field. Also, the atoms have to be excited or they don't get photoionized this way. That's because at any given time, a large fraction of the trapped atoms are in the excited state, so it works.

For polarized or partially polarized atoms, absorption stops, because of selection rules. We've intentionally polarized the laser to do this. I think.

Include a picture of the trap. Possibly on both the rMCP and the eMCP. Also, maybe a thing for the polarization as a function of time? But I don't really need to do that, because it's in the paper.

Anyway, the probing only works if you have a detector to see what things. We have a photodiode(?) to observe what fraction of things get photoionized overall. This gives us polarization. We also have an MCP with delay lines, and an electric field to guide (positive) ions into it. And another MCP with delay lines on the other side of the electric field, to collect the electrons removed by photoionization. Also, the (orbital) electrons removed during beta decay. The photoionization laser is pulsed. So, when there's a pulse, we can check whether there was a hit detected in the ion detector and/or the electron detector. It turns out, we can't check both at the same time, because our two detectors hate each other. They're natural enemies, really. Turns out, the ion detector is better at measuring the polarization, and the trap position. Because reasons. Anyway, during the 2014 run, we spent like half of the beamtime collecting ions and half collecting electrons.

\*Have\* we even polarized the photoionization laser in any particular way? I'm not sure we even have. Why doesn't it absorb? When it's stretched, it's not just all in the excited state! It's...stretched. Do angular momentum selection rules even apply if the electron is going into the continuum?

But it's worse for  $A_\beta$  and  $b_{\text{Fierz}}$ , because .... reasons that probably shouldn't go in this chapter.

This also doesn't go in this section.

# Chapter 4

## The Experimental Setup

JB: First, I'm pretty sure you should combine Chs. 3 and 4. Ch 4 has good solid general description and details *in situ* about optical pumping and about the photoionization probe that are, unfortunately, wrong in Ch. 3. You could call the first half of such a chapter "General considerations of Atomic techniques used" and the 2nd half "Experimental Implementation of Atomic Techniques used"

Remember the pulser LED! To evaluate the stability of the scintillator gain!

### 4.1 Overview of the Double MOT System and Duty Cycle

JB says: chapter 4.1 (this section) is really good, and in good shape for the committee

We obtain a sample of neutral, cold, nuclear spin-polarized  $^{37}\text{K}$  atoms with a known spatial position, via the TRIUMF accelerator facility, by intermittently running a magneto-optical trap (MOT) to confine and cool the atoms, then cycling the trap off to polarize the atoms. With  $\beta$  detectors placed opposite each other along the axis of polarization, we are able to directly observe the momenta of  $\beta^+$  particles emitted into 1.4% of the total solid angle nearest this axis. We also are able to extract a great deal of information about the momentum of the recoiling  $^{37}\text{Ar}$  daughters by measuring their times of flight and hit positions on a microchannel plate detector with a delay line. Because the nuclear polarization is known to within < 0.1% [1], and we are able to account for many systematic effects by periodically reversing the polarization and by collecting unpolarized decay data while the atoms are trapped within the MOT, we expect to be well equipped to implement a test of 'handedness' within the nuclear weak force.

The experimental subject matter of this thesis was conducted at TRIUMF using

the apparatus of the TRIUMF Neutral Atom Trap (TRINAT) collaboration. The TRINAT laboratory offers an experimental set-up which is uniquely suited to precision tests of Standard Model beta decay physics, by virtue of its ability to produce highly localized samples of cold, isotopically pure atoms within an open detector geometry. Although the discussion in this chapter will focus on the methodologies used to collect one particular dataset, taken over approximately 7 days of beamtime in June 2014, the full apparatus and the techniques used are fairly versatile, and can be (and have been) applied to several related experiments using other isotopes.

Surely most of this paragraph goes in an intro chapter somewhere.

Cite a bunch of papers here.

### TRINAT DOUBLE MOT TRAPPING SYSTEM

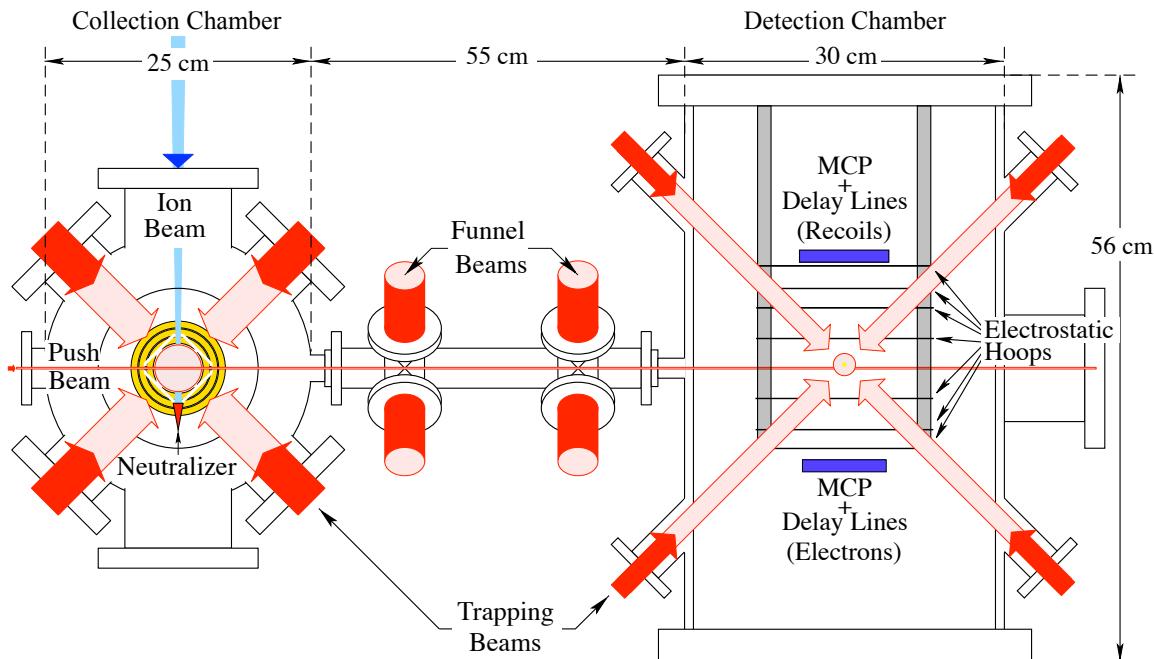


Figure 4.1: The TRINAT experimental set-up, viewed from above. The two MOT system reduces background in the detection chamber. Funnel beams along the atom transfer path keep the atoms focused.

Figure was originally created by Alexandre, modified by ... someone else? Or Alexandre? And I got it from ... probably an experimental proposal? I should figure out how to cite a proposal...

The TRINAT lab accepts radioactive ions delivered by the ISAC beamline at TRIUMF. These ions are collected on the surface of a hot zirconium foil where they are electrically neutralized, and subsequently escape from the foil into the first of two experimental chambers (the “collection chamber”). Further details on the neutral-

ization process are presented in a previous publication [24]. Within the collection chamber, atoms of one specific isotope – for the purposes of this thesis,  $^{37}\text{K}$  – are continuously collected into a magneto-optical trap (MOT) from the tail end of the thermal distribution. Although this procedure preferentially traps only the slowest atoms, once trapped, atoms will be cooled further as a side-effect of the MOT’s trapping mechanism. The result is a small ( $\sim 1$  mm diameter), cold ( $\sim 1$  mK) cloud of atoms of a particular isotope.

defined ‘MOT’ in Ch.3.

Mumble mumble UHV. Mumble mumble tail end of the Boltzmann distribution.

These properties of the atomic cloud allow for a relatively clean transfer of linear momentum from an appropriately tuned laser beam to the atoms within the cloud, and we use this mechanism to “push” the atoms out of the collection MOT and into the “detection chamber”, where they are loaded into a second MOT (see Fig. 4.1). During regular operation, atoms are transferred approximately once per second.

There is no need to release previously trapped atoms in the second MOT when a new group of atoms is loaded. Although the trap loses atoms over time as a result of a variety of physical processes, during typical operation the majority of atoms loaded in a given transfer will still be trapped at the time the next set of atoms is loaded, and after several transfer cycles, something like a steady state is obtained.

discussed ... idk, somewhere else.

Because the transfer and trapping mechanisms rely on tuning laser frequencies to specific atomic resonances, these mechanisms act on only a single isotope, and all others remain unaffected. The result is a significant reduction of background contaminants within the detection chamber relative to initial beamline output. The transfer methodology is discussed in some detail within another publication [25].

We now turn our attention to what happens to the atom cloud in the detection chamber between loading phases (see Fig. 4.2). One of the goals for the 2014  $^{37}\text{K}$  beamtime required that the atom cloud must be spin-polarized, as well as being cold and spatially confined. Although the MOT makes it straightforward to produce a cold and well confined cloud of atoms, it is fundamentally incompatible with techniques to polarize these atoms. The physical reasons behind this are discussed in Section 4.2.

Once the newly transferred set of  $^{37}\text{K}$  atoms has been collected into the cloud, the entire MOT apparatus cycles 100 times between a state where it is ‘on’ and actively confining atoms, and a state where it is ‘off’ and instead the atoms are spin-polarized by optical pumping while the atom cloud expands ballistically before being re-trapped. These 100 on/off cycles take a combined total of 488 ms. The laser components of

I \*do\* discuss this, right? Right??

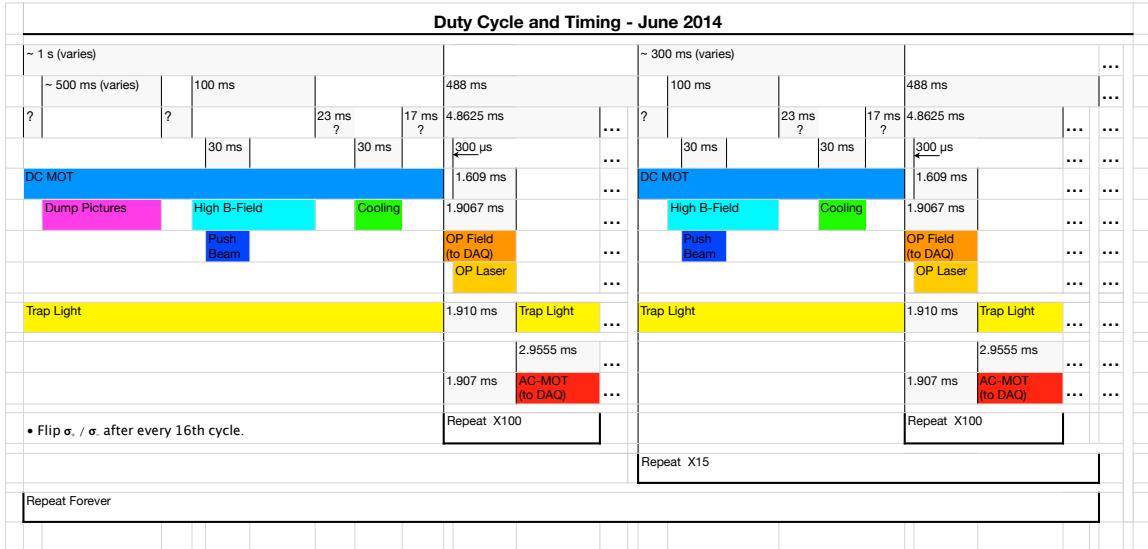


Figure 4.2: The duty cycle used for transferring, cooling, trapping, and optically pumping  $^{37}\text{K}$  during the June 2014 experiment. Not drawn to scale. Question marks indicate timings that varied either as a result of electronic jitter or as a result of variable times to execute the control code. Atoms are transferred during operation of the DC-MOT. Though the push beam laser itself is only on for 30 ms, the bulk of the DC-MOT's operation time afterwards is needed to collect and cool the transferred atoms. After 100 on/off cycles of optical pumping and the AC-MOT, the DC-MOT resumes and the next group of atoms is transferred in. After 16 atom transfers, the polarization of the optical pumping laser is flipped to spin-polarize the atoms in the opposite direction, in order to minimize systematic errors.

the trap are straightforward to cycle on and off on these timescales, but the magnetic field is much more challenging to cycle in this manner.

How to segue here? Do I... Cite myself? Cite Harvey+Murray? Reference the next section?

Reference the previous chapter? This is clunky and stupid.

Immediately following each set of 100 optical pumping cycles, another set of atoms is transferred in from the collection chamber to the detection chamber, joining the atoms that remain in the trap (see Fig. 4.2). The details of the trapping and optical pumping cycles are described further in Section 4.2, and the optical pumping technique and its results for this beamtime are the subject of a recent publication [1].

## 4.2 The AC-MOT and Polarization Setup

JB on Chapter 4.2 "The AC-MOT and Polarization Setup" (that's this section!):

...

"We use only the polarized portion of the duty cycle in order to minimize other systematic errors, such as the scintillator energy calibration and overall trap position."

...

We need to fix this. We use the polarized portion of the cycle because the atoms are almost totally polarized during it, and almost totally unpolarized outside of it. You leave the impression we could measure  $b_{Fierz}$  during the rest of the cycle, during unpolarized time ?

...

MJA: Ugh, no, that's really not what I meant. Oh well, time to rephrase.

Probably document things about the waveform and frequency used for the beamtime, since I don't think it's in my MSc.

Some of the contents of this section should be removed from here and moved over to Ch-3.

Probably.

As alluded to in the previous section (4.1), the measurement in question required a spin-polarized sample of atoms, and a precise knowledge of what that polarization was. This was primarily needed in order to facilitate a measurement of  $A_\beta$  that was performed on the same data that is the subject of discussion here. [26] While this is arguably less critical to a measurement of  $b_{Fierz}$ , it can still be an asset for eliminating systematic effects. We use only the polarized portion of the duty cycle in order to minimize other systematic errors, such as the scintillator energy calibration and overall trap position. It also makes for a more straightforward interpretation of the relationship of the measured values of  $A_\beta$  and  $b_{Fierz}$  when the systematic effects are the same for both measurements. Finally, using only polarized data allows us to make use of the ‘superratio’ construction in data analysis, a powerful tool for reducing (many) systematic errors at the expense of statistical precision (see Chapter 5).

have I defined the beta asymmetry yet in some previous chapter? otherwise I have to do it here..

have I defined  $b_{Fierz}$  yet?

Plus, it barely makes sense to talk about measuring  $b_{Fierz}$  if you don't know  $A_\beta$ .

I could move this (above) whole paragraph over to Ch.5, where I surely discuss this in more detail. It provides nice context here though.

The Magneto-Optical Trap is a well-known technique from atomic physics, used to confine and cool neutral atoms [22], and it is also discussed in more detail in Chapter 3.

Removed: stuff about how a MOT works. It's in Ch.3. It lives there now.

The TRINAT science chamber includes 6 ‘viewports’ specifically designed to be

Should I move a bunch of this section's content over there, to live in Ch.3?? Should I move a bunch of content from Ch.3 to live here? Current thinking: It seems best to split the content up into a general picture of the MOT (Ch.3), and stuff that's unique to this experiment (here).

used for the trapping laser (see Fig. 4.4.).

Is my photoionization description adequate? ... in light of John's feedback: no.

JB says: "Since you worked hard on the logic triggers, a photoion spectrum with duty cycle would be appropriate if you want."

Need to describe how polarization works.

JB says: all polarization details could be deferred to [1]. (be sure to list all authors including [me]). )

A MOT also requires a quadrupolar magnetic field, which we generate with two current-carrying anti-Helmholtz coils located within the vacuum chamber itself. The coils themselves are hollow, and are cooled continuously by pumping temperature-controlled water through them.

One feature which makes our MOT unusual has been developed as a result of our need to rapidly cycle the MOT on and off – that is, it is an “AC-MOT”. Rather than running the trap with one particular magnetic field and one set of laser polarizations to match, we run a sinusoidal AC current in the magnetic field coils, and so the sign and magnitude of the magnetic field alternate smoothly between two extrema, and the trapping laser polarizations are rapidly swapped to remain in sync with the field [5][2]. See Figure 3.3.

Note that because the atoms within a MOT can be treated as following a thermal distribution, some fraction of the fastest atoms continuously escape from the trap's potential well. Even with the most carefully-tuned apparatus, the AC-MOT cannot quite match a similar standard MOT in terms of retaining atoms. The TRINAT AC-MOT has a ‘trapping half-life’ of around 6 seconds, and although that may not be particularly impressive by the standards of other MOTs, it is more than adequate for our purposes.  $^{37}\text{K}$  itself has a radioactive half-life of only 1.6 seconds (cite someone), so our dominant loss mechanism is radioactive decay rather than thermal escape.

We spin-polarize  $^{37}\text{K}$  atoms within the trapping region by optical pumping [1]. A circularly polarized laser is tuned to match the relevant atomic resonances, and is directed through the trapping region along the vertical axis in both directions. When a photon is absorbed by an atom, the atom transitions to an excited state and its total angular momentum (electron spin + orbital + nuclear spin) along the vertical axis is incremented by one unit. When the atom is de-excited a photon is emitted isotropically, so it follows that if there are available states of higher and lower angular momentum, the *average* change in the angular momentum projection is zero. If the atom is not yet spin-polarized, it can absorb and re-emit another photon, following a

biased random walk towards complete polarization.

In order to optimally polarize a sample of atoms by this method, it is necessary to have precise control over the magnetic field. This is because absent other forces, a spin will undergo Larmor precession about the magnetic field lines. In particular, the magnetic field must be aligned along the polarization axis (otherwise the tendency will be to actually depolarize the atoms), and it must be uniform in magnitude over the region of interest (otherwise its divergencelessness will result in the field also having a non-uniform direction, which results in a spatially-dependent depolarization mechanism). Note that this type of magnetic field is not compatible with the MOT, which requires a linear magnetic field gradient in all directions (characteristic of a quadrupolar field shape), and has necessitated our use of the AC-MOT as described in (Sub-)Section 3.4.

At some point I have to decide if that's going to be a section or a subsection.

## 4.3 Measurement Geometry and Detectors

JB says of Chapter 4.3 (this section: first few paragraphs are poorly phrased. You have to fix those. Most of the rest looks really good.

This section is really disorganized.

Possibly needs more diagrams? Possibly just needs more words.

MCPs. Hoops. Only one MCP works at a time! Blarg. Upon decay, atoms literally aren't trapped anymore by the trap. No trapping forces, no slowing forces, because it's all isotope-specific.

Back-to-back MCPs in an electric field to tag events from the trap, and to measure the trap position and polarization. Hoops to produce the electric field. The electric field sucks negatively charged electrons into one MCP, and positively charged ions into the other. See Fig. 4.3

Back-to-back beta detectors along the polarization axis.

Many laser ports to make the MOT functional, and for optical pumping. Fancy mirror geometry to combine optical pumping and trapping light along the vertical axis. Water-cooled (anti-)Helmholz coils within the chamber for the AC-MOT, fast switching to produce an optical pumping field.

Detectors are positioned about the second MOT for data collection. The detection chamber operates at ultra-high vacuum (UHV) and provides not only the apparatus

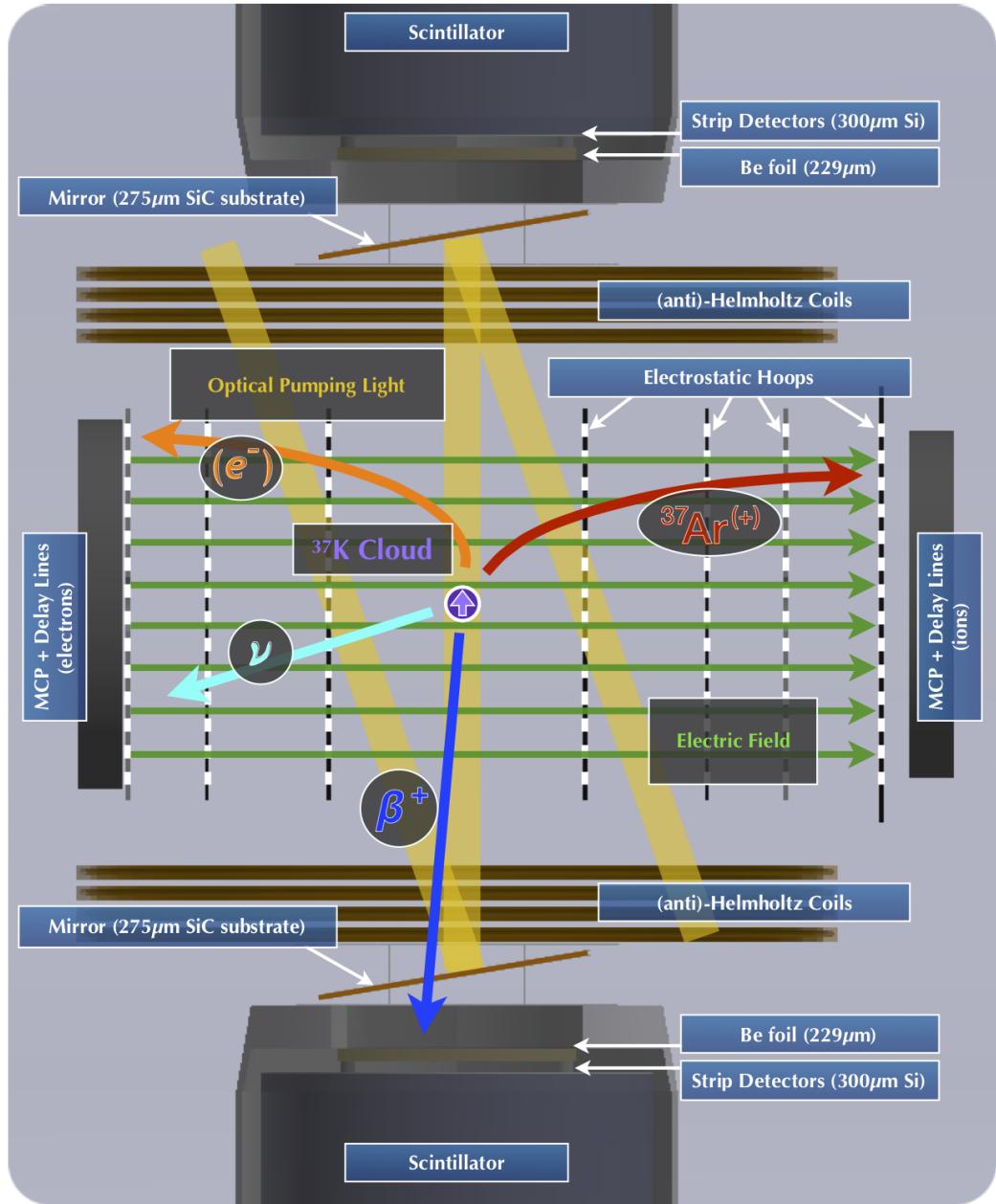


Figure 4.3: A decay event within the TRINAT science chamber. After a decay, the daughter will be unaffected by forces from the MOT. Positively charged recoils and negatively charged shake-off electrons are pulled towards detectors in opposite directions. Although the  $\beta^+$  is charged, it is also highly relativistic and escapes the electric field with minimal perturbation.

necessary to intermittently confine and then spin-polarize atoms, but also the variety of detectors and implements required to quantify their position, temperature, and

polarization. The detection chamber further boasts an array of electrostatic hoops to collect both positively and negatively charged low energy particles into two microchannel plates (MCPs), and a further set of two beta detectors positioned along the polarization axis, each of which consists of a 40x40 pixel double-sided silicon strip detector (DSSD) and a scintillator and photomultiplier tube (PMT).

....(shown in Figure 4.4) ...

The beta detectors, located above and below the atom cloud along the axis of polarization (see Figure 4.3), are each the combination of a plastic scintillator and a set of silicon strip detectors. Using all of the available information, these detectors are able to reconstruct the energy of an incident beta, as well as its hit position, and provide a timestamp for the hit's arrival. Together the upper and lower beta detectors subtend approximately 1.4% of the total solid angle as measured with respect to the cloud position.

The two sets of beta detectors were positioned directly along the axis of polarization. Each beta detector consists of a plastic scintillator and photo-multiplier tube (PMT) placed directly behind a 40×40-pixel double-sided silicon strip detector (DSSD). The scintillator is used to measure the overall energy of the incoming particles, as well as to assign a timestamp to these events, while the DSSD is used both to localize the hit position to one (or in some cases, two) individual pixel(s), and also to discriminate between different types of incoming particles. In particular, though the scintillator will measure the energy of an incoming beta or an incoming gamma with similar efficiency, the beta will lose a portion of its kinetic energy as it passes through the DSSD into the scintillator. By contrast, an incident gamma will deposit only a very small amount of energy in the DSSD layer, making it possible to reject events with insufficient energy deposited in the DSSD as likely gamma ray events. Given that the decay of interest to us emits positrons, we expect a persistent background 511 keV gamma rays that are not of interest to us, so it is extremely important that we are able to clean these background events from our spectrum.

It must be noted that the path between the cloud of trapped atoms and either beta detector is blocked by two objects: a 275  $\mu\text{m}$  silicon carbide mirror (necessary for both trapping and optical pumping), and a 229  $\mu\text{m}$  beryllium foil (separating the UHV vacuum within the chamber from the outside world). In order to minimize beta scattering and energy attenuation, these objects have had their materials selected

There's gotta be a better way to describe it

what's the open area of the detector? how big is each pixel?

to use the lightest nuclei with the desired material properties, and have been manufactured to be as thin as possible without compromising the experiment. As the  $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$  decay process releases  $Q = 5.125 \text{ MeV}$  of kinetic energy [27], the great majority of betas are energetic enough to punch through both obstacles without significant energy loss before being collected by the beta detectors.

On opposing sides of the chamber, and perpendicular to the axis of polarization, two stacks of  $\sim 80 \text{ mm}$  diameter microchannel plates (MCPs) have been placed (see Figure 4.4) as detectors, providing a time stamp when a particle is incident on their surfaces. Behind each stack of MCPs there is a set of delay lines, which provide position sensitivity for these detectors.

In order to make best use of these MCPs, we create an electric field in order to draw positively charged particles into one MCP, while drawing negatively charged electrons into the other MCP. Seven electrostatic hoops have been placed within the chamber (see Figure 4.4), and are connected to a series of high voltage power supplies. See Sections 4.4 and I.3 for a discussion of what sort of charged particles we expect to observe in these detectors and how they are created.

Wait. Am I repeating myself?

Scientific data has been collected at field strengths of  $66.7 \text{ V/cm}$ ,  $150 \text{ V/cm}$ ,  $395 \text{ V/cm}$ ,  $415 \text{ V/cm}$ , and  $535 \text{ V/cm}$ . It should be noted that these field strengths are too low to significantly perturb any but the least energetic of the (positively charged) betas from the decay process, and these low energy betas would already have been unable to reach the upper and lower beta detectors due to interactions with materials in the SiC mirror and Be foil vacuum seal.

## 4.4 Photoionization Laser

Some of this stuff is in Ch.3 (see Section 3.5) and/or Ch.6 (see Section 6.1). Some of it \*should\* be.

Chapter 4.4 (this section!) is very good and complete, showing you understand what is needed about photoionization. A good reason to omit 3.5 'photoionization as a probe' as I said above.

In order to measure properties of the trapped  $^{37}\text{K}$  cloud, a  $10 \text{ kHz}$  pulsed laser at  $355 \text{ nm}$  is directed towards the cloud. These photons have sufficient energy to photoionize neutral  $^{37}\text{K}$  from its excited atomic state, which is populated by the trapping laser when the MOT is active, releasing  $0.77 \text{ eV}$  of kinetic energy, but do

not interact with ground state  $^{37}\text{K}$  atoms. The laser is of sufficiently low intensity that only  $\sim 1\%$  of excited state atoms are photoionized, so the technique is only very minimally destructive.

Probably worth mentioning that we test this stuff offline on stable  $^{41}\text{K}$ .

Because an electric field has been applied within this region (see Section ??) the  $^{37}\text{K}^+$  ions are immediately pulled into the detector on one side of the chamber, while the freed  $e^-$  is pulled towards the detector on the opposite side of the chamber. Because  $^{37}\text{K}^+$  is quite heavy relative to its initial energy, it can be treated as moving in a straight line directly to the detector, where its hit position on the microchannel plate is taken as a 2D projection of its position within the cloud. Similarly, given a sufficient understanding of the electric field, the time difference between the laser pulse and the microchannel plate hit allows for a calculation of the ion's initial position along the third axis.

JB: "you could reference the letter for the value of the field 150V/cm."

As a check: the camera measurements for photons from de-excitation. It's aimed 35 degrees from vertical, with its horizontal axis the same as ..... one of the other axes. I think it's the TOF axis. I can check this when my computer comes back. Also, there's an unknown additional delay between some of our DAQ channels that can't be explained by accounting for cable lengths, so we really like having the check there.

JB says: "yes, camera x-axis is tof axis."

With this procedure, it is possible to produce a precise map of the cloud's position and size, both of which are necessary for the precision measurements of angular correlation parameters that are of interest to us here. However, it also allows us to extract a third measurement: the cloud's polarization.

The key to the polarization measurement is that only atoms in the excited atomic state can be photoionized via the 355 nm laser. While the MOT runs, atoms are constantly being pushed around and excited by the trapping lasers, so this period of time provides a lot of information for characterizing the trap size and position. When the MOT is shut off, the atoms quickly return to their ground states and are no longer photoionized until the optical pumping laser is turned on. As described in Section ??, and in greater detail in [1], the optical pumping process involves repeatedly exciting atoms from their ground states until the atoms finally cannot absorb any further angular momentum and remain in their fully-polarized (ground) state until they are perturbed. Therefore, there is a sharp spike in excited-state atoms (and therefore photoions) when the optical pumping begins, and none if the cloud has been fully

polarized. The number of photoion events that occur once the sample has been maximally polarized, in comparison with the size and shape of the initial spike of photoions, provides a very precise characterization of the cloud's final polarization [1].

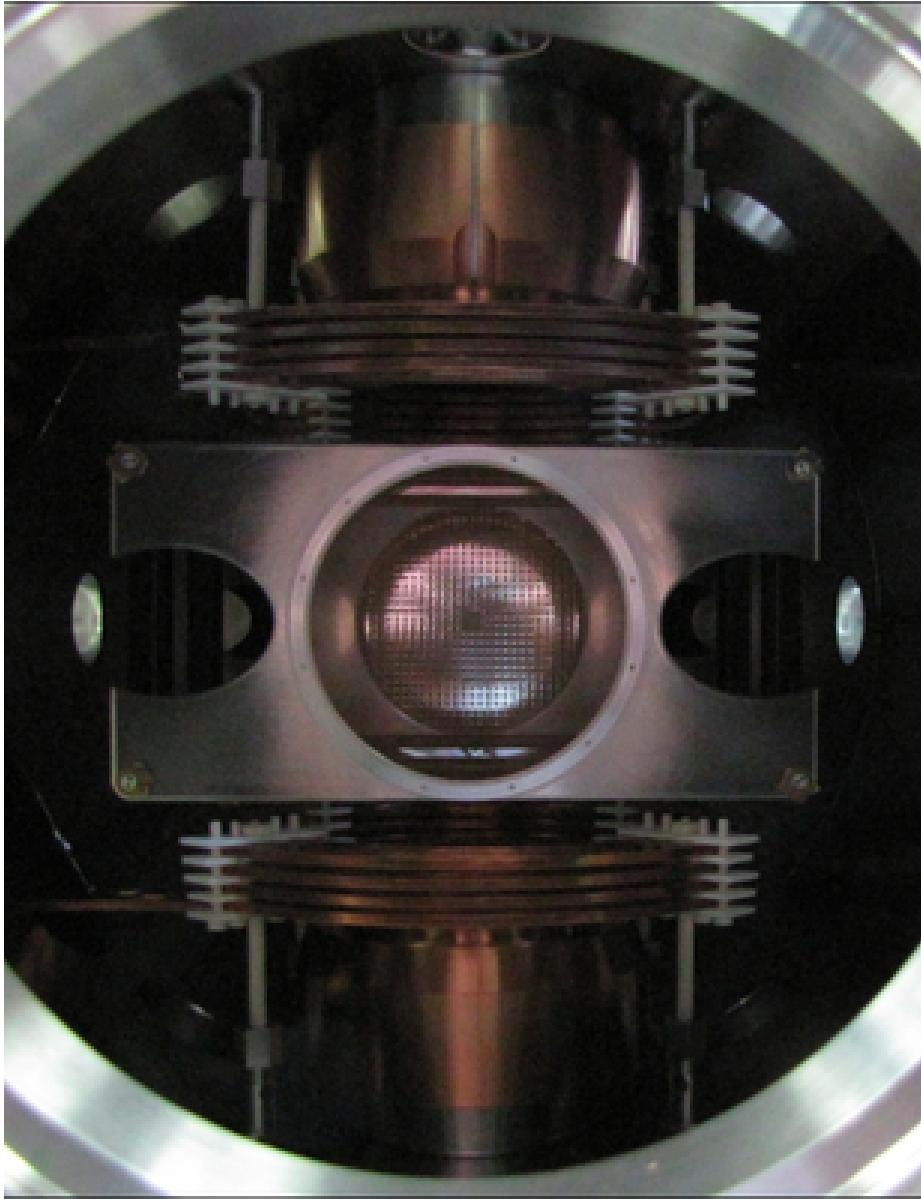


Figure 4.4: Inside the TRINAT science chamber. This photo is taken from the vantage point of one of the microchannel plates, looking into the chamber towards the second microchannel plate. The current-carrying copper Helmholtz coils and two beta telescopes are visible at the top and bottom. The metallic piece near the center is one of the electrostatic ‘hoops’ used to generate an electric field within the chamber. The hoop’s central circular hole allows access to the microchannel plate, and the two elongated holes on the sides allow the MOT’s trapping lasers to pass unimpeded at an angle of 45 degrees ‘out of the page’.

# Chapter 5

## The Experimental Signature

JB: “I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now.”

Possibly this can be combined with the “Background and Motivation” or “Theory” chapters?

Why do I even \*have\* two of those chapters, if not for this? Anyway, surely I don’t need \*three\* of them...

### 5.1 General Stuff

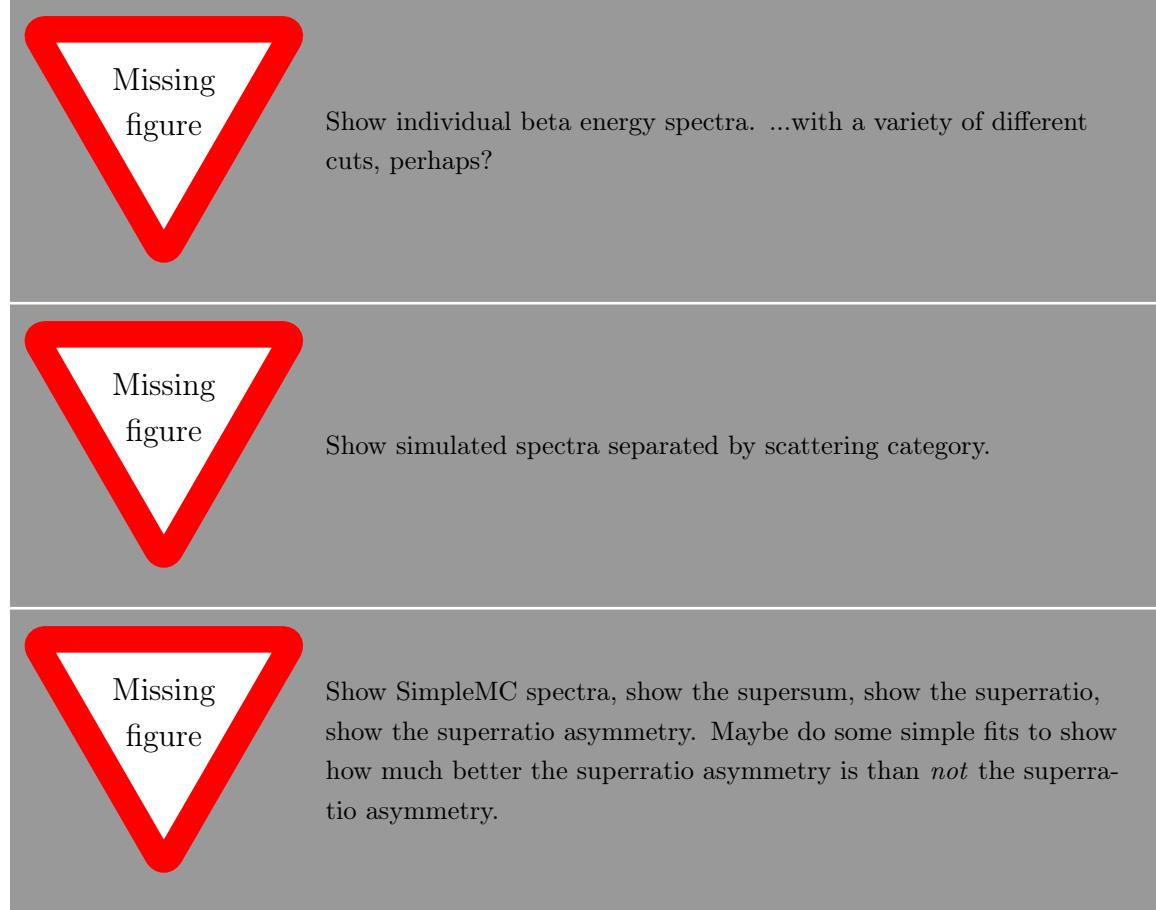
The point is, the presence of either scalar or tensor interactions will produce a  $b_{\text{Fierz}}$  term in the decay PDF. It has other effects on the PDF, but those come in at higher-order in the tiny scalar and tensor couplings. So, the Fierz term would be by far the biggest thing that changes in the PDF. The PDF describes the energy and momentum of the outgoing beta w.r.t. a variety of other things. Notably, we can write an elegant-ish description of beta momentum w.r.t. nuclear polarization direction, and ignore the neutrino completely after integrating over it. We have a PDF in beta *direction* (w.r.t. polarization), and beta *energy*. To lowest order (and lowest order is best order) the distribution w.r.t. polarization direction doesn’t change, but the distribution w.r.t. energy does change. Or ... something? The point is, it makes a change in the beta energy spectrum. This change is most pronounced at low energies, because the Fierz term is scaled by  $(1/E_\beta)$ . However, the asymmetry is also a function of  $E_\beta$ . A different function of  $E_\beta$ . In fact, it is scaled by  $(p_\beta/E_\beta)$  within the PDF, which is distinctly different than  $b_{\text{Fierz}}$ . So, one might ask what effect a  $b_{\text{Fierz}}$  term would produce on a constructed asymmetry spectrum. ....This explanation has gone way off track.

## 5.2 TBD

JB: You need to at some point say that the supersum is the beta energy spectrum. There are experiments trying to do this method better, but they are very difficult. UCNA published a combined energy spectrum and Abeta[Ebeta] analysis on the neutron in March 2020 [6].

I can't help but also notice the follow-up article from September 2020 [7]. Ugh.

I really need an excuse to include more pictures of data. Also, more pictures of simulations.



## 5.3 The Superratio and Asymmetry

The data can be combined into a superratio asymmetry. This has the benefit of causing many systematics to cancel themselves out at leading order. It also will increase the fractional size of the effects we're looking for. This can be shown by using math.

## 5.4 Signature of a Fierz Term in This Experiment

Not all systematics effects are eliminated. We'll want to be careful to propagate through any effects that are relevant. Using the superratio asymmetry as our physical observable makes this process a bit messier for the things that don't cancel out, but it's all just math.

## 5.5 Comparative Merits of the Superratio and Supersum for Measurement

Some other groups have performed similar measurements using the supersum as the physical observable. There are pros and cons to both methods. I can show, using a back-of-the-envelope calculation, that for this particular dataset, the superratio asymmetry method produces a better result.

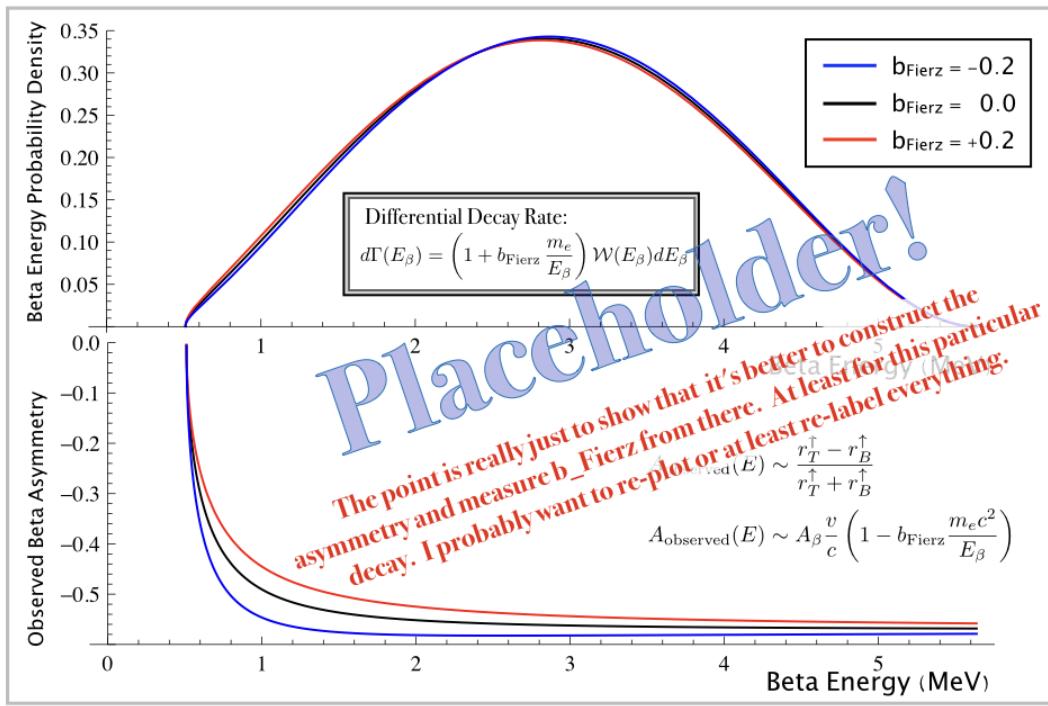


Figure 5.1: Here's why it's better to extract  $b_{\text{Fierz}}$  from an asymmetry, in this case.

# Chapter 6

## Calibrations

I have some tables summarizing which types of data were measured. Presently, that's in Ch-4, but probably I should move it here.

### 6.1 Measurements of the Atom Cloud

Content that previously was in this subsection is now in Section 4.4. But also, see Section 3.5. Probably in this section here, I need to just jump right in assuming everyone understands the physical principle of how the data works. Then, I can talk about what the data *\*is\**, and how it gets interpreted.

Trap position – Measured using the same dataset that was used to quantify the polarization. The trap drifts slightly over the course of our data collection. Describe the rMCP calibration needed to extract this info.

Polarization measurement was conducted on a different set of data, collected in between the measurements used for  $A_\beta$  and  $b_{\text{Fierz}}$ , and at a higher electric field, because we were unable to run both our MCP detectors simultaneously.

Missing figure

Need pictures of the cloud. Possibly need projections of the cloud as a function of time, for AC/OP cycles.

Anyway, here is a nice table describing the atom cloud, for each of 3 runsets, and I'll immediately reference it right now, as Table 6.1:

		Initial Position	Final Position	Initial Size	Final Size
Runset B	$x$	1.77 ± 0.03	2.06 ± 0.08	0.601 ± 0.013	1.504 ± 0.047
	$y$	-3.51 ± 0.04	-3.33 ± 0.05	1.009 ± 0.013	1.551 ± 0.018
	$z$	-0.661 ± 0.005	-0.551 ± 0.021	0.891 ± 0.004	1.707 ± 0.015
Runset C	$x$	2.22 ± 0.05	2.33 ± 0.11	1.18 ± 0.04	1.538 ± 0.087
	$y$	-3.68 ± 0.04	-3.31 ± 0.06	0.965 ± 0.012	1.460 ± 0.030
	$z$	-0.437 ± 0.09	-0.346 ± 0.037	0.927 ± 0.007	1.797 ± 0.026
Runset D	$x$	2.274 ± 0.012	2.46 ± 0.06	0.386 ± 0.016	1.382 ± 0.046
	$y$	-4.54 ± 0.04	-4.28 ± 0.04	0.986 ± 0.08	1.502 ± 0.013
	$z$	-0.587 ± 0.04	-0.481 ± 0.018	0.969 ± 0.003	1.861 ± 0.013

Sig figs here need work.

Table 6.1: Cloud Positions and Sizes – Measured immediately before and immediately following the optical pumping phase of the trapping cycle. All entries are expressed in units of mm, and the “size” parameters describe the gaussian width.

Also, we noticed the trap drifting after one of the runs, because one of the batteries on one of the thingies adjusting the laser frequency (I think) was failing.

JB: “If we rejected the data with the MOT moving (indeed a battery determining the voltage controlled oscillator frequency offset between absorption in stable  $^{41}\text{K}$  cell and the  $^{37}\text{K}$  resonance) then that’s all you need to say.”

describe how you’d turn this into a physical description of the cloud, with like a temperature and a sail velocity and shit. with equations.

## 6.2 The eMCP

I can describe the eMCP calibration here, even though it mostly wasn’t implemented by me. It is tangentially relevant to data selection and background estimation by providing an experimental energy spectrum for shake-off electrons. It’s actually a pretty neat algorithm that I basically wasn’t involved with.

JB: eMCP. You need to describe the timing information obtained. You also need a statement of whether or not you used the position information in your cuts.

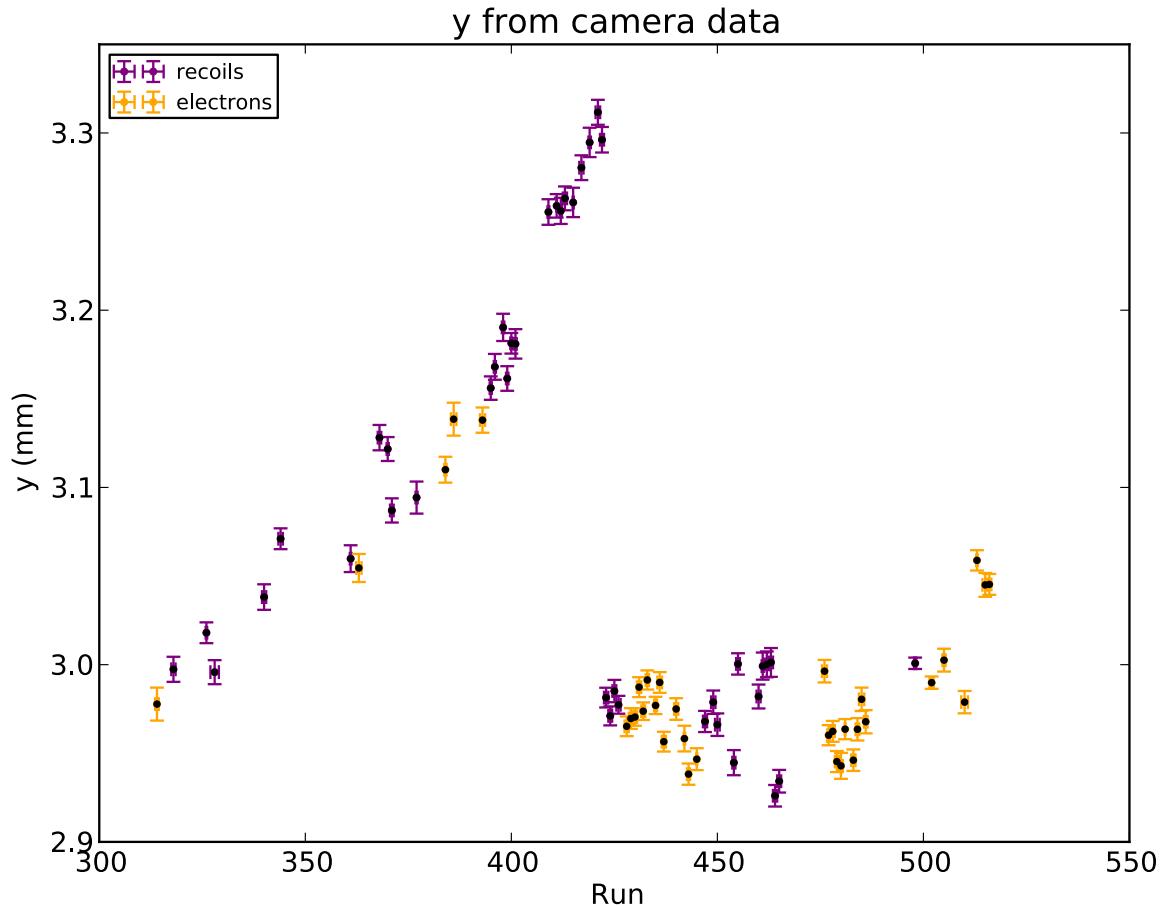
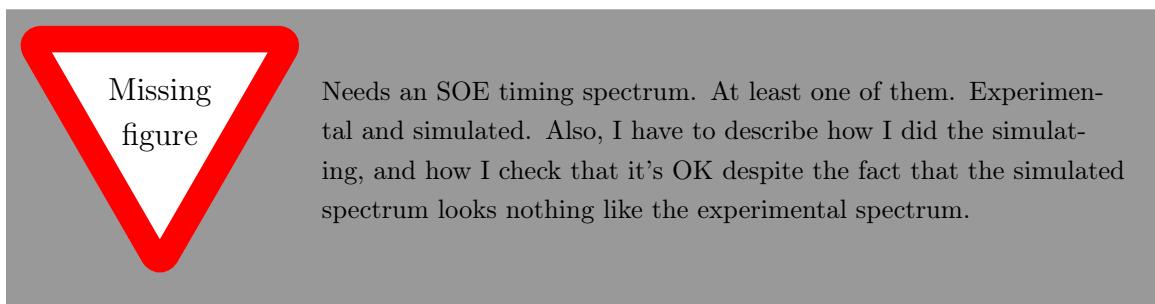


Figure 6.1: Trap Position along the “Time-of-Flight” Axis. Electron runs and recoil runs plotted by run number. (I should probably re-plot this. Maybe combine info with Fig. (6.2).)



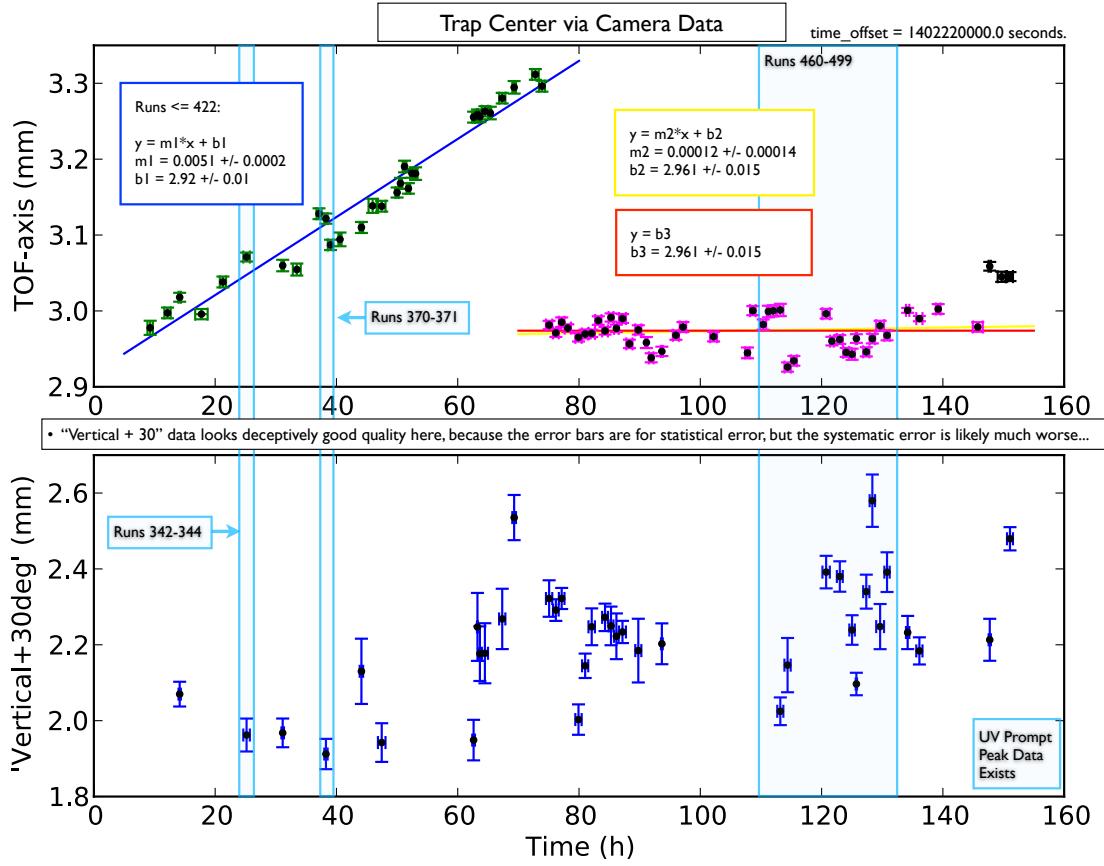


Figure 6.2: Trap Position along the “Time-of-Flight” Axis and the “Vertical+30” Axis. All runs plotted by time of run. (Need to re-plot this.)

## 6.3 The rMCP

I did this, and they’re absolutely needed to make any sense of the trap position data. These calibrations are done during AC-MOT time, and we’re actually interested in the rMCP data taken during OP time. Can I find pictures to estimate the size of the change resulting from the magnetic field? In any case, the change is pretty small.

### 6.3.1 rMCP Bullet Points!

Using the “other” data set with the rMCP: Measure the trap position/size/velocity/expansion with the rMCP and with the camera. Necessitates calibrating the rMCP, which is its own whole thing. Also measure polarization.

- rMCP calibration probably gets its own section, if not its own chapter..

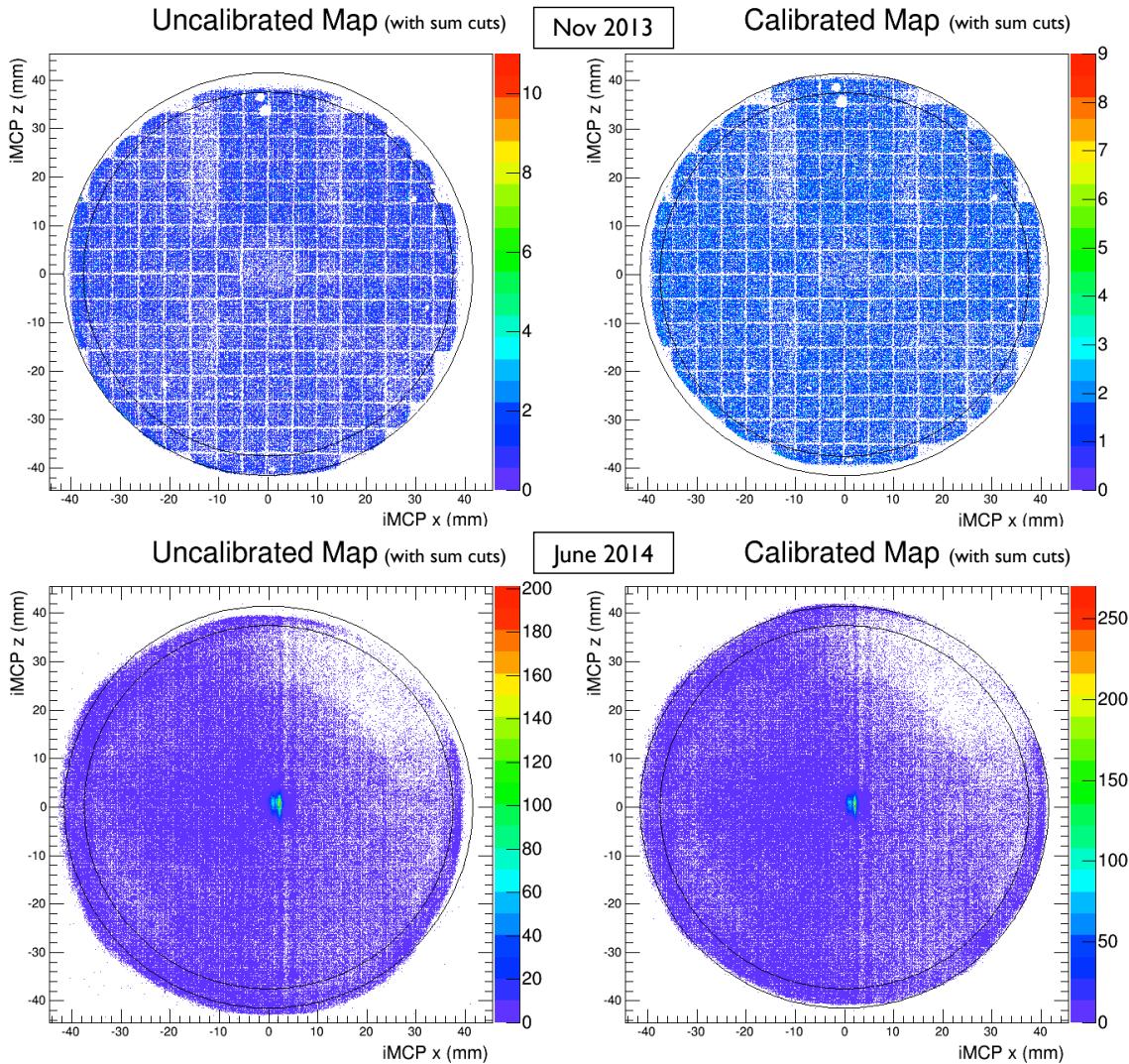


Figure 6.3: rMCP Calibration. Definitely comment about how this calibration went here, in the figure caption. (Do I definitely want a picture with the stupid line-y runs? Maybe it's better to just \*not\*...)

- We took the mask off before the 2014 run, to give us more detector area. Use previous reference calibration *with the mask* during the test run in Nov 2013. The delay line's non-linearities should be the same, assuming we can get the centering the same. Cables have changed and stuff, so we have to re-center the pre-calibration image to where the old pre-calibration image was. ... So, center the new runs w.r.t the old run.
- We'll want to make some sum cuts for these things. We might like them to be

identical, or at least identical-ish, but the peaks don't really look the same. So we'll settle for "decent sum cuts for all!". ... So, apply sum cuts to the new runs and old run.

- Calibrate the old run, with the mask. In fact, I don't remember which order I did things in. But I have a record of it here, somewhere...

# Chapter 7

## Analysis

JB says: “ch 7 looks fine to me.” ...even in spite of the giant paragraph suggestions below, I guess.

Do I actually want to combine this with Chapter 6?

from JB: you ask at the start if you should combine it (Ch.7, ‘Analysis’) with Ch.6 (‘Calibrations’. I see no special reason either way. I would say the topics seem distinct.

JB: Here is what I suggest for the intro the Analysis section: (n.b.: at the time of this comment, that was Ch.7)

...

7.0 Analysis is critical to a precision measurement, as most of the research is in determining systematic uncertainties by self-consistent analysis and simulations. Each detector in this experiment is critical and has independent calibrations and cuts. Full explanation is here (and in two following chapters) justifying choice of the deterministic cuts, because in the analysis in this thesis the data was not blinded. The main goal of blinding was nevertheless achieved— to make sure all analysis is done completely with full redundancy of checks wherever possible— so the discipline entailed must be described in full detail. Here there are details of detectors: eMCP rMCP beta DSSD scintillator.

...

The collaboration has done an independent analysis fixing bFierz to zero. Differences with that analysis are interleaved in this section. Critical physics improvements concern an emcp-beta timing walk correction which enabled an improved cut against background, also incorporating a more complete modelling of decay backgrounds from untrapped atoms. Technical corrections include a correct treatment of the polarization cycle. An arbitrary change in the deltaE radius cut is kept self-consistent.

...

Then Section 7.1...

## 7.1 Data Selection and Preliminary Cuts

Although the detection chamber was designed to feature two MCP detectors on opposing sides of an applied electric field intended for simultaneous use (see Section 4.3), in practice the two detectors produced quite a bit of feedback when operated at the same time. In order to salvage usable data from the beamtime, we ended up running only one detector at a time, but switched which detector was in use every few hours, collecting approximately the same amount of data with each detector. Thus, the runs are sorted into ‘electron’ and ‘recoil’ runs, depending on what the detector in use was intended to detect.

While the beta asymmetry and Fierz interference are best evaluated using the electron runs, the recoil runs are best for evaluating the polarization (a dominant uncertainty in the beta asymmetry measurement) and the cloud position. The polarization measurement is the subject of a recent publication (see [1]), and the cloud position evaluation is discussed in more detail in Chapter 6.

The data is further split up into four runsets: A, B, C, and D based on when certain detection settings were adjusted, and each of these runsets contains both electron and recoil runs. These four runsets were then treated separately for nearly all parts of the analysis. In particular, Data Set A was neglected completely during analysis after it was determined that one scintillator had an improperly set hardware threshold such that lower energy betas weren’t being detected at all. Additionally, there was a QDC module failure between Runsets B and C, resulting in an abrupt change in calibration for the two scintillators.

Also, the background was too noisy in Set A I think. Also, pretty sure there isn’t really that much data in Set A at all. Also-also, Set A has  $E=66.7$ , so can’t use it for more stats on the other backgrounds.

Before proceeding further, several basic cuts are performed on the data. For the Electron Runs which are to be processed directly into a physical measurement, we consider only events in which there was a recorded hit *both* on the eMCP *and* on (at least) one of the scintillators. The required scintillator hit, of course, is potentially a beta, and so it is obvious why this must be present. The eMCP hit requirement – particularly with the timing of the eMCP hit occurring within a certain time range relative to the scintillator hit – is used to tag beta decay events originating from the cloud, as opposed to those originating from some other surface within the chamber.

	Electron Runs	Recoil Runs	OP Delay
Runset A	314, 362, 363, 383, 384, 385, 386, 393, 420.	303, 308-313, 318, 326, 327, 328, 340, 342, 343, 376, 377, 378, 394, 395, 396, 398-402, 409-419.	$300\ \mu s$
Runset B	428-437, 440-445.	421-426, 446, 447, 449.	$300\ \mu s$
Runset C	476, 477.	450, 454, 455, 460-466, 473, 474.	$700\ \mu s$
Runset D	478-489, 502, 503, 504, 505, 510, 513.	491, 497, 498, 499, 509.	$400\ \mu s$

MUST check to make sure I didn't use Run 420 in "good runs" in the end!!!

I have more (really early) runs classed in Set A than Ben had. In the end, we didn't use them so it doesn't matter. But...what?

Ugh. My categorization system is slightly different than Ben's on the later recoils. That's annoying.

Other things I could list here: Electric field strengths, total runtime.

Table 7.1: A list of 2014 online runs with potentially usable data. Runset A was discarded completely due to problems with hardware threshold settings. There was a QDC module failure before Run 450, so it and all subsequent runs were performed using a different module, and as a result the scintillator calibrations changed slightly at this time.

The precise time interval to be used, and how its results should be evaluated, is discussed in Section 7.3, but for the first pass through the data it is good enough to simply require that an eMCP hit occurred. Although not every beta decay event from the cloud will produce a hit on the eMCP, this requirement eliminates a great deal of background that would otherwise be challenging to evaluate.

also discussed: how we decide what counts as an eMCP hit at all

Awkward stupid phrasing.

Also, we claim that it doesn't bias the data. Much. Didn't I try to evaluate how much it biased the data at one point?

Because the eMCP hit is required as a 'tag' of good events, it is also necessary to remove from direct consideration any event which is coincident with a pulse of the photoionization laser. When photoionization occurs within the atom cloud, an orbital electron is removed from the atom and will be accelerated by the electric field into the eMCP, just as a shake-off electron from a decay might be. If, by chance, this photoelectron arrives in coincidence with a scintillator hit, it would be interpreted as a decay event from the trap – unless we preemptively discard it.

Over the course of the runtime, there were several instances where we noted an apparent electrical discharge within the experimental chamber, producing enormous

backgrounds for a short time. The detectors typically recovered quickly afterward, so it was neither necessary nor useful to stop an entire run to wait for the system to recover. Instead, the time when the discharge occurred was recorded, and events within approximately one minute of the spark time were discarded.

We use only the “fully polarized” events for which we have a detailed understanding of the nuclear polarization (described in more detail in [1]). This means we must use *only* events from the “optical pumping” portion of the duty cycle (see Fig. 4.2), and discard events when the DC- or AC-MOT is active. After the AC-MOT is shut off, there is a short delay before optical pumping begins (see Table 7.1) to allow the magnetic field to decay, and it is only after  $100\ \mu s$  of optical pumping that we consider the atoms to be fully polarized. Furthermore, because the magnetic field from the DC-MOT is slow to decay (relative to the field from the AC-MOT), all events from the first five AC/OP cycles after every atom transfer are discarded. A secondary benefit of our insistence on considering only polarized data is that the scintillators’ gains are more stable in the presence of only the (small, stable) magnetic field used for optical pumping than they are in the presence of a larger oscillating magnetic field used for trapping.

change by 0.2% of its value vs change by 0.5% of its value, according to Ben’s thesis pg 143.

Finally, because this analysis depends heavily on energy measurements from the two scintillators as a proxy for beta energy, it is necessary to remove events in which the pulser LED fired. Although the pulser LED is useful for evaluating the stability of the scintillators, in the case where an LED pulse occurs together with a true beta hit in the scintillator, it may change the measured energy. Therefore, we discard all events that include an LED pulse.

## 7.2 Further Cuts Using the DSSD

Pretty sure I’m repeating myself from Chapter ??

JB says: To repeat a comment, since the Appendix on analysis changes is being dispersed throughout, when you state somewhere (I think it's in Ch. 6) make sure you state clearly that the only change for BB1 cuts is the radius cut, e.g. that you took the same T and E from the waveforms (I'm not even sure whether the waveforms are recorded anyway). You ask 'how can I state this' but there's no reason to be subtle. Just say upfront that Ben and Spencer's theses did all the groundwork on the BB1, and here you include selected details needed to understand the present analysis. If you need to include some redundant material, don't worry too much about that.

Although it was not possible to use the DSSD in real-time analysis or event triggering, the DSSDs may be used, after the data has been collected, to distinguish between different types of particles incident on the detector, as more energy will be deposited by heavier particles. When a scintillator hit is triggered by a particle originating within the experimental chamber, that particle will typically have passed through the DSSD before arriving at the scintillator.

In the present experiment, the two primary particles that will concern us are  $\beta^+$  particles originating from the decay of  $^{37}\text{K}$ , and  $\gamma$  rays, which may be produced through a variety of processes, e.g. directly from the 2% decay branch, through annihilation of  $\beta^+$  particles upon their interaction with regular-matter electrons, or bremsstrahlung radiation from emitted  $\beta$ s.

We would like to look specifically at events involving  $\beta^+$  particles arriving direct from a decay within the atom cloud, and the DSSD may be used to eliminate events in which the scintillator is triggered by a  $\gamma$ . An incident  $\beta$  will typically deposit some portion of its energy in the DSSD as it passes through, however an incident  $\gamma$  will deposit significantly less energy; for this setup the energy deposited by a  $\gamma$  is generally indistinguishable from background on the DSSDs. Therefore, we require that a 'good' event must include a 'good' hit to the DSSD as well as a hit to the associated scintillator.

In order to proceed at this point, and because the DSSD readout records so much information, it is necessary to develop some criteria to determine whether or not we will accept any given DSSD readout as a  $\beta$  hit.

How do I \*say\* that Ben was the one who did most of the DSSD calibration stuff? I maybe don't need to describe all of it here, but I \*could\*, and maybe it's needed in order to understand like 4 rows in my error budget.

JB: "You can describe anything you did differently or improved, but you can and should otherwise defer all details of the scintillator calibration and DSSD calibration to Ben's paper and his thesis and Spencer's. E.g. Section 9.2 'statistical agreement between BB1 X and Y detectors' energies only makes a small effect on results" does not need the technical details beyond that statement."

JB: "If you have some way of documenting the coding you used, that would be great." ... yeah, it would, wouldn't it?

We read out the full waveform for every strip at each event with a scintillator hit, but in post-processing take *only* the 'time' and 'energy' from the peak waveform height and the time in the waveform at which that occurs. Each strip will have its own noise spectrum and energy calibration. To classify an event as a good DSSD hit, we require at least one 'x' strip and one 'y' strip record an energy above the noise threshold. We require that the x strip and the y strip agree (to within some number of standard deviations) in amount of energy deposited, and in the time at which that hit occurred. In order to avoid problems resulting from the strips' non-uniform noise thresholds, we further require that the energy deposited be greater than some lower-end cutoff which is selected so as to be higher than every individual strip's noise threshold. In this case, the DSSD's lower energy uniform threshold was set at 50 keV.

I think Ben might have selected 60 keV? That's maybe something for the appendix.

We also elect to use only events where a beta hit the DSSD within a 15.5 mm radius of the center of the detector, so as to avoid scattering effects from the collimator walls.

Did I even mention the collimator? Like, in the previous chapter or something..?

## 7.3 Further Cuts Using the eMCP

I described the HEX-75 somewhere in a previous chapter, right??

The eMCP features a set of three delay lines, intended to be used to record the position of a hit, as in Fig. 7.1. Though only two delay lines is sufficient to determine the position within the plane of the MCP if they are both hit, the presence of a third delay line allows for some redundancy. In practice, however, a large fraction of otherwise 'good' events include a hit on the eMCP, but have insufficient information recorded on the delay line channels to reconstruct a position.

Do I need to describe MCPs and delay lines somewhere? Maybe not...

Because a SOE from the trap is most likely to land in the centre of the plate, while the background from other sources is roughly constant across the plate, it might make

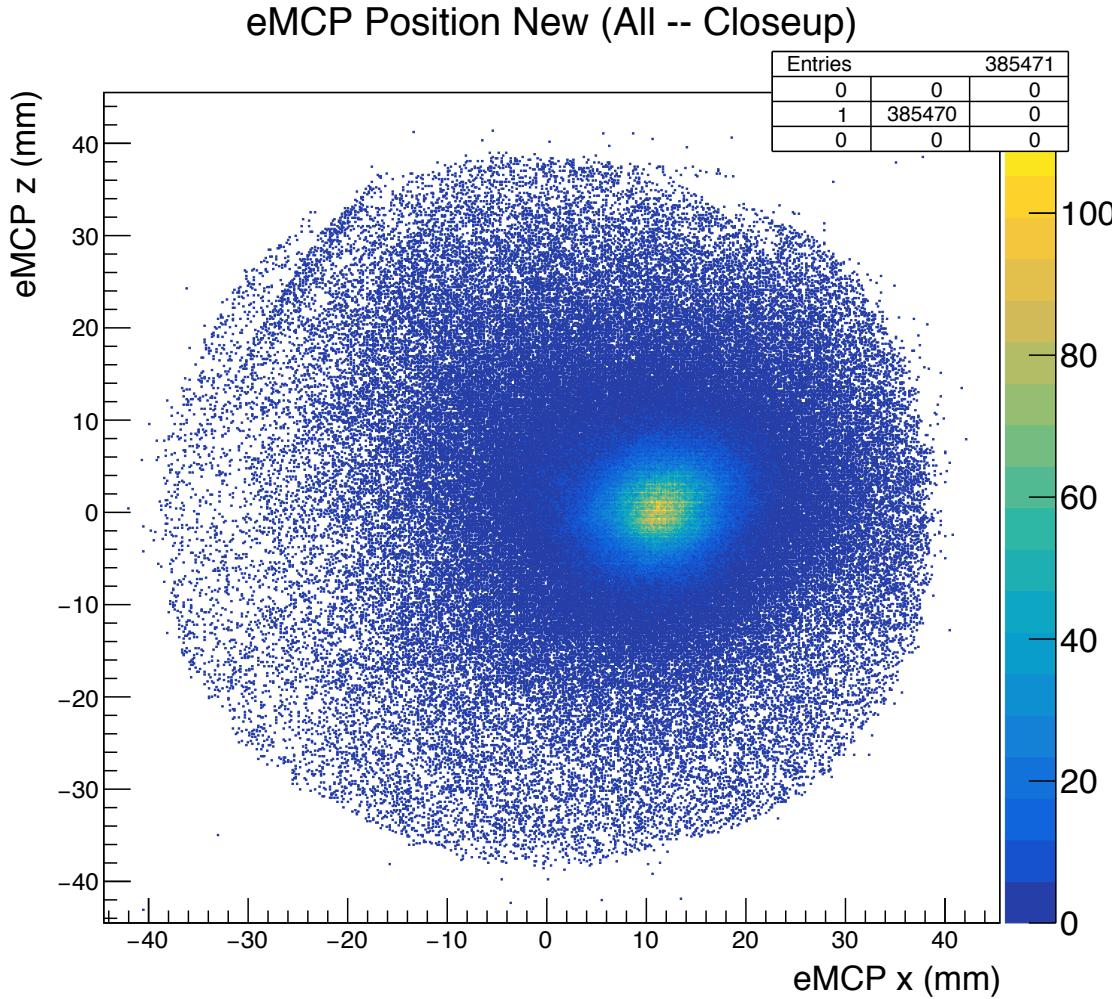
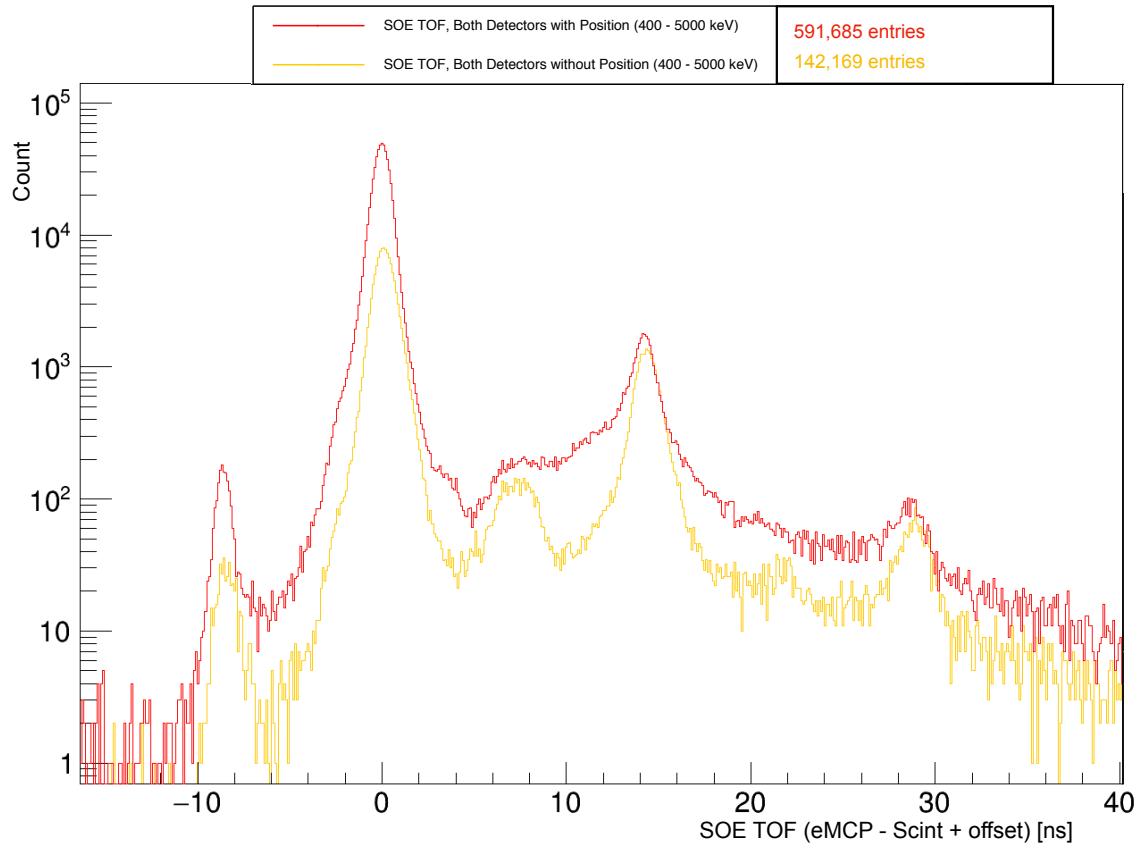


Figure 7.1: Position as measured on the eMCP, after some data cleaning.

sense to accept only events where the eMCP hit is within some radius of the central peak. This methodology was seriously considered because the remaining data has a much lower fraction of background events polluting it – however this results in a loss of around half of the events even for the most generous eMCP radius cuts (see Fig. 7.2). Therefore, it was decided that no position cuts on the eMCP would be made in the final analysis.

Several years after the data was initially collected, a problem was discovered with our low-level analyzer software, which we had been using to convert large and unwieldy MIDAS data sets into somewhat smaller and more manageable ROOT data sets. In particular, for every timestamp recorded, our raw MIDAS data actually included both



Um. Did I for sure get the labels correct on this??? It seems really wrong.

Figure 7.2: Beta-electron TOF, for events with and without eMCP hit position information. A cut will eventually be taken to accept only events sufficiently near the largest peak – in this case the number of events is ‘only’ decreased by a factor of 2.

a timestamp for the leading edge (LE) of the pulse, and a timestamp for the trailing edge (TE). The analyzer had—for years—been reporting the timestamp associated with the trailing edge of the pulse. Initially it was unclear if there might have been a reason behind this choice, but a closer examination of the data showed less timing noise and a sharper distribution of timing pulses across the board (see Fig. 7.3), with some channels showing a larger effect than others. This was corrected, and the entirety of this analysis has been performed now using the cleaner LE spectra.

The place where this change between the TE and LE timestamps had the biggest impact on the analysis is in the shake-off electron time-of-flight spectra, on which a cut must eventually be taken. Although this problem was not discovered in time

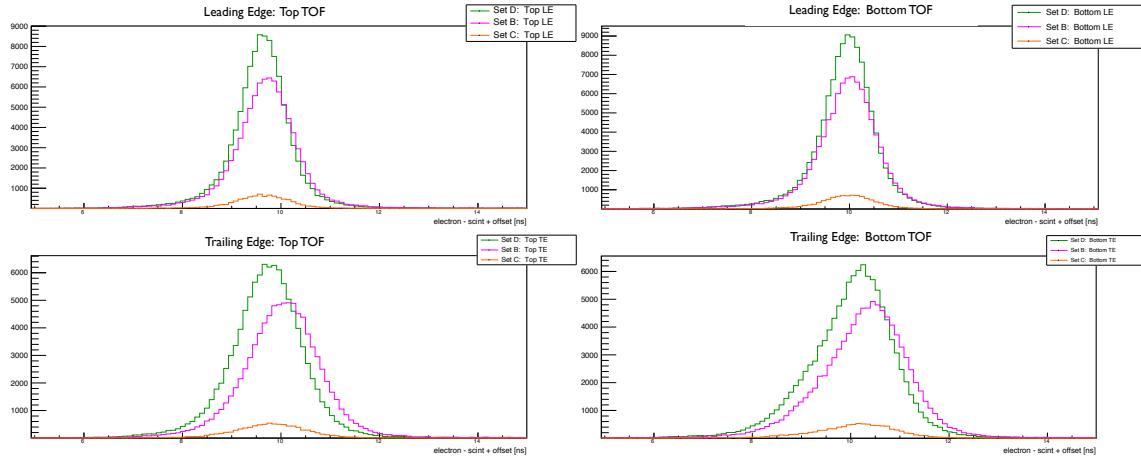


Figure 7.3: SOE TOF peaks (eMPC - Scintillator), using the leading edge (LE) and using the trailing edge (TE). Data is sorted according to runset. For each individual runset, the TE peak is broader than the LE peak. The centroid of each runset is also more variable in the TE plots.

to be used in the previous measurement of  $A_\beta$  using this same data [26], it likely would have had a negligible effect on the final result, because the SOE TOF cut that was used there was comparatively loose, and the evaluation of the background that remained was not a dominant systematic effect.

With the data reprocessed using the leading edge for timestamps, I wanted to eliminate as much background as possible from the SOE TOF spectrum. With this goal in mind, the next step was to correct the scintillator timing for its low energy ‘walk’ (see Fig. 7.4). A quartic polynomial was fit to each of the 2D timing vs energy spectra (the top and bottom detectors were treated separately), and the result was used to produce a ‘straightened’ SOE TOF spectrum with respect to measured scintillator energy, and as expected, the resulting SOE TOF spectrum was a bit more sharply peaked.

This goes in that one appendix, if I haven't already put it there.

At this point it becomes necessary to attempt to model the SOE TOF spectrum, as in Fig. 7.5. To check the agreement of the model with reality, we compare the averaged superratio asymmetries from both, as in Fig. 7.6.

Okay. This needs more description.

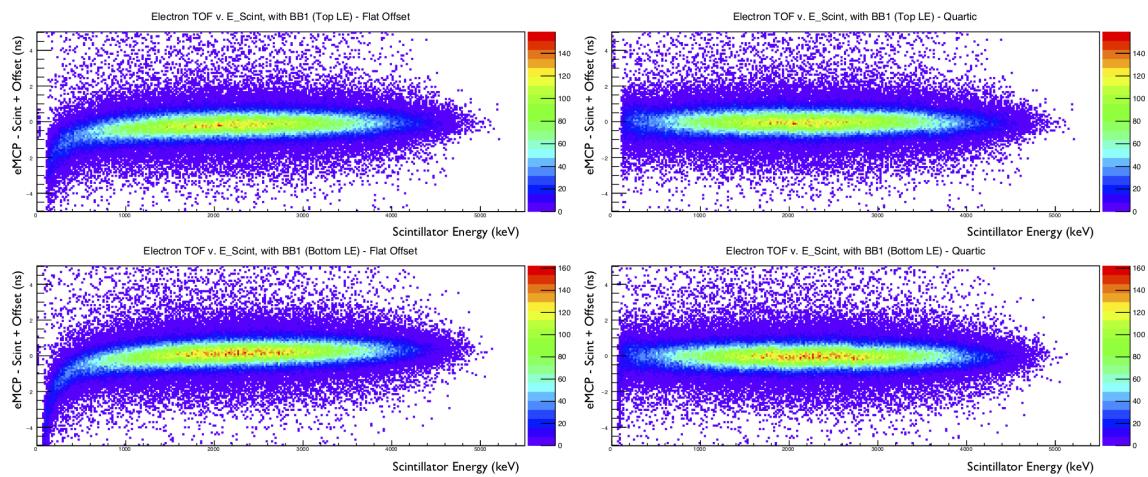


Figure 7.4: SOE TOF walk, before (left) and after (right) applying a quartic adjustment to straighten out the effective TOF.

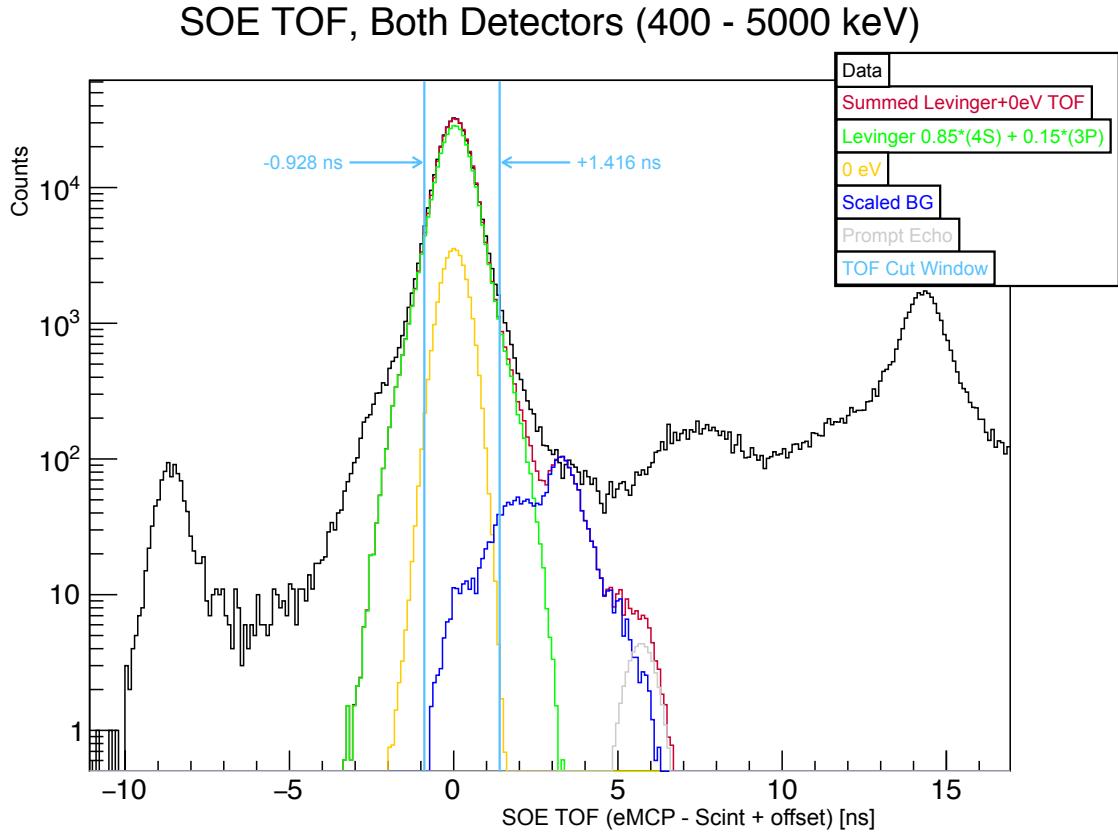


Figure 7.5: SOE TOF, model and data. In the end, I cut the data to use only events with a TOF between -0.928 ns and +1.416 ns. Max. possible background is like a factor of two too big. Similar quality results no matter how you distribute the Levinger spectra between 4S and 3P, however adding the 0 eV electrons makes a big improvement to the agreement.

## Superratio Asymmetry

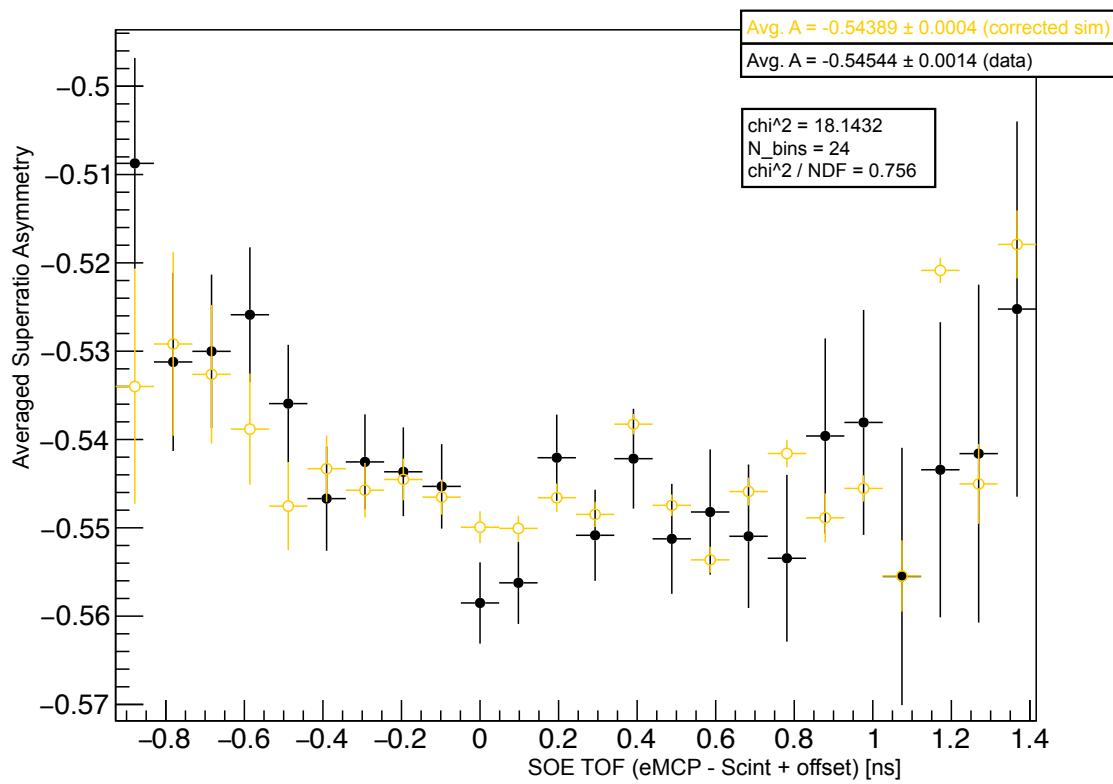


Figure 7.6: The superratio asymmetry, averaged over all scintillator energies between 400-4800 keV, is used to compare the experimental data and simulated TOF model. All other cuts have been applied.

# Chapter 8

## Simulations

JB: "I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now."

Do I want this in a separate chapter? Or maybe I want it to be combined with the stuff on data selection and analysis. Surely that chapter is getting too big even without this stuff.

- Multithreading. There was multithreading, and it took a stupidly long time to get working.
- Update G4 event generator to be able to model non-zero scalar and tensor coupling. These things show up in  $A_\beta$  too, not just in  $b_{\text{Fierz}}$ . Though, the effects on  $A_\beta$  are much smaller.
- Run 3 sets of G4 simulations with a bunch of statistics (N events, for data with like N/10 events). Each one has the same nominal value of  $A_\beta$ , but with 3 different values of the scalar coupling  $C_S$ : zero, and +/- (whatever). Keep  $C_T = 0$ . Because reasons, we're not really able to distinguish between  $C_S$  and  $C_T$  in this experiment anyway, so might as well keep the analysis simple.
- Just run one set of 0.02\*N events for the two percent branch. We can't neglect it, but it isn't going to change (much?) when we adjust BSM couplings.
- Match cuts in simulated data up to the cuts on experimental data. Obviously. DSSD cut, DSSD energy, one hit DSSD, one hit scint. TOF cut, which requires a whole extra model of background in the TOF spectrum..
  - Suppose background in the TOF spectrum is coming from decays of atoms that have gotten themselves stuck to surfaces within the chamber...

- Run G4 to get a beta TOF spectrum (w.r.t. the decay)
  - Run COMSOL (credit to Alexandre) to track low-energy SOEs through the electric field from wherever they started, into the detectors. Energy spectra from Levinger.
  - Combine G4 and COMSOL spectra, event-by-event, while requiring that both the beta detector and the eMCP are hit according to the set of random numbers generated by each monte carlo separately. Then, the beta and SOE will each have a TOF from decay to detector, and subtracting one from the other gives a timing spectrum that can be observed experimentally. See Fig. 7.5.
  - Upper limit for the fraction of events generated this way can be estimated by assuming that all losses from the trap not due to radioactive decay emerge isotropically from the trap and then stick to whatever chamber wall is in its path. This upper limit is too big by a factor of 2.
- For each of those 3 simulations, sort the “good” data according to emission angle relative to the detector. Do each detector individually. For both polarizations.
  - Assemble the (simulated) superratio asymmetry. We’ll compare it to data, and the  $\chi^2$  from that comparison will be our figure of merit.
  - We can make a whole 2D parameter space for different values of  $A_\beta$  and  $b_{\text{Fierz}}$ , and compare them all (via their superratio asymmetries) to the experimental data. We get the “best” values of  $A_\beta$  and  $b_{\text{Fierz}}$ , where  $\chi^2$  is minimized.
  - We can do this whole thing again for simulated data sets with different values of parameters that we vary as systematics. Note how the best values of  $A_\beta$  and  $b_{\text{Fierz}}$  change when each of the systematics are varied.
  - Then there’s the lineshape thing. My god, this wasn’t nearly as useful as I thought it was going to be. Certainly not worth the whole goddamn year that I spent on it.

# Chapter 9

## Estimating Systematic Effects

JB: "I doubt I will have further useful comments on the Ch. 5, 8, 9 as they are now.

I've tried to email you the paragraphs on "collaboration determination of uncertainties" for Ch. 9.

My intent of all that other advice was to keep your time spent on Chs 1-4 concise, so you could concentrate on these real jobs. (n.b.: the advice he's already sent was almost all about chapters 1-4, which are the various intros/background info and experimental setup stuff)

...

I can only say that if you have an equal choice between including a detail or not, pick "not."

JB: I will try to schedule meeting with Dan for you to show us the final version of Ch. 9 Estimating systematic effects soon.

Do I want this chapter combined with the analysis chapter? If that chapter includes all the stuff about G4, it's going to be unwieldy and huge.

JB says:

A simple estimate from the collaboration that builds intuition for this result: Scatter in the SiC mirrors and DSSD actually produces an efficiency change at low beta energy. Energy loss is not minimally ionizing in these structures, and instead will have a long Landau tail that can take events below energy threshold in the scintillator. The collaboration has modelled explicitly the false asymmetry as a function of Kbeta between 600 and 1300 keV, producing roughly  $(K-0.6 \text{ MeV})/(0.7 \text{ MeV})$ , i.e. 50% at  $K\beta=0.95 \text{ MeV}$ . This efficiency degradation would be distributed roughly equally between the SiC and DSSD. If completely ignored, this would introduce by inspection a false bFierz of approximately 0.5. Scattering effects will vary between linear and sqrt of thickness, so assuming worst case of linear, the mechanical thickness uncertainty of 5 micron/300 micron and 6 micron /275 micron, an average of 2%, making a random contribution of order 0.01 each. The Be window has larger mechanical thickness uncertainty of 23micron/229 micron, but energy loss and scattering in this material is 5x smaller, so the net effect would be similar.

...

To minimize this systematic for future experiments, the collaboration has implemented pellicle mirrors of negligible thickness, 100 nm Au on 4 micron kapton. The collaboration is also implementing Be-windowed wire chambers in place of the DSSD.

...

MJA: ....huh?

## 9.1 Overview

A summary of systematics goes here. In words, yes, but also in table form.

Choice of low-energy scintillator threshold has a large systematic effect...

from John: "I used Ben's threshold when determining the uncertainty from the lineshape tail (UFTLT). If you're saying the UFTLT depends on the threshold used, ok, of course it does. But if you're claiming that UFTLT depends on the \*\*uncertainty\*\* of the threshold, that's manifestly smaller than the UFTLT itself, and I'm going to assert it isn't worth evaluating."

It's actually not nearly as big as I'd originally expected. It's huge in the line-shape thing, but pretty tiny in everything else.

## 9.2 BB1 Radius, Energy Threshold, Agreement

BB1 radius cut can help to eliminate scattered events. Energy threshold selection and statistical agreement between BB1 detectors' energies only makes a small effect on results. BB1 radius itself has a pretty big effect on the result, but we can at least just G4 it away. The remaining systematic effect is pretty small.

Source	$b_{\text{Fierz}}$		$A_\beta$	
	Uncertainty	Correction	Uncertainty	Correction
Scintillator Calibration	0.003	-	0.0003	-
Scintillator Threshold	0.004	-	0.0004	-
DSSD Individual Strip SNR	0.006	-	0.0007	-
DSSD Energy Agreement	0.005	-	0.0006	-
DSSD Detection Radius	0.006	-	0.0017	-
DSSD Energy Threshold	0.005	-	0.0005	-
Atomic Cloud	0.002	(...)	0.0002	(...)
Background	0.004	(...)	0.0003	(...)
Beta Scattering	0.031	(...)	0.0025	(...)
Low Energy Tail	0.008	-	0.0007	-
Mirror Thickness	0.026	-	0.0025	-
DSSD Thickness	0.026	-	0.0025	-
<b>Be Foil Thickness</b>	<b>0.026</b>	-	<b>0.0025</b>	-
Total Systematics	0.056	-	0.0055	-
Statistics	0.084	-	0.0082	-

Table 9.1: Error Budget goes here.

JB: I hope the discussion is clear in your head. Any effect that relies on scattering computation in G4 should have an uncertainty on order 10% of the correction – hopefully you are keeping a distinction here between the finite geometry acceptance (which I guess is exact) scattering off the collimator.

As per JB's comment in section 7.2: "statistical agreement between BB1 X and Y detectors' energies only makes a small effect on results" does not need the technical details beyond that statement."

Missing figure

Surely this requires at \*least\* one image of the pixelated BB1 data. Maybe some of a few waveforms and energy distributions too. ....Feels like cheating to include some of that stuff, since Ben was the one who actually used it mostly.

JB on missing figure: "if you used such an image as part of your uncertainty estimate, yes [include it]"

Remember: There's noise applied to simulated BB1s, matching some spectrum.

In the end, we get our results from the scintillator energy only, without summing

the BB1 energy back in. Energy absorbed in DSSDs is only used as (a) a tag for events, and (b) contributing to the total beta energy loss before the beta arrives at the scintillator.

JB: The simulations of course include it event-by-event, not just a minimally ionizing average loss.

### 9.3 Background Modeling – Decay from Surfaces within the Chamber

So many surfaces, all of which can get stray 37K atoms stuck to them. Then they decay from a place that isn't the actual trap center, and it contaminates our stuff.

JB on figures that might go here: Figure 6.4 (currently that picture of the TOF spectrum) could either be here, or you could reference it from here. The TOF histogram is a great start. Adding the asymmetry[TOF] indeed would be vital.

Missing figure

Show the "average asymmetry" (all energies) as a function of TOF, with real data, best model normalization, and extrema of model normalizations. Show our cut. Turns out, it's a lot of work for a really tiny correction. Oh well.

JB on the \*actual\* figure I had been planning to put here, and my remarks about it: Indeed it will be critical to show a clear compelling version of this figure in thesis and in a paper. It was vital to minimize and determine this background to avoid fitting a polynomial to it from the wings, even more so for the energy dependence of A than for its average – you should say so.

...

The reason the correction is small is because of all your hard work.

We model the beta TOF from the surfaces in G4, event by event. This is necessary because scattered events will have their TOF changed to account for a longer beta pathlength, and we're preferentially cutting away the events that don't have a TOF in the appropriate range. ....And then have COMSOL generate electron TOFs for SOEs starting from the start points picked by G4. Ran COMSOL for 0 eV SOEs, and again for Levinger spectrum SOEs. Used 9% 0eV SOEs in the end. I forgot which Levinger distribution I used in the end. The point is, for each event, you've simulated a beta TOF that may or may not be scattered off of something before

JB: "I wouldn't call these "scattered" events... that's very misleading."

...  
Yeah, I should really stop doing that.

...  
Wait no, scattered is what I mean! But fine, my phrasing is really unclear.

JB: Please comment on whether or not it was important to have this energy distribution.

it hits a detector, and you have a SOE TOF for an event originating at that same point, so you subtract them to model the TOF you'd measure in an experiment. Also, because you've done the scattering with G4, you get the beta energy corrected for any scattering that happened. This way, one can estimate how many "bad" events are eliminated with the TOF cut, as well as the fraction of "bad" events left in what remains.

## 9.4 Quantifying the Effects Backscatter with Geant4

Beta decay (back-)scatter from surfaces within the experimental chamber is a significant systematic, and it must be evaluated, quantified, and corrected for. This is done via a series of GEANT4 simulations. While only a small fraction of events are affected, the process results in an energy loss in the beta that can, if not understood, be misinterpreted as the exact signal we're searching for. It is therefore imperative that this be well understood.

JB says: Please discuss this at the next meeting. (ETA: Done!)

Indeed this is why you should avoid calling the events originating not from the trap 'scattered events.' More importantly, why it was so critical that you reduced the size of the correction by timing bad events out. I would say you have a well-determined TOF cut to minimize this error—a cut that could not have been done blind without an unreasonably perfect simulation. Thus the exact spot of the cut should not be considered to introduce a systematic.

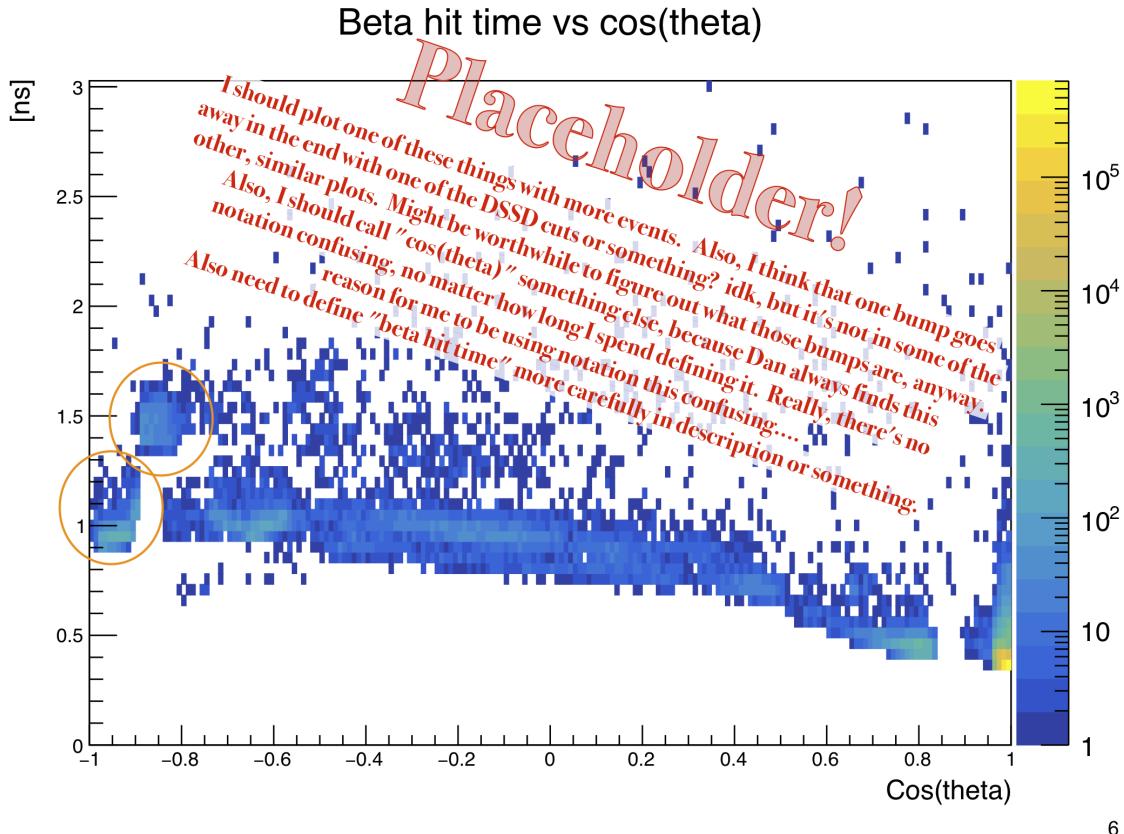
Oh god. Have I even tried to quantify the combined systematic that comes out of the TOF cut? Do I need to, or is it double-counting? Ugh, it would be such a headache to do this. Maybe I can at least do it at the end – because I might never get my code back to the way it was.

## 9.5 Lineshape Reconstruction

This section should reference Clifford. [8].

### 9.5.1 Motivation

This process is used because the (back-)scatter, which it itself an important systematic, is largely independent of a wide variety of other experimental effects. These other effects must all be evaluated, but it is computationally prohibitive to re-evaluate the scattering with every other effect under consideration.



6

Figure 9.1: Simulated Beta TOA vs emission angle w.r.t. detector orientation

### 9.5.2 What is it and how does it work?

Mono-energetic beta decay events are generated in GEANT4, which outputs an energy spectrum for unscattered and forward-scattered beta events in the detector. These spectra are fit to a function to model the scintillator resolution, as well as energy loss in materials that the beta passed through before arriving at the scintillator. These spectrum fits are performed for a set of beta energies, and parameters are extrapolated to be applied to betas emitted at intermediate energies. Thus, the whole spectrum can be modeled. Pictures will make this clearer.

### 9.5.3 The Math-Specifics

I'll write down the specific functions I'm using, and the parameter values I'm using. (Maybe this should go in an appendix instead?) I'll describe the adjustments I

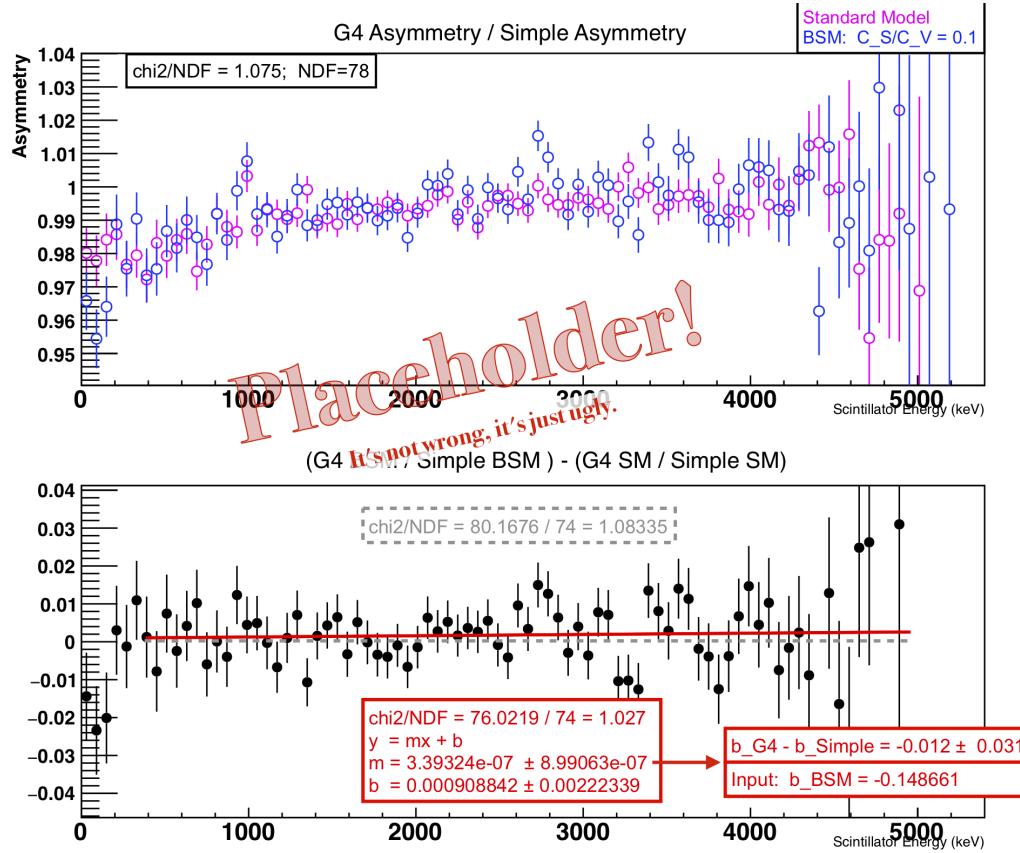


Figure 9.2: I'm not actually sure if this picture shows what I want it to. The point is, if I apply this rough lineshape to stuff that I SimpleMC-ed, then I can evaluate that way various systematic effects that would be time-consuming to actually simulate with G4. This picture is \*supposed\* to be a demonstration that this approach actually works...

make to the spectrum so that it can work even for the dataset where the scintillators' resolutions have changed.

#### 9.5.4 The Results – Things That Got Evaluated This Way

As it turns out, only cloud parameters were evaluated this way. Trap position, size, sail velocity, temperature. But then we varied the lineshape anyhow, to account for G4 doing a bad job of modelling the bremsstrahlung (sp?).

JB: so it's still critical to write down more of the lineshape work.

JB: yes, brems strahlung is 'braking radiation' so gets 2 ss's. the lineshape tail in any scintillator also includes backscattered events – we are not claiming the 2-pixel cut is complete

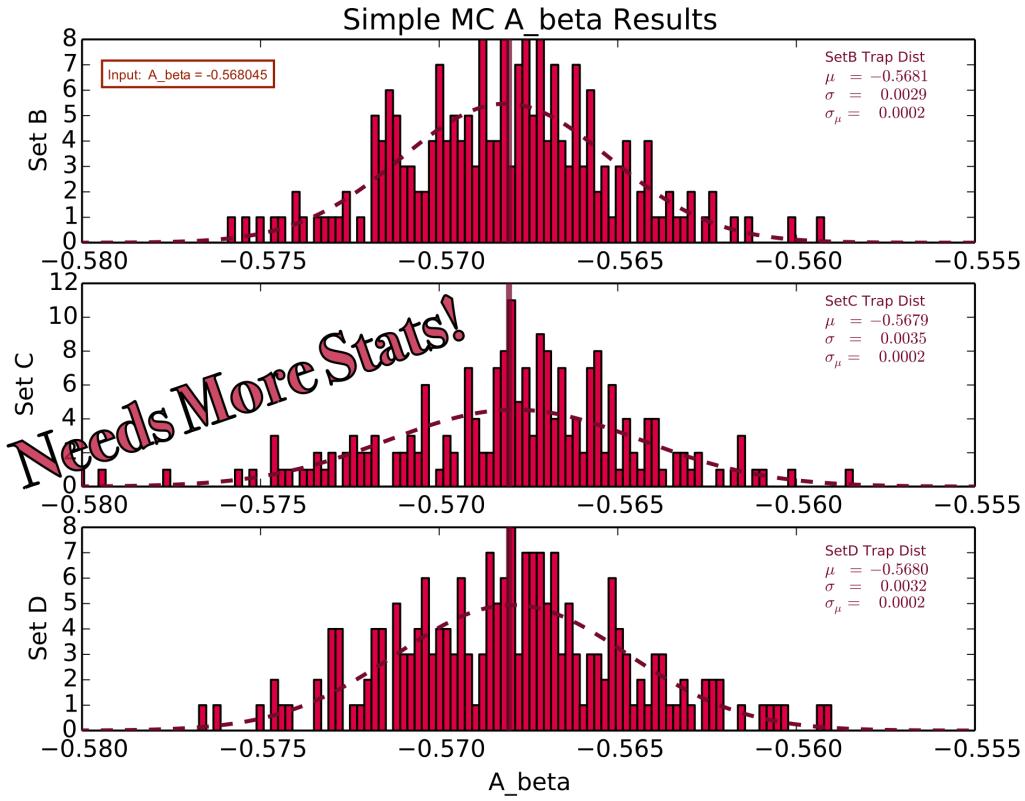


Figure 9.3: Estimated uncertainty in  $A_\beta$  resulting from uncertainty and variation in the cloud parameters. Evaluated by the lineshape reconstruction method.

### 9.5.5 The low-energy tail uncertainty, and what it does

Bremsstrahlung. It does Bremsstrahlung.

JB: "I will write this up better soon." (I think he already did that)

Here is Subsection 9.5.5 "The low-energy tail uncertainty, and what it does" complete. There should be no figure. You cite the Clifford paper now in other sections— please check if my [] is right. (I cc: everyone as my summary of the situation that I showed Dec 1, 2021 at tri-nat.triumf.ca)

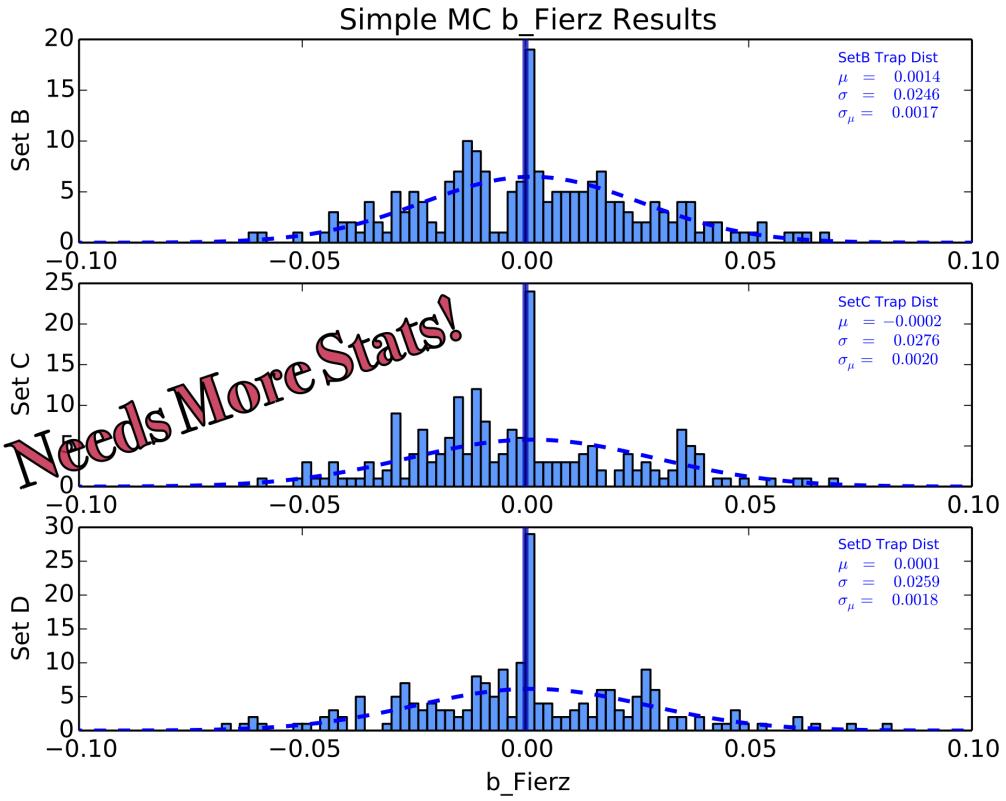


Figure 9.4: Estimated uncertainty in  $b_{\text{Fierz}}$  resulting from uncertainty and variation in the cloud parameters. Evaluated by the lineshape reconstruction method.

\*\*\*\*\*

This subsection has the collaboration's evaluation of the uncertainty from the scintillator detector's lineshape tail. The energy from a monoenergetic beta is not always fully absorbed in a plastic scintillator. Although most backscattered betas are vetoed by the DSSD, some produce bremsstrahlung photons, and these frequently escape low-Z plastic scintillator— all cross-sections are known to high accuracy, but there is always uncertainty entailed in the MC implementation. This lineshape tail will then effectively move events from higher to lower measured energy, artificially altering the lower-energy asymmetries and mimicking the effects of a Fierz term.

...

Since this detector effect is difficult to disentangle from the other scattering effects off volumes, the collaboration adds a linear function down to zero for the tail to a Gaussian for the peak, with linewidth varying by photon statistics [? ]. The convolution of this simple detector response function with v/c then scales the centroid MC, with the lineshape tail varied by  $\pm 10\%$  of its value, a generic uncertainty accepted by the community for MC electromagnetic simulations. The fit  $b_{\text{Fierz}}$  centroid changes by  $\pm 0.0076$ , summarized as the 0.008 “Low Energy Tail” in the systematics table at the start of this chapter. Compared to other uncertainties of the present data set, this is small enough that the accuracy of this estimate is adequate.

# Chapter 10

## Results

JB: Dan and I independently discussed Ch 10 yesterday, and he has suggestions to help. So I will also schedule a meeting with Dan and you to discuss Ch 10 Results and whether the S,T part must be deleted and left to a paper. You don't have enough time, and although this should be quite straightforward, it is not your critical result and it's the only thing that can go.

### 10.1 Measured Limits on $b_{Fierz}$ , $C_S$ , $C_T$

Results go here, with measured limits described and quantified in all formats anyone could ever care about.

### 10.2 Discussion of Corrections and Uncertainties

...

### 10.3 Relation to Other Measurements and New Overall Limits

In which I'll show exclusion plots and write down new limits, combining my result with results from the literature. Or, y'know, maybe I'll just talk about doing that.

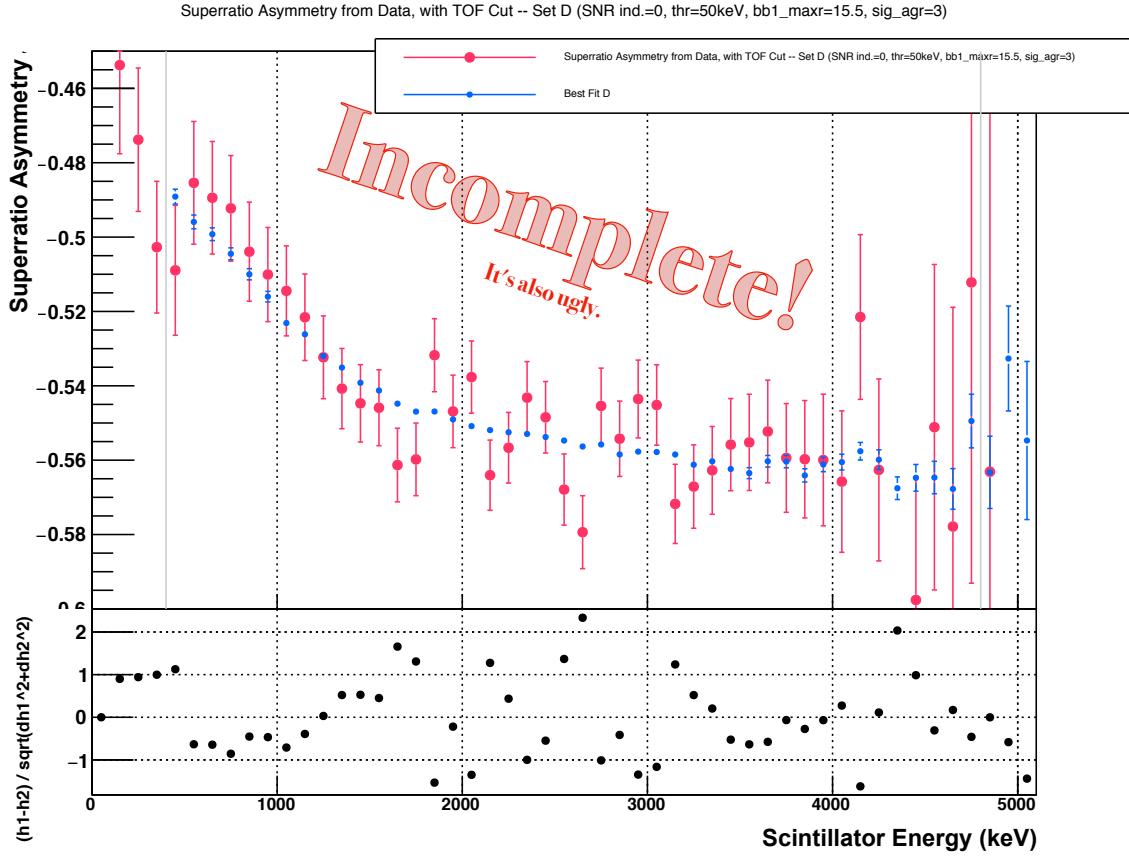


Figure 10.1: A superratio asymmetry from the data, and the best fit from simulations.

JB says: To put your work in context, please add at the end of that minimal S,T section, or at the end of "Our Decay" section

...

The best existing measurement of  $b_{\text{Fierz}}$  is in the decay of the neutron [9],  $b_{\text{Fierz}} = 0.017 \pm 0.021$ , consistent with the Standard Model prediction of zero. Our measurement is strongly related, yet complementary. In terms on non-Standard Model Lorentz current structures, to lowest order in the non-SM currents the same equation applies:

$$b_{\text{Fierz}} = \pm (C_S + C'_S + (C_T - C'_T)\lambda^2)/(1 + \lambda^2)$$

(the plus is for  $\beta^-$  decay and the - for  $\beta^+$  decay) [10]. [to be continued...]

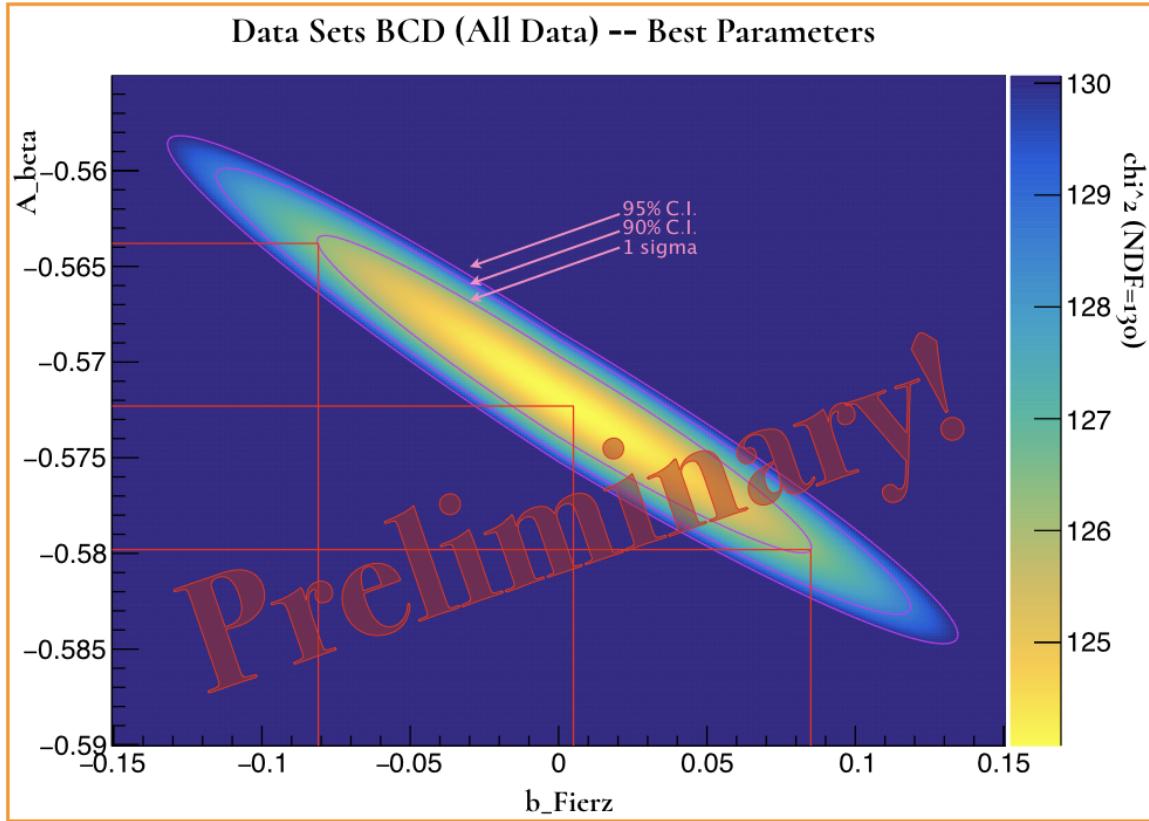


Figure 10.2: Some results. I'll want to show at least one of these things. Probably show a separate one for each runset, actually.

[...continued from prev.]

In our  $^{37}\text{K}$  case,  $\lambda^2 = |M_{\text{GT}}|^2/|M_F|^2$  is close to  $3/5$  (the expected value  $j/(j+1)$  for a single  $j=3/2$  d $3/2$  nucleon) [11], while for the neutron  $\lambda^2$  is close to  $3$  (the expected value for an  $(j+1)/j$   $j=1/2$  s $1/2$  nucleon).  $|M_F|$ , the Fermi matrix element, is nearly the same for both of these isospin =  $1/2$  decays (the largest correction is the larger isospin mixing of  $\sim 0.01$  in  $^{37}\text{K}$ ). So our observable is relatively less sensitive to Lorentz tensor currents, and will predominantly constrain or discover Lorentz scalar currents.

...

Full considerations would require a weighted fit of  $b_{\text{Fierz}}$  experiments and similar observables [12], and are beyond the scope of this thesis. The info from this thesis, values of  $A_\beta$  and  $b_{\text{Fierz}}$  with their uncertainties, can together with the known  $fT$  value (lifetime and branching ratio) allow the community and/or the collaboration to include the results in a future constraint or discovery of scalar and tensor Lorentz currents contributing to  $\beta$  decay.

## 10.4 Conclusions and Future Work

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The citation format I'm using is really stupid. You **must** force yourself to ignore this right now,  
Melissa!

# Appendix A

## Notable Differences in Data Selection between this and the Previous Result

JB says Appendix A should all go in the analysis section, and not in an appendix at all.

JB says: Appendix A (ie, \*this appendix\*) is very important, and should at least be a subsection in the Analysis chapter.

...

You could condense the Appendix into a set of bullet points at the end of the intro to the Analysis section (which you still need, badly!), and then its content could be interleaved in the Analysis chapter. E.g. you already have redundancy in the LE and TE discussion vs. the Appendix, and the discussion is more complete in the Analysis chapter, which is good.

### A.1 Polarization Cycle Selection

Data used for our recent PRL article was slightly less polarized than we thought it was, due to an oversight in the data selection procedure.

### A.2 Leading Edge / Trailing Edge and Walk Correction

Using the leading edge rather than the trailing edge to mark the timing of TDC pulses cleans up jitter, eliminates background, and changes the relative delays between different inputs. It is immediately relevant to the shape of the ‘walk correction’ on

scintillator timing pulses, which give a different prediction for beta arrival time as a function of scintillator energy.

### A.3 TOF Cut + Background Modelling

A SOE-beta time-of-flight cut is necessary to reduce background. The above mentioned walk correction directly results in a change in which specific events are selected in a given TOF cut. It further results in an adjustment to the expected fraction of background events in any such cut.

### A.4 BB1 Radius

Possibly my default radius cut on the DSSDs is a bit different. The region of the parameter space that I'm taking for the systematic uncertainty on this is definitely a bit different.

Somebody will surely ask for a justification for why I did this differently, and I don't have one beyond "this seemed more reasonable to me", which is of course nobody will ever accept as a reason.

## Appendix B

# A PDF For The People

John says to keep this appendix, because it's great now.

JB: Appendix B is in good shape. "someone other than me" please change to "the collaboration."

### B.1 JTW

Here's a master equation from JTW to describe beta decay kinematics [10], [20]:

$$\begin{aligned} d^5\Gamma_{\text{JTW}} \equiv & \frac{F_{\mp}(Z, E_\beta)}{(2\pi)^5} p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d^3\hat{\Omega}_\beta d^3\hat{\Omega}_\nu \\ & \times \xi \left[ 1 + a_{\beta\nu} \frac{\vec{p}_\beta \cdot \vec{p}_\nu}{E_\beta E_\nu} + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + c_{\text{align}} T_{\text{align}}(\vec{J}) \left( \frac{\vec{p}_\beta \cdot \vec{p}_\nu}{3E_\beta E_\nu} - \frac{(\vec{p}_\beta \cdot \hat{\vec{j}})(\vec{p}_\nu \cdot \hat{\vec{j}})}{E_\beta E_\nu} \right) \right. \\ & \left. + \frac{\vec{J}}{J} \cdot \left( A_\beta \frac{\vec{p}_\beta}{E_\beta} + B_\nu \frac{\vec{p}_\nu}{E_\nu} + D_{\text{TR}} \frac{\vec{p}_\beta \times \vec{p}_\nu}{E_\beta E_\nu} \right) \right] \end{aligned} \quad (\text{B.1})$$

where, for convenience, we have defined a nuclear alignment term,

$$T_{\text{align}}(\vec{J}) \equiv \frac{J(J+1) - 3\langle(\vec{J} \cdot \hat{\vec{j}})^2\rangle}{J(2J-1)}. \quad (\text{B.2})$$

Note that this master equation depends on neutrino momentum, which we cannot observe directly. Furthermore, we cannot reconstruct neutrino momenta in our

We have already specialized to  $\beta^+$  decay.

decay events either, because it would be necessary to account for the momentum of the recoiling daughter nucleus, treating the decay as a three-body problem. From an experimental standpoint, we failed to measure the momenta of the daughters in conjunction with the “tagged” beta decay events with which we are primarily concerned in this thesis. From a theoretical standpoint, JTW has intentionally neglected recoil-order terms – meaning that the daughter nucleus is treated, for the purpose of kinetic energy calculations, as being infinitely massive, and as such it must have no change in kinetic energy from the decay. This approximation makes it a bit tricky to correctly re-formulate Eq. (B.1) in terms of the momentum of the daughter instead of the momentum of the neutrino.

Fortunately, it is possible to simplify Eq. (B.1) by integrating over all possible neutrino directions, such that the result no longer depends on parameters that we do not observe. The neutrino energy itself is not a free variable in this equation, because the energy release in the decay is fixed, and given the approximation that none of that energy is allocated to the recoiling daughter, it is very straightforward to calculate the neutrino energy for a decay event in which the beta energy is known.

The result of performing this integration over neutrino direction is:

$$\begin{aligned} d^3\Gamma dE_\beta d^3\hat{\Omega}_\beta &= \frac{2}{(2\pi)^4} F_\mp(Z, E_\beta) p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d^3\hat{\Omega}_\beta \xi \\ &\times \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \left( \frac{\vec{J}}{J} \cdot \frac{\vec{p}_\beta}{E_\beta} \right) \right], \end{aligned} \quad (\text{B.3})$$

which is a great simplification on Eq. (B.1). We still must write the remaining parameters in terms of the relevant nuclear matrix elements and fundamental coupling constants. These coupling constants are, in general, complex-valued, and JTW does

not choose a phase angle for us. We write them out in Eqs. (B.4-B.6).

$$\begin{aligned}\xi &= |M_F|^2 (|C_S|^2 + |C_V|^2 + |C'_S|^2 + |C'_V|^2) \\ &\quad + |M_{GT}|^2 (|C_T|^2 + |C_A|^2 + |C'_T|^2 + |C'_A|^2)\end{aligned}\quad (\text{B.4})$$

$$b_{\text{Fierz}} \xi = \pm 2\gamma \operatorname{Re} [|M_F|^2 (C_S C_V^* + C'_S C_V'^*) + |M_{GT}|^2 (C_T C_A^* + C'_T C_A'^*)] \quad (\text{B.5})$$

$$\begin{aligned}A_\beta \xi &= |M_{GT}|^2 \lambda_{J'J} \left[ \pm 2 \operatorname{Re}[C_T C_T'^* - C_A C_A'^*] + 2 \frac{\alpha Z m_e}{p_\beta} \operatorname{Im}[C_T C_A'^* + C'_T C_A^*] \right] \\ &\quad + \delta_{J'J} M_F M_{GT} \left( \frac{J}{J+1} \right)^{1/2} \left[ 2 \operatorname{Re}[C_S C_T'^* + C'_S C_T^* - C_V C_A'^* - C'_V C_A^*] \right. \\ &\quad \left. \pm 2 \frac{\alpha Z m_e}{p_\beta} \operatorname{Im}[C_S C_A'^* + C'_S C_A^* - C_V C_T'^* - C'_V C_T^*] \right]\end{aligned}\quad (\text{B.6})$$

Note that JTW presents slightly different expressions for the sign convention in components of  $A_\beta$  within [10] and [20]. Here, we adopt the convention from the latter publication. Furthermore, we do not require that either  $M_F$  or  $M_{GT}$  be positive (which would allow us to safely drop their absolute value indicators and make the conventions of these two papers equivalent). In order to obtain the correct, physically observed value for  $A_\beta$ , we require that the  $M_F M_{GT}$  term in Eq. (B.6) have an overall positive value. Because we know that the scalar and tensor couplings must be small, and any imaginary contributions to the term must be small, we conclude that

$$M_F M_{GT} (C_V C_A'^* + C'_V C_A^*) < 0. \quad (\text{B.7})$$

Also,  $\xi = G_F^2 \cos \theta_C f_1(E)$ .

## B.2 Holstein

Holstein [21] [28] generously provides explicit equations to match both Eq. (B.1) (i.e. Holstein's Eq. (51), where neutrino direction is a parameter of the probability distribution) and Eq. (B.3) (Holstein's Eq. (52), where neutrino direction has already been integrated over).

Here's Holstein's Eq. (52):

$$\begin{aligned} d^3\Gamma_{\text{Holstein}} &= 2G_v^2 \cos^2 \theta_c \frac{F_{\mp}(Z, E_\beta)}{(2\pi)^4} p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d^3\hat{\Omega}_\beta \\ &\times \left\{ F_0(E_\beta) + \Lambda_1 F_1(E_\beta) \hat{\mathbf{n}} \cdot \frac{\vec{p}_\beta}{E_\beta} + \Lambda_2 F_2(E_\beta) \left[ \left( \hat{\mathbf{n}} \cdot \frac{\vec{p}_\beta}{E_\beta} \right)^2 - \frac{1}{3} \frac{p_\beta^2}{E_\beta^2} \right] \right. \\ &\left. + \Lambda_3 F_3(E_\beta) \left[ \left( \hat{\mathbf{n}} \cdot \frac{\vec{p}_\beta}{E_\beta} \right)^3 - \frac{3}{5} \frac{p_\beta^2}{E_\beta^2} \hat{\mathbf{n}} \cdot \frac{\vec{p}_\beta}{E_\beta} \right] \right\} \end{aligned} \quad (\text{B.8})$$

A careful reader will eventually note that Holstein's spectral functions  $F_i(E_\beta)$  are not the same as the  $F_i(E_\beta, u, v, s)$  in any limit, despite the notational similarities. Among other rules, Holstein's spectral functions obey these:

$$F_i(E_\beta) \neq F_i(E_\beta, u, v, s) \quad (\text{B.9})$$

$$F_i(E_\beta) = H_i(E_\beta, u, v, 0) \quad (\text{B.10})$$

$$f_i(E_\beta) = F_i(E_\beta, u, v, 0). \quad (\text{B.11})$$

For the  $F_i(E_\beta)$  functions of interest to us here, we find the following relationships:

$$\begin{aligned} F_0(E_\beta) &= H_0(E_\beta, J, J', 0) = F_1(E_\beta, J, J', 0) &= f_1(E_\beta) \\ F_1(E_\beta) &= H_1(E_\beta, J, J', 0) = F_4(E_\beta, J, J', 0) + \frac{1}{3} F_7(E_\beta, J, J', 0) &= f_4(E_\beta) + \frac{1}{3} f_7(E_\beta) \\ F_2(E_\beta) &= H_2(E_\beta, J, J', 0) = F_{10}(E_\beta, J, J', 0) + \frac{1}{2} F_{13}(E_\beta, J, J', 0) &= f_{10}(E_\beta) + \frac{1}{3} f_{13}(E_\beta) \\ F_3(E_\beta) &= H_3(E_\beta, J, J', 0) = F_{18}(E_\beta, J, J', 0) &= f_{18}(E_\beta). \end{aligned} \quad (\text{B.12})$$

Note that the  $f_i(E_\beta)$  in Eq. B.12 are the same spectral functions used to describe a polarized decay spectrum when the neutrino (ie, the recoil) is also observed – though of course such a spectrum must have other terms as well. For the spectrum of interest to us here, in which the neutrino direction has already been integrated over, we can simply look up the  $H_i(E_\beta, J, J', 0) = H_i(E, u, v, s=0)$  spectral functions, and leave it

at that. We find:

$$\begin{aligned}
F_0(E_\beta) = & |a_1|^2 + 2 \operatorname{Re}[a_1^* a_2] \frac{1}{3M^2} \left[ m_e^2 + 4E_\beta E_0 + 2 \frac{m_e^2}{E_\beta} E_0 - 4E_\beta^2 \right] \\
& + |c_1|^2 + 2 \operatorname{Re}[c_1^* c_2] \frac{1}{9M^2} \left[ 11m_e^2 + 20E_\beta E_0 - 2 \frac{m_e^2}{E_\beta} E_0 - 20E_\beta^2 \right] \\
& - 2 \frac{E_0}{3M} \operatorname{Re}[c_1^*(c_1 + d \pm b)] + \frac{2E_\beta}{3M} (3|a_1|^2 + \operatorname{Re}[c_1^*(5c_1 \pm 2b)]) \\
& - \frac{m_e^2}{3ME_\beta} \operatorname{Re} \left[ -3a_1^* e + c_1^* \left( 2c_1 + d \pm 2b - h \frac{E_0 - E_\beta}{2M} \right) \right]
\end{aligned} \tag{B.13}$$

$$\begin{aligned}
F_1(E_\beta) = & \delta_{u,v} \left( \frac{u}{u+1} \right)^{1/2} \left\{ 2 \operatorname{Re} \left[ a_1^* \left( c_1 - \frac{E_0}{3M}(c_1 + d \pm b) + \frac{E_\beta}{3M}(7c_1 \pm b + d) \right) \right] \right. \\
& + 2 \operatorname{Re}[a_1^* c_2 + c_1^* a_2] \left( \frac{4E_\beta(E_0 - E_\beta) + 3m_e^2}{3M^2} \right) \Big\} \\
& \mp \frac{(-1)^s \gamma_{u,v}}{u+1} \operatorname{Re} \left\{ c_1^* \left( c_1 + 2c_2 \left( \frac{8E_\beta(E_0 - E_\beta) + 3m_e^2}{3M^2} \right) - \frac{2E_0}{3M}(c_1 + d \pm b) \right. \right. \\
& \left. \left. + \frac{E_\beta}{3M}(11c_1 - d \pm 5b) \right) \right\} + \frac{\lambda_{u,v}}{u+1} \operatorname{Re} \left\{ c_1^* \left[ -f \left( \frac{5E_\beta}{M} \right) \right. \right. \\
& \left. \left. + g \left( \frac{3}{2} \right)^{1/2} \left( \frac{E_0^2 - 11E_0E_\beta + 6m_e^2 + 4E_\beta^2}{6M^2} \right) \right] \right\} \pm 3j_2 \left( \frac{8E_\beta^2 - 5E_0E_\beta - 3m_e^2}{6M^2} \right)
\end{aligned} \tag{B.14}$$

$$\begin{aligned}
F_2(E_\beta) = & \theta_{u,v} \frac{E_\beta}{2M} \operatorname{Re} \left[ c_1^* \left( c_1 + c_2 \frac{8(E_0 - E_\beta)}{3M} - d \pm b \right) \right] \\
& - \delta_{u,v} \frac{E_\beta}{M} \left[ \frac{u(u+1)}{(2u-1)(2u+3)} \right]^{1/2} \operatorname{Re} \left\{ a_1^* \left( \left( \frac{3}{2} \right)^{1/2} f + g \frac{E_\beta + 2E_0}{4M} \right. \right. \\
& \left. \left. \pm \left( \frac{3}{2} \right)^{1/2} j_2 \frac{E_0 - E_\beta}{2M} \right) \right\} + (-1)^s \kappa_{u,v} \frac{E_\beta}{2M} \operatorname{Re} \left[ c_1^* \left( \pm 3f \pm \left( \frac{3}{2} \right)^{1/2} g \frac{E_0 - E_\beta}{M} \right. \right. \\
& \left. \left. + 3j_2 \frac{E_0 - 2E_\beta}{2M} \right) \right] + \epsilon_{u,v} \operatorname{Re}[c_1^* j_3] \left( \frac{21E_\beta^2}{8M^2} \right)
\end{aligned} \tag{B.15}$$

$$\begin{aligned}
F_3(E_\beta) = & -\delta_{u,v} (3u^2 + 3u - 1) \left[ \frac{u}{(u-1)(u+1)(u+2)(2u-1)(2u+3)} \right]^{1/2} \\
& \times \operatorname{Re}[a_1^* j_3] \left( \frac{E_\beta^2 \sqrt{15}}{4M^2} \right) + \frac{\rho_{u,v}}{u+1} \operatorname{Re} \left[ c_1^* (g\sqrt{3} + j_2 \sqrt{2}) \left( \frac{5E_\beta^2}{4M^2} \right) \right] \\
& \pm \frac{(-1)^s \sigma_{u,v}}{u+1} \operatorname{Re}[c_1^* j_3] \left( \frac{5E_\beta^2}{2M^2} \right)
\end{aligned} \tag{B.16}$$

and we might really appreciate if these things could be simplified a bit.

The terms  $a_1, a_2, b, c_1, c_2, d, e, f, g, h, j_2, j_3$  are “structure functions”. Holstein gives some predictions for their form, assuming the impulse approximation holds, in his Eq. (67). For the most part, the values and form of these structure functions are beyond the scope of this thesis, so I will not re-write them all here. It should be noted that the numerical values used for these parameters came from a private communication from Ian Towner to ... someone other than me. However, it is important to note the expressions for  $a_i$  and  $c_i$ , because these will directly come into play when we try to reconcile Holstein’s expression with JTW’s. Therefore,

$$a(q^2) \approx \frac{g_V(q^2)}{(1 + \frac{\Delta}{2M})} \left[ M_F + \frac{1}{6}(q^2 - \Delta^2)M_{r^2} + \frac{1}{3}\Delta M_{\mathbf{r} \cdot \mathbf{p}} \right] \tag{B.17}$$

$$\begin{aligned}
c(q^2) \approx & \frac{g_A(q^2)}{(1 + \frac{\Delta}{2M})} \left[ M_{GT} + \frac{1}{6}(q^2 - \Delta^2)M_{\sigma r^2} + \frac{1}{6\sqrt{10}}(2\Delta^2 + q^2)M_{1y} \right. \\
& \left. + A \frac{\Delta}{2M} M_{\sigma L} + \frac{1}{2}\Delta M_{\sigma rp} \right]
\end{aligned} \tag{B.18}$$

Somewhere I have to define  $q^2$  and  $\Delta$  are.

...where the  $M_{xxx}$ ’s are certain nuclear matrix elements. However, Eqs. (B.13-B.16) are not written in terms of  $a(q^2)$  and  $c(q^2)$ , but rather in terms of  $a_1, a_2, c_1$ , and  $c_2$ . In fact, Holstein is implicitly using series expansions to remove the dependence on recoil momentum, so that

$$a(q^2) = a_1 + \left( \frac{q^2}{M^2} \right) a_2 + \dots \tag{B.19}$$

$$c(q^2) = c_1 + \left( \frac{q^2}{M^2} \right) c_2 + \dots \tag{B.20}$$

There was something wrong with this assumption. Something circular. I forgot. Blah.

Or will I?

Somewhere, just list out the goddamn values of things that I inherited from Ian Towner’s personal communication that one time, over multiple generations of grad students.

Next, Holstein goes and tweaks those  $F_i(E_\beta)$  terms that we've already written out, by adding in an adjustment for Coulomb corrections. Those corrections have this form:

$$F_i(E_\beta) \rightarrow \tilde{F}_i(E_\beta) := F_\mp(Z, E_\beta) [F_i(E_\beta) + \Delta F_i(E_\beta)] \quad (\text{B.21})$$

To obtain expressions for the  $\Delta F_i(E_\beta)$ , Holstein invokes some Feynman diagrams and provides expressions for several integrals, all of which are both complex and complicated. The modified spectral functions are provided in terms of functions of these integrals. Since nobody wants to have to evaluate those integrals, Holstein makes a further approximation by taking only the first term in an expansion of the  $\Delta F_i(E_\beta)$  in terms of  $Z\alpha$ , where  $Z\alpha \ll 1$ . Then, the resulting expressions for  $\Delta F_i(E_\beta)$  can be written in terms of much more straightforward integrals over form factors for electric charge and weak charge.

If we make the further assumption that these form factors are identical, and that both types of charge are spread over a ball of uniform density with radius  $R$ , then we find:

$$X = Y = \frac{9\pi R}{140} \quad (\text{B.22})$$

and also, I think something like that the weak charge is the same distribution as the electric charge

in the Eqs. (B.23 - B.25) that follow.

Because Holstein doesn't actually write this stuff out in terms of  $F_i(E_\beta)$ , but rather in terms of  $F_i(E_\beta, u, v, s)$ , this correction presents yet another opportunity for the reader to interpret his notation incorrectly. We note that one must remember to make use of the relations in Eq. (B.12). Furthermore, Holstein notes that some of the terms  $F_i(E_\beta, u, v, s)$  are suppressed already, and he does not consider those terms further. We will take this approximation to be adequate for our purposes here.

What is less clear, given the context in the paper, is whether or not when Holstein writes out his simplified expressions for  $\Delta F_x(E_\beta, u, v, s)$  he actually means  $F_\mp(Z, E_\beta)\Delta F_i(E_\beta, u, v, s)$ . These terms are pretty small, so it probably doesn't \*really\* matter, but it would still be really nice to \*know\*, damn it.

So, we'll write out the functions for these corrections.

$$\begin{aligned} \Delta F_1(E_\beta, u, v, s) &= \mp \left( \frac{8\alpha Z}{3\pi} \right) X \left[ E_\beta \left( 8|a|^2 + \frac{28}{3}|c|^2 \right) + E_0 \left( |a|^2 - \frac{1}{3}|c|^2 \right) \right. \\ &\quad \left. + 3 \left( \frac{m_e c^2}{E_\beta} \right) (|a|^2 + |c|^2) \right] \end{aligned} \quad (\text{B.23})$$

$$\Delta F_4(E_\beta, u, v, s) = \mp \left( \frac{8\alpha Z}{3\pi} \right) 9XE_\beta \left[ 2\delta_{u,v} \left( \frac{u}{u+1} \right)^{1/2} \text{Re}[a^*c] \mp (-1)^s \left( \frac{\gamma_{u,v}}{u+1} \right) |c|^2 \right] \quad (\text{B.24})$$

$$\begin{aligned} \Delta F_7(E_\beta, u, v, s) = & \mp \left( \frac{8\alpha Z}{3\pi} \right) X (E_0 - E_\beta) \left[ 2\delta_{u,v} \left( \frac{u}{u+1} \right)^{1/2} \text{Re}[a^*c] \right. \\ & \left. \mp (-1)^s \left( \frac{\gamma_{u,v}}{u+1} \right) |c|^2 \right] \end{aligned} \quad (\text{B.25})$$

We note that the above corrections have been written in terms of  $a(q^2)$  and  $c(q^2)$ , and we must use Eqs. (B.19, B.20) to put the results in terms of  $a_1$ ,  $a_2$ ,  $c_1$ , and  $c_2$  so that they can be correctly combined with Eqs. (B.13-B.16).

If we evaluate Holstein's Eqs. (B8), which I will absolutely not type out here, for the case  $u = v = J = J' = 3/2$ , we find the following values:

$$\begin{aligned} \delta_{u,v} &= 1 & \theta_{u,v} &= 1 & \rho_{u,v} &= \frac{-41}{40} \\ \gamma_{u,v} &= 1 & \kappa_{u,v} &= \frac{1}{2\sqrt{2}} & \sigma_{u,v} &= \frac{-41}{4\sqrt{35}} \\ \lambda_{u,v} &= \frac{-\sqrt{2}}{5} & \epsilon_{u,v} &= \frac{-1}{2\sqrt{5}} & \phi_{u,v} &= 0 \end{aligned} \quad (\text{B.26})$$

Furthermore, in our calculations here, we will be considering only the  $\beta^+$  decay modes, and therefore we take the *lower* sign when the option arises. We also will use  $s = 0$ , so that  $(-1)^s = +1$ .

Also, pretty sure one of those never gets used. Which one was it? idk.

- - - - -

Let's define some of that notation! Firstly,

$$\text{Holstein's } \hat{n} = \text{JTW's } \mathbf{j}, \quad (\text{B.27})$$

and the  $\Lambda_i$  are given by Holstein's Eq. (48):

$$\Lambda_1 := \frac{\langle M \rangle}{J} \quad (B.28)$$

$$\Lambda_2 := 1 - \frac{3\langle M^2 \rangle}{J(J+1)} \quad (B.29)$$

$$\Lambda_3 := \frac{\langle M \rangle}{J} - \frac{5\langle M^3 \rangle}{J(3J^2 + 3J - 1)}. \quad (B.30)$$

---

We immediately see that Holstein's  $\Lambda_1$  is closely related to JTW's  $\frac{\vec{J}}{J}$ , and a bit later after John points it out to us, we see that Holstein's  $\Lambda_2$  is closely related to JTW's  $T_{\text{align}}$ . JTW doesn't have any equivalent to  $\Lambda_3$ . In particular, we find:

$$\Lambda_1 \hat{\mathbf{j}} = \frac{\langle M \rangle}{J} \hat{\mathbf{j}} = \frac{\vec{\mathbf{J}}}{J} \quad (B.31)$$

$$\Lambda_2 = T_{\text{align}} \frac{(2J-1)}{(J+1)}. \quad (B.32)$$

Note: It's not the case that  $|\vec{\mathbf{J}}| == J$ . It's actually super fucking infuriating notation.

# Appendix C

## Comparing Notation between Holstein and JTW

JB on how to triage Appendix C:

keep after:

C.1: keep 1st two sentences, discard next few.

C.2 Remove subsection title, replace by one sentence 'This section includes handwritten notes.' without further apology. We can't pay you anymore to make nice Latex. If the committee or Gerald objects to complex handwritten notes, App. C -> internal.

JB: Appendix C has some redundancies with B. You will have to sort that out. (n.b.: from context, it's less clear which appendices he's actually talking about, but whatever, there's certainly redundancies all around.)

### C.1 Comparison Guide

This is a short guide to differences in notation, sign convention, and normalization. There will be many tables here, chosen to aid in conversion between the two conventions. In the mean time, here's a bunch of old handwritten notes on the topic, that I'll eventually have to typeset and process into something intelligible.

Here's a table.

Here's another table.

Here's a third table.

Here's a fourth table.

Must find a better way to smush this table with display style math font typesetting. At present, it's somehow both too smooshed and not smooshed enough.

Holstein	JTW	Thesis	Comments
$k$			Neutrino momentum 4-vector
	$E_\nu$		Neutrino energy
$\hat{k}$	$\frac{\mathbf{p}_\nu}{E_\nu}$		3D Neutrino emission direction unit vector. Neutrinos are always treated as massless.
$p$			Beta momentum 4-vector, or sometimes the magnitude of the beta momentum 3-vector. Never the magnitude of the 4-vector.
$E$	$E_e$	$E_\beta$	Beta energy
$\mathbf{p}$	$\mathbf{p}_e$	$\vec{p}_\beta$	Beta momentum 3-vector
$q$			Recoil momentum 4-vector, or sometimes a magnitude.

Table C.1: A comparison of some kinematic terms in JTW [10] [20] and Holstein [21]. Yes, the bolding/italicization carries meaning.

Holstein	JTW	Comments
$u$	$J$	Initial state total nuclear angular momentum.
$v$	$J'$	Final state total nuclear angular momentum.
$s$	No equivalent?	Umm... I should check on this.

Table C.2: A comparison of some angular momenta in JTW [10] [20] and Holstein [21].

## C.2 Some old handwritten notes.

One day, these will go away and be replaced by nice clean latex.

Holstein	JTW	Thesis	Comments
$G_v^2 \cos \theta_C f_1(E)$	$\xi$	$\xi(E_\beta)$	Normalization. Proportional to the fractional decay rate.
$\hat{n}$	$\mathbf{j}$	$\hat{\mathbf{j}}$	Nuclear polarization unit vector. Also the axis of quantization.
$J$	$J$		Total nuclear angular momentum quantum number
$\langle M \rangle$	$ \langle \mathbf{J} \rangle $		Angular momentum projection along the axis of quantization
$\Lambda^{(1)} \hat{n} = \frac{\langle M \rangle}{J} \hat{n}$	$\frac{\langle \mathbf{J} \rangle}{J}$	$\Lambda_1 \hat{\mathbf{n}}$	Dipole element vector. Proportional to nuclear polarization. <i>(Rephrase this.)</i>
$\Lambda^{(1)} = \frac{\langle M \rangle}{J}$	...	$\Lambda_1$	...
$\Lambda^{(2)}$	$\frac{J(J+1)-3\langle(\vec{\mathbf{J}} \cdot \hat{\mathbf{j}})^2\rangle}{J(2J-1)} \frac{(2J-1)}{(J+1)}$	$T_{\text{align}}(\vec{\mathbf{J}}) \frac{(2J-1)}{(J+1)}$	Quadrupole element
$\Lambda^{(3)}$	No equivalent	$\Lambda_3$	Octopole element
$\Lambda^{(4)}$	No equivalent	$\Lambda_4$	Hexadecapole element

Table C.3: A comparison of the multipole elements and their normalizations (and some other stuff) in JTW [10] [20] and Holstein [21].

Term	Integral
$f_1(E_\beta)$	$\leftrightarrow \int 1 d\hat{\Omega}_k = 4\pi$
$f_2(E_\beta)$	$\leftrightarrow \int \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E_\beta} \right) d\hat{\Omega}_k = 0$
$f_3(E_\beta)$	$\leftrightarrow \int \left( \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E_\beta} \right)^2 - \frac{1}{3} \frac{p_\beta^2}{E_\beta^2} \right) d\hat{\Omega}_k = 0$
$f_4(E_\beta)$	$\leftrightarrow \int \left( \hat{n} \cdot \frac{\vec{p}_\beta}{E_\beta} \right) d\hat{\Omega}_k = 4\pi \left( \hat{n} \cdot \frac{\vec{p}_\beta}{E_\beta} \right)$
$f_5(E_\beta)$	$\leftrightarrow \int \left( \hat{n} \cdot \frac{\vec{p}_\beta}{E_\beta} \right) \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E_\beta} \right) d\hat{\Omega}_k = 0$
$f_6(E_\beta)$	$\leftrightarrow \int \left( \hat{n} \cdot \hat{k} \right) d\hat{\Omega}_k = 0$
$f_7(E_\beta)$	$\leftrightarrow \int \left( \hat{n} \cdot \hat{k} \right) \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E_\beta} \right) d\hat{\Omega}_k = \frac{1}{3} 4\pi \left( \hat{n} \cdot \frac{\vec{p}_\beta}{E_\beta} \right)$
$f_8(E_\beta)$	$\leftrightarrow \int \hat{n} \cdot \left( \frac{\vec{p}_\beta \times \hat{k}}{E_\beta} \right) d\hat{\Omega}_k = 0$
$f_9(E_\beta)$	$\leftrightarrow \int \hat{n} \cdot \left( \frac{\vec{p}_\beta \times \hat{k}}{E_\beta} \right) \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E_\beta} \right) d\hat{\Omega}_k = 0$
$f_{10}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right] d\hat{\Omega}_k = 4\pi T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right]$
$f_{11}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right] \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E} \right) d\hat{\Omega}_k = 0$
$f_{12}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \hat{k} \right] d\hat{\Omega}_k = 0$
$f_{13}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \hat{k} \right] \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E} \right) d\hat{\Omega}_k = \frac{1}{3} 4\pi T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right]$
$f_{14}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \hat{k}, \hat{k} \right] d\hat{\Omega}_k = 0$
$f_{15}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \hat{k}, \hat{k} \right] \left( \frac{\vec{p}_\beta \cdot \hat{k}}{E} \right) d\hat{\Omega}_k = 0$
$f_{16}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \times \hat{k} \right] d\hat{\Omega}_k = 0$
$f_{17}(E_\beta)$	$\leftrightarrow \int T_2(\hat{n}) : \left[ \hat{k}, \frac{\vec{p}_\beta}{E} \times \hat{k} \right] d\hat{\Omega}_k = 0$
$f_{18}(E_\beta)$	$\leftrightarrow \int T_3(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right] d\hat{\Omega}_k = 4\pi T_3(\hat{n}) : \left[ \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E}, \frac{\vec{p}_\beta}{E} \right]$

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Table C.4: Integrals of terms from Holstein's Eq. (51) [21].

$C_A = C'_A$ ;  $g = \pm \frac{1}{\sqrt{2}} M_{GT} (C_A + C'_A)$  ← prefer  $\oplus$   
 $C_V = C'_V$ ;  $a_1 = \pm \frac{1}{\sqrt{2}} M_F (C_V + C'_V)$  ← depends on above sign.

In our code:  
 $M_F = 1.0$   
 $M_{GT} = -0.62376$

Also in our Code:  
 $g_V = 1.00$   
 $g_A = 0.91210$

\* Really, the conversion above doesn't include the extra terms in  $a_1$  and  $g$  that couple to <sup>nuclear</sup> matrix elements other than  $M_F$  and  $M_{GT}$ .

I think the point is that I should actually interpret:

$g_V = \pm \frac{1}{\sqrt{2}} (C_V + C'_V) = 1.0 \rightarrow a_1 = g_V M_F \Rightarrow C_V = C'_V = \frac{1}{\sqrt{2}}$   
 $g_A = \pm \frac{1}{\sqrt{2}} (C_A + C'_A) \approx 0.91210 \rightarrow C_1 = g_A M_{GT} \Rightarrow C_A = C'_A = \frac{-0.91210}{\sqrt{2}} \approx -0.644952$

\* Check: with  $C_A = C'_A$  and  $C = C'_V$ , does JTW give the right  $A_\beta$ ?  
 $A_\beta = 0.763683; A_\beta = 0.496903 \rightarrow A_\beta = \underline{\underline{0.375394}}$  (bad!)

$A_\beta = 0.763671 \leftarrow$  Allow  $M_{GT}$  and  $M_F$  to have opposite signs. (Bad!)  
 $A_\beta = -0.568045 \leftarrow$  enforce  $\oplus M_{GT}$ . Or earlier JTW "convention".  
 Because Holstein insists on his own sign convention, we get  $M_{GT-Holstein} = -M_{GT-JTW}$

Figure C.1: "Notes 0"

In our code:

$$\boxed{\begin{array}{lll} M_F = 1.0 & ; g_V = 1.0 & \rightarrow g_V M_F = 1.0 \\ M_{GT} = -0.62376 & ; g_A = 0.91210 & \rightarrow g_A M_{GT} = -0.568931 \end{array}} \Rightarrow \rho \equiv \frac{g_A M_{GT}}{g_V M_F} \approx -0.568931_{\text{Holstein}}$$

\* If we require that  $C_A, C_A', C_V, C_V', M_F, M_{GT}$  are all real, and enforce that these can all take values which allow for Holstein and JTWW to be equivalent in some limits, we require:

$$\begin{array}{lll} \text{JTWW} & \text{Holstein} & \text{Holstein} \quad \text{JTWW} \\ \left\{ \begin{array}{l} f_1(E) \\ f_4(E) \end{array} \right. & \rightarrow \begin{array}{l} |a_1|^2 = |M_F|^2 (|C_V|^2 + |C_V'|^2) \\ |C_1|^2 = |M_{GT}|^2 (|C_A|^2 + |C_A'|^2) \end{array} \\ \downarrow \quad \downarrow & & \downarrow \quad \downarrow \\ \rightarrow |a_1|^2 = 2|M_{GT}|^2 \cdot \text{Re}[C_A C_A'^*] \end{array}$$

$$\begin{aligned} \text{Re}[a_1^* C_1] &= -\text{Re}[M_F M_{GT} (C_V C_A'^* + C_V' C_A^*)] \leftarrow \text{later} \\ \text{Re}[a_1^* C_1] &= -\text{Re}[|M_F| |M_{GT}| (C_V C_A'^* + C_V' C_A^*)] \leftarrow \text{earlier.} \end{aligned}$$

The Results:

$$\begin{array}{lll} C_V = C_V' & \text{Holstein} & \text{JTWW} \\ C_A = C_A' & a_1 = \pm \frac{1}{\sqrt{2}} M_F (C_V + C_V') = \pm M_F (\pm \sqrt{2}) (C_V) \\ & C_1 = \pm \frac{1}{\sqrt{2}} M_{GT} (C_A + C_A') = \pm M_{GT} (\pm \sqrt{2}) (C_A) \end{array}$$

$$a_1 \approx g_V M_{F,H}; \quad C_1 \approx g_A M_{GT,H};$$

Holstein

$$\boxed{M_{GT, \text{Holstein}} = -M_{GT, \text{JTWW}}}$$

which sign?!

$$a_1 \approx g_V M_F; \quad g_V = \pm \frac{1}{\sqrt{2}} (C_V + C_V') = 1.0; \quad M_F = 1.0; \quad ; \quad C_V = C_V'$$

$$C_1 \approx g_A M_{GT}; \quad g_A = \pm \frac{1}{\sqrt{2}} (C_A + C_A') \approx 0.91210; \quad M_{GT} = -0.62376; \quad C_A = C_A'$$

$\uparrow \uparrow$   
JTWW terms!

Figure C.2: "Notes 1"

$$\begin{aligned}
 b \cdot \bar{z} &= \pm 2\gamma \operatorname{Re} \left[ |M_F|^2 (C_S C_V^* + C'_S C_V'^*) + |M_{GT}|^2 (C_T C_A^* + C'_T C_A'^*) \right] \\
 &= -2\gamma \left[ |M_F|^2 (C_S C_V^* + C'_S C_V'^*) + |M_{GT}|^2 (C_T C_A^* + C'_T C_A'^*) \right] \\
 &= -2\gamma |M_F|^2 (C_S + C'_S) C_V^* + -2\gamma |M_{GT}|^2 (C_T + C'_T) C_A^* \\
 &= -2\gamma |M_F|^2 \cdot \frac{1}{\sqrt{2}} g_V \underbrace{(C_S + C'_S)}_{+\sqrt{2} g_S} + -2\gamma |M_{GT}|^2 \frac{(-1)}{\sqrt{2}} g_A \underbrace{(C_T + C'_T)}_{-\sqrt{2} g_T} \\
 &= -2\gamma |M_F|^2 \cdot g_V \cdot g_S - 2\gamma |M_{GT}|^2 \cdot g_A \cdot g_T \\
 \boxed{b \cdot \bar{z} = -2\gamma [ |M_F|^2 g_V g_S + |M_{GT}|^2 g_A g_T ]}
 \end{aligned}$$

~~BYE~~  $\gamma = (1 - \alpha^2 z^2)^{1/2}$

Figure C.3: "Notes 2"

To Match Up Holstein and JT W:

$$g_V = \frac{1}{\sqrt{2}}(C_V + C_V') ; \quad C_V = C_V' = \frac{1}{\sqrt{2}}g_V = \frac{1}{\sqrt{2}}$$

$$a_1 \approx g_V M_F$$

$$g_A = \frac{-1}{\sqrt{2}}(C_A + C_A') ; \quad C_A = C_A' = \frac{-1}{\sqrt{2}}g_A \approx \frac{-1}{\sqrt{2}}(0.91210) \quad c_1 \approx g_A M_{GT}$$

Also define:

$$g_\phi \equiv \frac{1}{\sqrt{2}}(C_\phi + C_\phi') ; \quad C_\phi = C_\phi' = \frac{1}{\sqrt{2}}g_\phi \approx 0$$

$$g_T \equiv \frac{-1}{\sqrt{2}}(C_T + C_T') ; \quad C_T = C_T' = \frac{-1}{\sqrt{2}}g_T \approx 0$$

Then, we find:

$$\ddot{\xi} = |M_F|^2(g_V^2 + g_\phi^2) + |M_{GT}|^2(g_A^2 + g_T^2)$$

$$A_p \ddot{\xi} = \frac{2}{5}|M_{GT}|^2(g_A^2 + g_T^2) + 2(\frac{3}{5})^{\frac{1}{2}}M_F M_{GT}(g_V g_A - g_\phi g_T)$$

$$b \cdot \ddot{\xi} = -2\gamma [ |M_F|^2 g_V g_\phi + |M_{GT}|^2 g_A g_T] ; \quad \gamma \equiv (1 - \alpha^2 \Xi^2)^{\frac{1}{2}}$$

$$F_0(E) \rightarrow F_0(E) + |M_F|^2 g_\phi^2 + |M_{GT}|^2 g_T^2$$

$$F_1(E) \rightarrow F_1(E) + \delta_{uv} \left( \frac{u}{u+v} \right) (-2) \cdot M_F M_{GT} \cdot g_V g_T + \alpha'_{uv} \left( \frac{1}{u+v} \right) |M_{GT}|^2 g_T^2$$

Figure C.4: "Notes 3"

- \* In some limits, Holstein and JT<sub>W</sub> are equivalent. For simplicity, we will require that JT<sub>W</sub>'s terms  $C_A, C_A', C_V, C_V', M_F$ , and  $M_{GT}$  are entirely real.
- \* The physical interpretation of this is that we ~~do not~~ require time-reversal symmetry to be obeyed.
- \* We use the following relationships:

$$\begin{aligned} \zeta &= f(E) \rightarrow |C_V|^2 = |M_F|^2 \cdot (|C_V|^2 + |C_V'|^2) \\ A_B \zeta &= f'(E) \rightarrow |C_A|^2 = |M_{GT}|^2 \cdot (|C_A|^2 + |C_A'|^2) \\ \end{aligned}$$

use the later JT<sub>W</sub> sign convention.

$$\begin{aligned} |C_V|^2 &= 2 \cdot |M_{GT}|^2 \cdot \operatorname{Re}[C_A C_A'^*] \\ \operatorname{Re}[a_i^* C_V] &= -M_F M_{GT} \cdot \operatorname{Re}[C_V C_A'^* + C_V' C_A^*] \end{aligned}$$

- \* Then, via trial and error, we find that the following set of relationships gives consistent results:

$$\begin{aligned} C_V &= C_V'; & a_i &= \left( \pm \frac{1}{\sqrt{2}} M_F (C_V + C_V') \right) \\ C_A &= C_A'; & g_i &= \left( \mp \frac{1}{\sqrt{2}} M_{GT} (C_A + C_A') \right) \end{aligned} \quad \text{Thus far, either set of signs is consistent. But they must be opposite.}$$

- \* In our code, which evaluates Holstein, we use these:

$a_i \approx g_V M_F$	$M_F = 1.0$	$g_V = 1.0$	Note that we believe we know $M_F$ better than we know $g_A$ . So, to measure " $A_B$ ", we vary $g_A$ and leave $M_{GT}$ fixed.
$g_i \approx g_A M_{GT}$	$M_{GT} = -0.62376$	$g_A \approx 0.91210$	

- \* We'll define some quantities,  $\rho$ :

$$\rho_{JT\bar{W}} = \frac{C_A \cdot M_{GT}}{C_V \cdot M_F}; \quad \rho_{\text{Holstein}} = \frac{g_A \cdot M_{GT}}{g_V \cdot M_F} \approx -0.568931$$

\* This  $\rho_{JT\bar{W}}$  uses our own definition from PRL.

\* Note that  $A_B$  comes out physically wrong unless  $\rho_{JT\bar{W}}$  is  $\oplus$ , ie, in  $A_B$ , there's a term  $\sim [C_V C_A'^* + C_V' C_A^*]$  and we need the whole thing to come out  $\oplus$ .

$\sim [M_F M_{GT} \cdot \operatorname{Re}[C_V C_A'^* + C_V' C_A^*]] \therefore C_V$ 's and  $C_A$ 's must have opposite signs.

- \* We'll take the convention that every body has the same matrix elements:

$$\begin{aligned} M_{GT, JT\bar{W}} &= M_{GT, \text{Holstein}} \\ M_{F, JT\bar{W}} &= M_{F, \text{Holstein}} \end{aligned}$$

- \* Then:

$$\begin{aligned} C_V &= C_V' \Rightarrow \oplus \\ C_A &= C_A' \Rightarrow \ominus \end{aligned} \quad \text{This because at the end of the day, we want Holstein's } a_i \text{ to be } \oplus, \text{ and } g_i \text{ to be } \ominus, \text{ or else we don't produce the right physics.}$$

$g_V = \frac{1}{\sqrt{2}} (C_V + C_V') = 1.0$	$C_V = C_V' = \frac{+1}{\sqrt{2}} g_V = \frac{1}{\sqrt{2}}$
$g_A = \frac{-1}{\sqrt{2}} (C_A + C_A') \approx +0.91210$	$C_A = C_A' = \frac{-1}{\sqrt{2}} g_A \approx \frac{-1}{\sqrt{2}} (0.91210)$

Figure C.5: "Notes 4"

- \* In some limits, Holstein and JTW are equivalent. We require that JTW's terms  $C_A, C_A', C_V, C_V', M_F$ , and  $M_{GT}$  must all be entirely real. We can probably do this WLOG. (Or without very much loss of generality, at least.)
- \* Use the following relationships:

$$\xi = f_x(E) \rightarrow |q_i|^2 = |M_F|^2 (|C_V|^2 + |C_V'|^2)$$

$$|C_V|^2 = |M_{GT}|^2 \cdot (|C_A|^2 + |C_A'|^2)$$

$$A \cdot \xi = f_x(E) \rightarrow |C_V|^2 = 2 \cdot |M_{GT}|^2 \cdot \text{Re}[C_V C_A'^*]$$

$$\text{Re}[C_V] = -\text{Re}[M_F M_{GT} (C_V C_A'^* + C_V' C_A)] \leftarrow \text{later JTW convention.}$$

we'll only use this.

- \* Then, the following relationships give us internally consistent results:

$$\begin{aligned} C_V &= C_V' ; & a_i &= \pm \frac{1}{\sqrt{2}} M_F (C_V + C_V') \\ C_A &= C_A' ; & C_i &= \pm \frac{1}{\sqrt{2}} M_{GT} (C_A + C_A') \end{aligned} \quad \begin{array}{l} \text{either set of signs is consistent.} \\ \text{But they must be opposite.} \end{array}$$

Code has these signs for  $q_i$  and  $C_i$ .  
Do I know why?

- \* In our code (which evaluates Holstein), we use these values:

$\oplus$	$a_i \approx g_V M_F$	$M_F = 1.0$	$g_V = 1.0$
$\ominus$	$C_i \approx g_A M_{GT}$	$M_{GT} = -0.62376$	$g_A = 0.91210$

- \* We can also define:

$\rho_{\text{Holstein}} \equiv \frac{g_A M_{GT}}{g_V M_F} \approx -0.568931$	$\rho_{\text{JTW}} \equiv \frac{C_A M_{GT}}{C_V M_F} \leftarrow \text{we use } \rho_{\text{JTW}} \text{ def. in PRL.}$
--	--

- \* We do not get the correct JTW  $A_B$  unless we require that  $\rho_{\text{JTW}}$  is  $\oplus$ . Equivalently, we require that  $\text{Re}[M_F M_{GT} (C_V C_A'^* + C_V' C_A)]$  must be  $\oplus$ . But  $M_{GT}$  is  $\ominus$ !  $\rightarrow$  Option 1: Take  $M_{GT, \text{JTW}} = -M_{GT, \text{Holstein}}$ .  $\rightarrow$  Option 2: Take  $C_A = C_A'$  to be  $\ominus$ , and  $C_V = C_V'$  to be  $\oplus$ .

(really, there are other options.  
But let's leave  $M_F$  and  $C_V$  alone.)

- \* For Holstein to come out right, we need  $C_i$  to be  $\ominus$ , and  $q_i$  to be  $\oplus$ . I think.

$\bullet$   $g_V = \frac{1}{\sqrt{2}} (C_V + C_V') = 1.0$   
 $g_A = \frac{-1}{\sqrt{2}} (C_A + C_A') = 0.91210 \Rightarrow C_A$  must be  $\ominus$  then, bc  $g_A$  is  $\oplus$  and  $M_{GT, \text{Holstein}}$  is  $\ominus$ , and we need  $C_i$  to be  $\ominus$ .

- \* OK. Now what do I do with  $C_i$  and  $G_i$ ?  $\rightarrow$  can't just stick them into  $M_F$  and  $M_{GT}$ . even in JTW it doesn't come out consistent.
- \* Have to write JTW in Holstein notation so I can figure out where to put  $C_i, G_i$  in.

Figure C.6: "Notes 5"

## Appendix D

# Holstein/JTW Comparison Confusion

Appendix D -> internal document.

JB: Appendix C has some redundancies with B. You will have to sort that out. The page-long shaggy dog story of f7 is distracting and needs to be truncated to the insight gained: "f7 is a recoil order term, and there are no recoil order terms in JTW." There may be many more things to do of that sort.

...

MJA: There's fuck all mention of f7 in Appendix C at the time of this comment, so he probably meant Appendix D. Probably there are redundancies everywhere though.

Ben at pg 17(30) claims the relation between JTW and Holstein for  $A_\beta$  is:

$$A_\beta = \frac{f_4(E) + \frac{1}{3}f_7(E)}{f_1(E)} \quad (\text{D.1})$$

See, it's counterintuitive, because I would have guessed that it would be just

$$A_\beta = \frac{f_4(E)}{f_1(E)} \quad (\text{D.2})$$

...But it's not. That extra  $f_7$  term is there, being weird. In Holstein (51), it's all like,

$$d^5\Gamma = (...) + (...) * \Lambda_1(\hat{n} \cdot \hat{k}) \left( \frac{\vec{p}}{E} \cdot \hat{k} \right) f_7(E), \quad (\text{D.3})$$

and that just doesn't look like  $A_\beta$ .

So, maybe there's some magic that happens when you integrate it and it turns into (52). From (52), I would (naively??) think that:

$$A_\beta = \frac{F_1(E)}{F_0(E)} \quad (\text{D.4})$$

Is it even true?!? Let's see what Holstein has to say...

In general,

$$f_i(E) = F_i(E, J, J', 0) \quad (\text{D.5})$$

$$F_i(E) = H_i(E, J, J', 0) \quad (\text{D.6})$$

So here specifically, we have:

$$F_0(E) = H_0(E, u, v, s) = F_1(E, u, v, s) \quad (\text{D.7})$$

$$F_1(E) = H_1(E, u, v, s) = F_4(E, u, v, s) + \frac{1}{3}F_7(E, u, v, s) \quad (\text{D.8})$$

$$= f_4(E) + \frac{1}{3}f_7(E) \quad (\text{D.9})$$

So I guess whatever the fuck Ben did to get his result checks out, and my naive supposition was correct. But now how do I translate that into JTW for anything else?!

JTW just straight-up has \*nothing\* that corresponds to the  $f_7$  term in Holstein. The integral that puts  $f_7$  into  $A_\beta$  has simply \*not been done\* at the point where JTW writes down their equation.

So, okay, let's take a look at how the dominant terms in  $f_4$ ,  $f_1$ , and  $f_7$  scale. From Holstein (pg 807):

$$f_1(E) \approx a_1^2 + c_1^2 \quad (\text{D.10})$$

$$f_4(E) \approx (\text{const}) * 2a_1c_1 + (\text{const})c_1^2 \quad (\text{D.11})$$

$$f_7(E) \approx (\text{const}) * a_1c_1 \frac{E_0}{M} + (\text{const})a_1c_1 \frac{E}{M} + (\text{const})c_1^2 \frac{E_0}{2M} + (\text{const})c_1^2 \frac{E}{2M} \quad (\text{D.12})$$

OK, so I think  $f_7$  wouldn't be included in JTW anyway, because it's too high order in  $E/M$ . (Is there really nothing in  $f_7$  that's not multiplied by at least one factor of  $1/M$  ?? .... yep, nothing.)

So here's what Coulomb-JTW says (set  $C_S = C'_S = C_T = C'_T = 0$ , and require that  $C_A = C'_A$  and  $C_V = C'_V$  are real):

$$\xi = |M_F|^2(2C_V^2) + |M_{GT}|^2(2C_A^2) \quad (\text{D.13})$$

$$A_\beta \xi = |M_{GT}|^2 \frac{1}{J+1} \left[ +2C_A^2 + M_F M_{GT} \left( \frac{J}{J+1} \right)^{1/2} * (-2C_V C_A) \right] \quad (\text{D.14})$$

Indeed, there are no E/M terms. So we agree with ourselves here. That's nice. But actually, we need to figure out how to convert *\*all\** of the JTW letters into Holstein notation. Not just  $A_\beta$ . Of particular importance is anything with a *\*linear\** dependence on  $C_T$  (or  $C_S$ ). That includes  $bFierz$ , for which there is no Holstein equivalent, but also:

- Real parts of  $b_{\text{Fierz}}$
- Imaginary parts of  $a_{\beta\nu}$
- Imaginary parts of  $c_{\text{align}}$
- Imaginary parts of  $A_\beta$
- Real parts of  $B_\nu$
- Real parts of  $D_{\text{TR}}$

...which is actually all of the things. All of them. So, I claim these are the relationships:

$$\xi = f_1(E) \quad (?) \quad (\text{D.15})$$

$$a_{\beta\nu} = f_2(E) / f_1(E) \quad (\text{D.16})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}}{E} A_\beta = \Lambda_1 \hat{n} \cdot \frac{\vec{p}}{E} f_4(E) / f_1(E) \quad (\text{D.17})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}_\nu}{E E_\nu} B_\nu = \Lambda_1 \hat{n} \cdot \vec{k} f_6(E) / f_1(E) \quad (\text{D.18})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{(\vec{p} \times \vec{p}_\nu)}{E E_\nu} D_{\text{TR}} = \Lambda_1 \hat{n} \cdot \left( \frac{\vec{p}}{E} \times \hat{k} \right) f_8(E) / f_1(E) \quad (\text{D.19})$$

$$\left[ \frac{J(J+1) - 3\langle (\vec{J} \cdot \hat{j})^2 \rangle}{J(2J-1)} \right] \left[ \frac{1}{3} \frac{\vec{p} \cdot \vec{p}_\nu}{E E_\nu} - \frac{(\vec{p} \cdot \hat{j})(\vec{p}_\nu \cdot \hat{j})}{E E_\nu} \right] c_{\text{align}} = \Lambda_2 \left[ \left( \hat{n} \cdot \frac{\vec{p}}{E} \right) (\hat{n} \cdot \hat{k}) - \frac{1}{3} \left( \frac{\vec{p}}{E} \cdot \hat{k} \right) \right] f_{12}(E) / (\text{D.20})$$

Other Holstein terms in (51) have no JTW equivalent, either because JTW didn't include recoil-order corrections, or because JTW didn't bother with higher multipole moments. These Holstein-specific spectral functions are not used in JTW:

- $f_3(E)$  (dipole)
- $f_5(E)$  (dipole)
- $f_7(E)$  (dipole)
- $f_9(E)$  (dipole)
- $f_{10}(E)$  (quadrupole)
- $f_{11}(E)$  (quadrupole)
- $f_{13}(E)$  (quadrupole)
- $f_1(E)$  (quadrupole, used elsewhere)
- $f_{16}(E)$  (quadrupole)
- $f_{17}(E)$  (quadrupole)
- Octopoles:  $f_{18}, f_{19}, f_{20}, f_{21}, f_{22}, f_{23}, f_{24}$
- 16-poles:  $f_{25}, f_{26}, f_{27}$ .

OK, so what needs to happen now is for me to convert the JTW alphabet into \*other\* Holstein notation. Since I know how they scale with the  $f_i(E)$ 's, let's see if we can convert those specific  $f_i(E)$ 's into any of the Holstein notation that is going into my code – ie, the  $F_i(E)$ 's. In particular, we'll want  $f_1(E), f_2(E), f_4(E), f_6(E), f_8(E), f_{12}(E)$ . This will actually have the pleasant side-effect of telling us how to fucking do that goddamn neutrino momentum integral in JTW. I think. So, from Holstein:

- $f_1(E) = F_1(E, u, v, s) = H_0(E, u, v, s) = F_0(E)$
- $f_2(E) = F_2(E, u, v, s) = ?$
- $f_4(E) = F_4(E, u, v, s) = ?$

- $f_6(E) = F_6(E, u, v, s) = ?$
- $f_8(E) = F_8(E, u, v, s) = ?$
- $f_{12}(E) = F_{12}(E, u, v, s) = ?$

...which, let's be honest, doesn't really help. Let's go the other direction, then.

- $F_0(E) = H_0(E, u, v, s) = F_1(E, u, v, s) = f_1(E)$  as before, but also:
- $F_1(E) = H_1(E, u, v, s) = F_4(E, u, v, s) + \frac{1}{3}F_7(E, u, v, s) = f_4(E) + \frac{1}{3}f_7(E)$
- $F_2(E) = H_2(E, u, v, s) = F_{10}(E, u, v, s) + \frac{1}{3}F_{13}(E, u, v, s) = f_{10}(E) + \frac{1}{3}f_{13}(E)$
- $F_3(E) = H_3(E, u, v, s) = F_{18}(E, u, v, s) = f_{18}(E)$

So, okay, I can write \*my\* PDF in terms of only Holstein's  $f_1(E)$ ,  $f_4(E)$ ,  $f_7(E)$ ,  $f_{10}(E)$ ,  $f_{13}(E)$ ,  $f_{18}(E)$ . I can write JTW's PDF in terms of only Holstein's  $f_1(E)$ ,  $f_2(E)$ ,  $f_4(E)$ ,  $f_6(E)$ ,  $f_8(E)$ ,  $f_{12}(E)$ . Those ... aren't the same thing. Like, at all. If I integrate those, do they come out to be the same things? Somehow?

OK. I can separate some terms out into what they \*should\* correspond to based on their multipole dependence... Roughly speaking,

$$F_0(E) \leftrightarrow f_1(E) \quad (\text{obviously}) \quad (\text{D.21})$$

$$F_1(E) \leftrightarrow f_4(E), f_5(E), f_6(E), f_7(E), f_8(E), f_9(E) \quad (\text{D.22})$$

$$F_2(E) \leftrightarrow f_{10}(E), f_{11}(E), f_{12}(E), f_{13}(E), f_1(E), f_{16}(E) \quad (\text{D.23})$$

$$F_3(E) \leftrightarrow \dots \text{who even cares?} \quad (\text{D.24})$$

- \* Check: in Holstein, are there simple relationships between those things?
- \* Check: if I do the integrals of the momentum-thingies multiplying those specific  $f_i(E)$ 's in Eq. (51) do they turn out the way I expect? ie, do I recover the corresponding terms in Eq. (52)?

# Appendix E

## Compare by Multipoles!

JB: Keep Appendix E iff you can clean it up in one (1) hour. Otherwise -> internal.

My code uses Holstein's Eq. (52), rather than Eq. (51). In his notation, I'm using  $F_i(E)$ 's rather than  $f_i(E)$ 's. I need to convert between them. This is because:

- (a): Coulomb/Radiative corrections ( some terms, up to  $f_{15}(E)$  ):  
 $f_1, f_2, f_4, f_6, f_7, f_{12}, f_{14}, f_{15}$
- (b):  $C_S/C_T$  inclusion (JTW has equivalents for only some terms, up to  $f_{12}(E)$ ):  
 $f_1, f_2, f_4, f_6, f_8, f_{12}$ .

Holstein and JTW terms have \*this\* relationship:

$$\xi = f_1(E) \quad (? \text{ times some constant? doesn't matter.}) \quad (\text{E.1})$$

In fact,

$$\xi = G_v^2 \cos \theta_C f_1(E) \quad (\text{E.2})$$

$$a_{\beta\nu} = f_2(E) / f_1(E) \quad (\text{E.3})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}}{E} A_\beta = \Lambda_1 \hat{n} \cdot \frac{\vec{p}}{E} f_4(E) / f_1(E) \quad (\text{E.4})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}_\nu}{E_\nu} B_\nu = \Lambda_1 \hat{n} \cdot \vec{k} f_6(E) / f_1(E) \quad (\text{E.5})$$

$$\frac{\langle \vec{J} \rangle}{J} \cdot \frac{(\vec{p} \times \vec{p}_\nu)}{EE_\nu} D_{\text{TR}} = \Lambda_1 \hat{n} \cdot (\frac{\vec{p}}{E} \times \hat{k}) f_8(E) / f_1(E) \quad (\text{E.6})$$

$$\begin{aligned} & \left[ \frac{J(J+1) - 3\langle(\vec{J} \cdot \hat{j})^2\rangle}{J(2J-1)} \right] \left[ \frac{1}{3} \frac{\vec{p} \cdot \vec{p}_\nu}{EE_\nu} - \frac{(\vec{p} \cdot \hat{j})(\vec{p}_\nu \cdot \hat{j})}{EE_\nu} \right] c_{\text{align}} \\ &= \Lambda_2 \left[ (\hat{n} \cdot \frac{\vec{p}}{E})(\hat{n} \cdot \hat{k}) - \frac{1}{3} (\frac{\vec{p}}{E} \cdot \hat{k}) \right] f_{12}(E) / f_1(E) \end{aligned} \quad (\text{E.7})$$

- JTW Monopole Terms:  $\xi$ ,  $\xi \frac{m}{E} * b_{\text{Fierz}}$ ,  $\xi \frac{\vec{p} \cdot \vec{p}_\nu}{EE_\nu} * a_{\beta\nu}$
- JTW Dipole Terms:  $\xi \frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}}{E} * A_\beta$ ,  $\xi \frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p}_\nu}{E_\nu} * B_\nu$ ,  $\xi \frac{\langle \vec{J} \rangle}{J} \cdot \frac{\vec{p} \times \vec{p}_\nu}{EE_\nu} * D_{\text{TR}}$
- JTW Quadrupole Terms:  $\xi \left( \frac{J(J+1)-3\langle(\vec{J} \cdot \hat{j})^2\rangle}{J(2J-1)} \right) \left( \frac{1}{3} \frac{\vec{p} \cdot \vec{p}_\nu}{EE_\nu} - \frac{(\vec{p} \cdot \hat{j})(\vec{p}_\nu \cdot \hat{j})}{EE_\nu} \right) * c_{\text{align}}$

...

- Holstein (52) Monopole Term:

$$F_0(E) = f_1(E)$$

- Holstein (52) Dipole Term:

$$\begin{aligned} & \Lambda_1 (\hat{n} \cdot \frac{\vec{p}}{E}) * F_1(E) \\ &= \Lambda_1 (\hat{n} \cdot \frac{\vec{p}}{E}) * (f_4(E) + \frac{1}{3} f_7(E)) \end{aligned}$$

- Holstein (52) Quadrupole Term:

$$\begin{aligned} & \Lambda_2 \left( (\hat{n} \cdot \frac{\vec{p}}{E})^2 - \frac{1}{3} \frac{p^2}{E^2} \right) * F_2(E) \\ &= \Lambda_2 \left( (\hat{n} \cdot \frac{\vec{p}}{E})^2 - \frac{1}{3} \frac{p^2}{E^2} \right) * (f_{10}(E) + \frac{1}{3} f_{13}(E)) \\ &= \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E}] * (f_{10}(E) + \frac{1}{3} f_{13}(E)) \end{aligned}$$

- Holstein (52) Octopole Term:

$$\Lambda_3 \left( (\hat{n} \cdot \frac{\vec{p}}{E})^3 - \frac{3}{5} \frac{p^2}{E^2} (\hat{n} \cdot \frac{\vec{p}}{E}) \right) * F_3(E)$$

$$= \Lambda_3 \left( (\hat{n} \cdot \frac{\vec{p}}{E})^3 - \frac{3}{5} \frac{p^2}{E^2} (\hat{n} \cdot \frac{\vec{p}}{E}) \right) * f_{18}(E)$$

$$= \Lambda_3 T_3(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E}, \frac{\vec{p}}{E}] * f_{18}(E)$$

- Holstein (52) Hexadecapole Term:  
(none)

...

- Holstein (51) Monopole Terms:

$$f_1(E), \quad \frac{\vec{p} \cdot \hat{k}}{E} * f_2(E), \quad \left( \frac{(\vec{p} \cdot \hat{k})^2}{E^2} - \frac{1}{3} \frac{p^2}{E^2} \right) * f_3(E)$$

- Holstein (51) Dipole Terms:

$$\begin{aligned} \Lambda_1 (\hat{n} \cdot \frac{\vec{p}}{E}) * f_4(E), & \quad \Lambda_1 (\hat{n} \cdot \frac{\vec{p}}{E}) \frac{\vec{p} \cdot \hat{k}}{E} * f_5(E), \\ \Lambda_1 (\hat{n} \cdot \hat{k}) * f_6(E), & \quad \Lambda_1 (\hat{n} \cdot \hat{k}) \frac{\vec{p} \cdot \hat{k}}{E} * f_7(E), \\ \Lambda_1 \hat{n} \cdot \left( \frac{\vec{p}}{E} \times \hat{k} \right) * f_8(E) & \quad \Lambda_1 \hat{n} \cdot \left( \frac{\vec{p}}{E} \times \hat{k} \right) \frac{\vec{p} \cdot \hat{k}}{E} * f_9(E) \end{aligned}$$

- Holstein (51) Quadrupole Terms:

$$\begin{aligned} \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E}] * f_{10}(E), & \quad \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E}] (\frac{\vec{p} \cdot \hat{k}}{E}) * f_{11}(E), \\ \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \hat{k}] * f_{12}(E), & \quad \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \hat{k}] (\frac{\vec{p} \cdot \hat{k}}{E}) * f_{13}(E), \\ \Lambda_2 T_2(\hat{n}) : [\hat{k}, \hat{k}] * f_{14}(E), & \quad \Lambda_2 T_2(\hat{n}) : [\hat{k}, \hat{k}] (\frac{\vec{p} \cdot \hat{k}}{E}) * f_{15}(E) \quad (?) \\ \Lambda_2 T_2(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E} \times \hat{k}] * f_{16}(E), & \\ \Lambda_2 T_2(\hat{n}) : [\hat{k}, \frac{\vec{p}}{E} \times \hat{k}] * f_{17}(E) & \end{aligned}$$

- Holstein (51) Octopole Terms:

$$\Lambda_3 T_3(\hat{n}) : [\frac{\vec{p}}{E}, \frac{\vec{p}}{E}, \frac{\vec{p}}{E}] * f_{18}(E)$$

(also some other stuff, but this is the only term that doesn't integrate to zero.)

- Holstein (51) Hexadecapole Terms:

(some stuff. don't care.)

Holstein's tensor notation definitions:

$$T_2(\hat{n}) : [\vec{a}, \vec{b}] = \left( (\hat{n} \cdot \vec{a})(\hat{n} \cdot \vec{b}) - \frac{1}{3} \vec{a} \cdot \vec{b} \right) \tag{E.8}$$

$$T_3(\hat{n}) : [\vec{a}, \vec{b}, \vec{c}] = \left( (\hat{n} \cdot \vec{a})(\hat{n} \cdot \vec{b})(\hat{n} \cdot \vec{c}) - \frac{1}{5} \left( (\hat{n} \cdot \vec{a})(\vec{b} \cdot \vec{c}) + (\hat{n} \cdot \vec{b})(\vec{a} \cdot \vec{c}) + (\hat{n} \cdot \vec{c})(\vec{a} \cdot \vec{b}) \right) \right) \tag{E.9}$$

$$T_4(\hat{n}) : [\vec{a}, \vec{b}, \vec{c}, \vec{d}] = (\text{some stuff}) \tag{E.10}$$

Integrals by inspection: [\*\*\*\*\* REINSPECT THEM TOMORROW MELISSA,  
FOR THE LOVE OF GOD.] ... Actually, I'm'a just reference Table C.4.

# Appendix F

## Proposed Notation

JB says: Appendix F -> internal

According to insight that Alexandre thought was very obvious, because it was, JTW-style notation only happened in the first place because the lab frame matters. it's measured w.r.t. polarization or alignment or something. The integration over the two leptons is really an integration over the lab frame + one lepton.

### F.1 Beta End-point Energy

Note that nuclear physics notational convention, which is fucking retarded, apparently defines kinetic energy  $T$  s.t.

$$E = T + mc^2, \quad (\text{F.1})$$

but of course, it's still the case that

$$E = (p^2 c^2 + m^2 c^4)^{1/2}. \quad (\text{F.2})$$

But since  $Q = T_{final} - T_{initial}$ , we're sort-of stuck, if we want to describe the beta end-point energy in terms of  $Q$ , we find that

$$E_{\text{end}} = Q + m_e c^2 \quad (\text{F.3})$$

$$Q = (p_{\max}^2 c^2 + m_e^2 c^4)^{1/2} - m_e c^2 \quad (\text{F.4})$$

... actually, pretty sure that's all wrong. ... ... Actually-actually, it's fine.

See "jtw\_integration\_scratch3.nb" for more-other details. Of things. Things I've deleted from the document now. ...That version of the mathematica notebook, I believe, has finally got all the thingies right.

## F.2 Beta End-point Energy.2

Firstly, some Q-values for  $^{37}\text{K}$  (via bnl).

$$Q_{\text{EC}} = 6.14745(23) \text{ MeV} \quad (\text{F.5})$$

$$Q_{\beta+} = 5.12545(23) \text{ MeV} \quad (\text{F.6})$$

It would be better if I knew the branching ratio for EC/ $\beta+$  though.

Anyway, in the jtw notation,

$$E_0 := Q + m_e c^2. \quad (\text{F.7})$$

Is that  $\uparrow$  even true?? Because I'm really not sure it is. Via Kofoedhansen,  $(E_0 - E_e) = E_\nu$ . So there.

What does that mean in terms of the individual particles' energies? Well, if that's the beta end-point energy, it just means that that's the total (kinetic + rest) energy for the beta. So, we can distribute  $Q$  of kinetic energy around to the other particles.

$$Q = T_\beta + T_\nu + T_r \quad (\text{F.8})$$

$$= (E_\beta - m_\beta c^2) + (E_\nu - m_\nu c^2) + (E_r - M_r c^2) \quad (\text{F.9})$$

$$\approx E_\beta - m_\beta c^2 + E_\nu + T_r \quad (\text{F.10})$$

$$E_0 \approx E_\beta + E_\nu + T_r \quad (\text{F.11})$$

A thing that's worth noting is that (I think!) recoil-order corrections have been implicitly excluded at some point here. ...Is this even true??

$$T_\beta = (p_\beta^2 c^2 + m_e^2 c^4)^{1/2} - m_e c^2 \quad (\text{F.12})$$

$$\begin{aligned} T_\nu &= (p_\nu^2 c^2 + m_\nu^2 c^4)^{1/2} - m_\nu c^2 \\ &\approx p_\nu c \end{aligned} \quad (\text{F.13})$$

$$\begin{aligned} T_r &= (p_r^2 c^2 + m_r^2 c^4)^{1/2} - m_r c^2 \\ &\approx \frac{p_r^2}{2m_r} \end{aligned} \quad (\text{F.14})$$

# Appendix G

## The Parity Operator: Vectors and Axial Vectors and Pseudoscalars, oh my!

JB says: Appendix G -> internal (needed for understanding for later ST analysis).

via Samuel Wong, 1990. pg 212.

### G.1 Scalars

A scalar does not change sign under the parity operation, because why even would it?

### G.2 Vectors

Vectors, or “polar vectors” ( $\mathbf{V}$ ) are exactly what I think they are. Position  $\vec{r}$  and (linear) momentum  $\vec{p}$  are examples. The thing about these vectors is that they change sign (or, really, direction) under a parity transformation.

## G.3 Axial Vectors

Axial vectors (A) *don't* change sign under a parity transformation. An example is angular momentum,  $\vec{L} = \vec{r} \times \vec{p}$ . This is where the "mirror" thing breaks down. A mirror only is only a one-dimensional parity operator, so if you think of a thing with angular momentum and its reflection in a mirror, you imagine the mirror image with reversed angular momentum too. But in reality, if you change the sign of *all components* of  $\vec{r}$  and simultaneously all components of  $\vec{p}$ , it's clear that the quantity  $(\vec{r} \times \vec{p})$  remains unchanged. Pauli spin matrices,  $\vec{\sigma}$ , are axial vectors.

## G.4 Pseudoscalars

Pseudoscalars, (P). You'd think they were scalars, because they're just a number, but have to remember that you got them by taking the scalar product of a (polar) vector and an axial vector. If you apply the parity operator to the quantity  $P = (\vec{V} \cdot \vec{A})$ ,  $\vec{V}$  changes sign (for all components) while  $\vec{A}$  does not. So the resultant "scalar"  $P$  has to change sign too. That's how you know that  $P$  is really a pseudoscalar.

## G.5 Tensors

...Yeah, Wong doesn't really get into that. At least not here.

## G.6 Comments on Parity Conservation

An interaction made from a mixture of scalars and pseudoscalars, or a mixture of vectors and axial vectors, does *not* conserve parity.

For the strong and electromagnetic forces, parity *is* strictly conserved. Not so for the weak force!

## G.7 Q-Values

The  $Q$ -value for a particular interaction is defined as the difference between the kinetic energies of the final and initial systems.

$$Q = T_f - T_i \quad (\text{G.1})$$

In particular, for  $\beta^+$  decay, we're including .... which things? ... as part of the mass of the various systems?

## G.8 Helicity

Helicity,  $h$ , is a pseudoscalar.

$$h = \frac{\vec{\sigma} \cdot \vec{p}}{|\vec{p}|} \quad (\text{G.2})$$

## G.9 Conserved Vector Current Hypothesis

The CVC hypothesis asserts that the Fermi interactions of nucleons within a nucleus are *not* changed by all the surrounding mesons. (What?)

JB: You're describing a consequence of CVC, and are not stating the actual hypothesis.

JB: I think you don't have time to explain the CBC hypothesis. You'll just have to assume it. I personally found that the technical derivation was the only way to see what was going on.

JB: A good sketch of CVC is done in Commins' notes and his book with Bucksbaum [? ]. Ian towner did it in his notes. I reproduce this in my own course notes for Phys 505 (p.21-28 of the thing John attached to that email). First you show in Dirac notation that the E&M current for pointlike particles is conserved--this just needs one use of the Dirac Equation and conservation of electric charge. Then look what happens for finite nucleons. Then construct the analogous weak interaction current for the nucleon, including three possible current terms that transform like Lorentz vectors, and note the consequences if that current is still conserved.

Figure out how to cite somebody's unpublished/semi-unpublished notes.

JB: Jolley [13] describes the qualitative background on his text page 110 (attached). Feynman and Gell-Mann proposed that the isovector weak vector current and the isovector E&M vector current are members of an isovector triplet of currents all of which are conserved. [This idea eventually went straight into the SM electroweak interaction, with the addition of nonabelian and massive operators for the weak part, though figuring out how to make a consistent theory with the massive bosons took the combination of Yang-Mills gauge theories and then Weinberg and Salam's approaches.]

JB: What you say is one consequence, that meson exchange currents don't change the vector part of the weak interaction— this is consistent with conservation of electric charge and its analog in the weak interaction. This particular consequence is a very powerful tool in electromagnetic effects in nuclei, e.g. if you take the isovector combination of magnetic moments ( $^{37}\text{Ar}$  -  $^{37}\text{K}$  e.g.) you get something without meson exchange corrections and therefore precisely sensitive to other higher-order physics effects. Arima and Towner separately studied this for significant parts of their careers. The axial vector interaction is changed by meson exchange currents. The recent calculation of Gysbeg et al. accounting for most of the Gamow-Teller strength is including meson exchange currents in what they call 2-body currents, natural higher-order corrections in their chiral EFT expansion of the strong interaction between nucleons when you consider electroweak interactions.

JB: But there are lots of other things that also don't change the Fermi interaction... so picking out meson exchange currents for discussion is maybe not fully motivated. Lots of people state one consequence or another of CVC to motivate what they are doing, without explaining.

Paraphrased JB: Don't try to derive CVC in this thesis. Just cite the hypothesis and say that our Abeta stuff provides a test of it.

JB: Something else beyond the thesis scope, sorry: a nonzero  $b_{\text{Fierz}}$  does not necessarily break CVC, as the vector part of the SM weak interaction could still be conserved whether or not there are other quark-lepton currents with different Lorentz structure. You could make a nonzero  $b_{\text{Fierz}}$  from a 2nd-class scalar in the nucleon-electron weak current which would break CVC, but that could not be distinguished from a quark-electron extra scalar current.

# Appendix H

## Constraining the Analysis with Lifetime Measurements

JB says: Appendix H -> internal (critical for later ST analysis, but only then)

### H.1 Background/Introduction

The goal here is to understand what physical interpretation to give to (linear combinations of) G4 simulations with sets of coupling constants that may or may not be physically possible, given previous lifetime measurements that were not taken into account. The results aren't broken forever or anything, but some care must be given to the interpretation.

John and Dan say this paper is widely accepted to be wrong about some of the things. (Some assumption was wrong, I guess?) So then presumably what I've written here can't be trusted either.

JB: The paper that fixes the known mistakes in Severijns et al. [14] is L. Hayen and N. Severijns, 2019 [15]. It's conveniently located on the arXiv, and I should definitely go read it. The known mistake was using the ratio of integrals of the lepton momentum  $f_A/f_V$  more than once—there is a more subtle radiative correction for the Gamow-Teller piece. A paper using the formalism is D. Combs et al [16] (which is *also* conveniently located on the arXiv) with  $^{19}\text{Ne}$  and  $^{37}\text{K}$  results— their extraction is so close to Ben and Dan's that we conclude we are doing the formalism well. i.e. we used the  $f_A/f_V$  ratio correctly.

We'll loosely follow Severijns et. al.'s procedure [14]. In this paper, (which conveniently sets  $b_{\text{Fierz}}$  to zero as soon as the math starts getting messy), we see that

the authors will eventually need to split their treatment into "Fermi" and "Gamow-Teller" parts to arrive at their final result. This becomes clear upon examining their 1D decay rate element:

$$d\Gamma = (\text{constants}) \xi \left(1 + \frac{m}{W} b\right) F(\pm Z, W) S(\pm Z, W) (W - W_0)^2 pW dW \quad (\text{H.1})$$

where we find that the nuclear shape correction function,  $S(\pm Z, W)$ , is slightly different in the case of Fermi and Gamow-Teller decays. Though they note that  $S(\pm Z, W) = 1$  for both types of decay under the allowed approximation, and it changes only slightly under a more complete treatment, it is this term which gives rise to the statistical rate functions  $f_V$  and  $f_A$ . Note that the overall value of the ratio  $f_A/f_V$  directly changes any estimate of the mixing ratio  $\rho$ , so we will need at least an estimate of its value in order to do anything useful.

In particular,

$$f_{V/A} = \int F(\pm Z, W) S_{V/A}(\pm Z, W) (W - W_0)^2 pW dW \quad (\text{H.2})$$

No matter the form of  $S(\pm Z, W)$  — and I definitely *do not* know its form — this is clearly a very challenging integral. Luckily, a calculation result is provided (with no associated uncertainty given):

$$\frac{f_A}{f_V} \Big|_{37K} = 1.00456. \quad (\text{H.3})$$

So, we follow their calculation through, and at the end it yields this result:

$$Ft^{\text{mirror}} = \frac{2Ft^{0^+ \rightarrow 0^+}}{1 + \frac{f_A}{f_V}\rho^2}, \quad (\text{H.4})$$

with

$$\rho \approx \frac{C_A M_{GT}^0}{C_V M_F^0}. \quad (\text{H.5})$$

But of course, there's no reason why we can't do a similar calculation while including non-zero values of  $C_S$  and  $C_T$ . Probably.

## H.2 Now What?

In the case where  $b_{\text{Fierz}} = 0$ , this treatment gets us to the sort of results we might want. However, if we start introducing non-zero scalar or tensor coupling constants, it's unclear (to me) how we should treat the associated shape correction function(s). Eg, do we think the shape correction function  $S_V(\pm Z, W)$  is associated with a *Fermi* decay, or with a *vector* coupling? In the case of zero scalar or tensor couplings, the question is irrelevant because the calculation must be the same either way –  $M_F$  and  $C_V$  go together every time.

With  $b_{\text{Fierz}} \neq 0$ , the distinction changes the calculation though, since its terms have factors of (eg)  $M_F^2 C_V$ . Perhaps there are a whole different set of shape correction functions associated specifically with  $C_S$  and  $C_T$ .

I *might* be able to find a treatment in the literature somewhere where they do one thing or the other. For my own mental clarity, I would really like to know the answer. However, I recognize that for the purpose of evaluating scalar and tensor coupling constants, the distinction is largely academic, and won't really affect the answer. I have to just pick something and go with it.

## H.3 After Picking Something...

I declare (and so it has to be true, regardless of reality) that the shape correction function  $S_{V/A}(\pm Z, W)$  is associated with the matrix element  $M_{F/GT}$  rather than the coupling constant  $C_{V/A}$ . So, given this, we'll switch our notation a bit and write down a new decay rate:

$$d\Gamma = \left( \vec{\xi} + \frac{m}{W} (b\vec{\xi}) \right) \cdot d\vec{\Gamma}_0, \quad (\text{H.6})$$

where the vector components are the Fermi and Gamow-Teller components of the decay:

$$\vec{\xi} = \begin{bmatrix} 2M_F^2 (C_V^2 + C_S^2) \\ 2M_{GT}^2 (C_A^2 + C_T^2) \end{bmatrix} \quad (\text{H.7})$$

$$(\vec{b}\vec{\xi}) = \begin{bmatrix} \pm 2\gamma \operatorname{Re}[C_S C_V^* + C'_S C'^*_V] \\ \pm 2\gamma \operatorname{Re}[C_T C_A^* + C'_T C'^*_A] \end{bmatrix} \quad (\text{H.8})$$

$$d\vec{\Gamma}_0 = (\text{constants}) \begin{bmatrix} F_F(\pm Z, W) S_F(\pm Z, W) (W - W_0)^2 pW dW \\ F_{GT}(\pm Z, W) S_{GT}(\pm Z, W) (W - W_0)^2 pW dW \end{bmatrix}. \quad (\text{H.9})$$

We find that the integrals involved here are still hard. I will need to make some simplifying assumptions to get anywhere.

# Appendix I

## An $R_{slow}$ Thesis Proposal

Appendix I keep, it's excellent. It should be moved as is to Conclusions under "Future Experiment for the collaboration"! so people know you worked so hard on it!!

### I.1 An Old Rslow Abstract

The nuclear weak force is known to be a predominantly left-handed vector and axial-vector (V-A) interaction. An experiment is proposed to further test that observation, constraining the strength of right-handed (V+A) currents by exploiting the principle of conservation of angular momentum within a spin-polarized beta decay process. Here, we focus on the decay  $^{37}\text{K} \rightarrow ^{37}\text{Ar} + \beta^+ + \nu_e$ . The angular correlations between the emerging daughter particles provide a rich source of information about the type of interaction that produced the decay.

### I.2 Motivation

The nuclear weak force has long been known to be a predominantly left-handed chiral interaction, meaning that immediately following an interaction (such as a beta decay) with a weak force carrying boson ( $W^+$ ,  $W^-$ ,  $Z$ ), normal-matter leptons (such as the electron and electron neutrino) emerge with left-handed chirality while the anti-leptons (e.g. the positron and electron anti-neutrino) emerge with right-handed chirality. In the limit of massless particles, the particle's chirality is the same as its helicity. Thus, in a left-handed model, the direction of an (ultrarelativistic) normal

lepton's spin is antiparallel direction of its motion, and the direction of spin for an anti-lepton is parallel to its direction of motion. For a non-relativistic particle the property of chirality is fairly abstract, and describes the appropriate group representation and projection operators to be used in calculations. It should be noted that a fully chiral model is also one which is maximally parity violating.

This odd quirk of the nuclear weak force is not only *predominantly* true, but it is, to the best of our current scientific knowledge, *always* true – that is, attempts to measure any right-handed chiral components of the weak force have produced results consistent with zero [18][29]. This project proposes a further measurement to constrain the strength of the right-handed component of the weak interaction.

### I.3 The Decay Process

The kinematics of nuclear  $\beta^+$  decay are described by the following probability density function:

$$W(\langle I \rangle | E_\beta \hat{\Omega}_\beta \hat{\Omega}_\nu) = \left( \frac{1}{2\pi} \right)^5 F(-Z, E_\beta) p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d\hat{\Omega}_\beta d\hat{\Omega}_\nu \xi \left[ 1 + a_{\beta\nu} \frac{\vec{p}_\beta \cdot \vec{p}_\nu}{E_\beta E_\nu} + b_{\text{Fierz}} \frac{m_e}{E_\beta} \right. \\ + c_{\text{align}} \left( \frac{\frac{1}{3} \vec{p}_\beta \cdot \vec{p}_\nu - (\vec{p}_\beta \cdot \hat{j})(\vec{p}_\nu \cdot \hat{j})}{E_\beta E_\nu} \right) \left( \frac{I(I+1) - 3\langle (\vec{I} \cdot \hat{i})^2 \rangle}{I(2I-1)} \right) \\ \left. + \frac{\langle \vec{I} \rangle}{I} \left( A_\beta \frac{\vec{p}_\beta}{E_\beta} + B_\nu \frac{\vec{p}_\nu}{E_\nu} + D_{\text{TR}} \frac{\vec{p}_\beta \times \vec{p}_\nu}{E_\beta E_\nu} \right) \right], \quad (\text{I.1})$$

where  $\vec{I}$  is the nuclear spin-polarization,  $F(-Z, E_\beta)$  is the Fermi function, and parameters  $\xi$ ,  $a_{\beta\nu}$ ,  $b_{\text{Fierz}}$ ,  $c_{\text{align}}$ ,  $A_\beta$ ,  $B_\nu$ , and  $D_{\text{TR}}$  are functions that vary with the strengths of the vector, axial, scalar, and tensor couplings (constant throughout nature), as well as the Fermi and Gamow-Teller nuclear matrix elements (specific to the individual decay) [10][20].

The decay may be treated as a three-body problem in which the available kinetic energy is divided up between the beta, the neutrino, and the recoiling  $^{37}\text{Ar}$  nucleus, and (of course) the total linear and angular momentum are conserved. While the neutrino cannot be detected directly, its kinematics may be reconstructed from observations of the beta and the recoiling daughter nucleus. By placing detectors above and below the decaying atom along the axis of its polarization, we are able to obtain

information about the outgoing beta's energy and momentum, in the cases of interest to us, where it is emitted along (or close to) the axis of polarization.

The recoiling  $^{37}\text{Ar}$  nucleus is a bit trickier to work with, but the task is not impossible. One useful feature of the  $^{37}\text{K} \rightarrow ^{37}\text{Ar}$  transition is that, in addition to the  $\beta^+$  emitted in the decay itself, one or more *orbital* electrons from the parent atom are typically lost. In the majority of decay events only one orbital electron is ‘shaken off’ and so the daughter  $^{37}\text{Ar}$  atom is electrically neutral [24][30]. In the remaining cases, two or more orbital electrons are lost this way, and the daughter atom is positively charged. If we apply an electric field perpendicular to the direction of polarization, these positively charged  $^{37}\text{Ar}^{(+n)}$  ions may be collected into a detector, from which hit position and time of flight information may be extracted. These shake-off electrons are emitted with an average energy of only  $\sim 2\text{ eV}$  so to a very good approximation the other decay products are not perturbed by the presence of shake-off electrons.

It should be noted that for the class of decays of greatest interest, where the beta and the neutrino emerge back-to-back along the polarization axis, the recoiling daughter nucleus will have zero momentum along the directions perpendicular to this axis, and on average less total energy than if the beta and neutrino were emitted in a parallel direction. Henceforth, daughter nuclei from a back-to-back decay as shown in Figure ?? will be described as ‘slow’ recoils. In terms of observables, this means that if the electric field is configured to point along one of the axes perpendicular to the polarization direction, then when the recoiling ion is swept away into a detector, the slow recoil’s hit position should be exactly along the projection of the polarization axis. Furthermore, the slow recoil’s time of flight should be in the middle of the time of flight spectrum, since other recoils will be emitted with momentum towards or away from the detector.

## I.4 Current Status

In June 2014, after several years of preparatory work beforehand (the author has been continuously involved with this project since 2010), approximately 7 days of beam time at TRIUMF was dedicated to the TRINAT  $^{37}\text{K}$  beta decay experiment. Approximately half of this data is suitable for use in this project. During this period, approximately 10,000 atoms were held within the trap at any given time. The cleaned

spectra show around 50,000 polarized beta-recoil coincidence events in total, divided among measurements at three different electric field strengths (535 V/cm, 415 V/cm, 395 V/cm).

A fit to simulation has shown that the data that has already been collected has sufficient statistical power to measure the *fractional* contribution of any polarized ‘new physics’ beta decay parameter (ie right-handed, scalar, and tensor currents within the weak interaction) to a sensitivity of  $\sim 2\%$  of its true value. Systematic limitations are still being assessed.

## Appendix J

# Things that Should Be Very Obvious

JB: Appendix J -> internal

JB: "You don't need anything about the atomic hyperfine structure in this thesis."

...

Me: yeah, that's fair. I probably should cut this whole section actually.

### J.1 Lifetimes and Half-Lives

Since different people use different notation to describe exponential decay of a physical quantity, it is useful to be able to relate two of the most common methods for describing the decay. We begin with the rate equation,

$$\frac{dN}{dt} = -\gamma N, \quad (\text{J.1})$$

where it is clear that the "rate" of decay must be  $\gamma N$ . If we initially have  $N_0$  of the quantity in question, then Eq. J.1 has as its solution

$$N(t) = N_0 e^{-\gamma t}. \quad (\text{J.2})$$

Note that the physical interpretation of  $\gamma$  is the "linewidth".

We'll wish to convert  $\gamma$  into other quantities of interest. In particular, we can

re-write the solution J.2 as

$$N(t) = N_0 e^{-t/\tau}, \quad (\text{J.3})$$

where  $\tau = 1/\gamma$  is referred to as the “lifetime”. Then, we find the half-life  $t_{1/2}$  by enforcing the fact that it is the time at which the number of remaining atoms is equal to half of what was originally present. Therefore,

$$N(t_{1/2}) = N_0 e^{-t_{1/2}/\tau} = \frac{1}{2} N_0 \quad (\text{J.4})$$

$$e^{-t_{1/2}/\tau} = 1/2 \quad (\text{J.5})$$

$$t_{1/2}/\tau = \ln(2). \quad (\text{J.6})$$

Thus, we see that

$$t_{1/2} = \ln(2) \tau, \quad (\text{J.7})$$

where  $\tau$  is the “lifetime” of the state, and  $t_{1/2}$  is its “half-life”.

## Appendix K

# Derivation of the $b_{\text{Fierz}}$ Dependence of the Superratio Asymmetry

Appendix KLM you have to pick what you want— I hope that's Appendix K— and remove the rest as you say they're "old". Appendix K could be moved to the end of Experimental Methods because it's absolutely critical and helpful!! but if you want to reference it there and leave it as an Appendix, it's up to you.

Recall the integrated JTW probability distribution for outgoing beta particles from Eq. (B.3):

$$\begin{aligned} d^3\Gamma(E_\beta, \hat{\Omega}_\beta) dE_\beta d^3\hat{\Omega}_\beta &= \frac{2}{(2\pi)^4} F_{\mp}(Z, E_\beta) p_\beta E_\beta (E_0 - E_\beta)^2 dE_\beta d^3\hat{\Omega}_\beta \xi \\ &\times \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \left( \frac{\vec{J}}{J} \cdot \frac{\vec{p}_\beta}{E_\beta} \right) \right]. \end{aligned} \quad (\text{K.1})$$

We note that the only angular dependence remaining in this equation is the dot product between the direction of beta emission and the direction of nuclear spin-polarization. This allows us to pull out a further factor of  $2\pi$  by choosing the axis of polarization as defining our coordinate system, and integrating over the “ $\phi_\beta$ ” coordinate. The result is a bit more friendly to work with:

$$d^2\Gamma(E_\beta, \theta) dE_\beta d\theta = W(E_\beta) \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \frac{v_\beta}{c} |\vec{P}| \cos \theta \right] dE_\beta d\theta, \quad (\text{K.2})$$

where  $\theta$  is the angle between the beta emission direction and the polarization direc-

tion, and is the only angular dependence that remains. Here, we have grouped the overall energy dependence into  $W(E_\beta)$ , so that

$$W(E_\beta) = \frac{2}{(2\pi)^3} F_\mp(Z, E_\beta) p_\beta E_\beta (E_0 - E_\beta)^2. \quad (\text{K.3})$$

We could also use this with the Holstein formulation, at least some of it. The point is, we can put \*anything\* that only depends on beta energy into  $W(E_\beta)$ . It doesn't matter, because it's already only integrable through numerical methods anyway – so we can't possibly make it worse.

In the TRINAT geometry with two polarization states (+/-) and two detectors (T/B) aligned along the axis of polarization, we are able to describe four different count rates, with different combinations of polarization states and detectors. Thus, neglecting beta scattering effects, we have:

$$r_{T+}(E_\beta) = \varepsilon_T(E_\beta) \Omega_T N_+ \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \frac{v}{c} |\vec{P}_+| \langle \cos \theta \rangle_{T+} \right] \quad (\text{K.4})$$

$$r_{B+}(E_\beta) = \varepsilon_B(E_\beta) \Omega_B N_+ \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \frac{v}{c} |\vec{P}_+| \langle \cos \theta \rangle_{B+} \right] \quad (\text{K.5})$$

$$r_{T-}(E_\beta) = \varepsilon_T(E_\beta) \Omega_T N_- \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \frac{v}{c} |\vec{P}_-| \langle \cos \theta \rangle_{T-} \right] \quad (\text{K.6})$$

$$r_{B-}(E_\beta) = \varepsilon_B(E_\beta) \Omega_B N_- \left[ 1 + b_{\text{Fierz}} \frac{m_e c^2}{E_\beta} + A_\beta \frac{v}{c} |\vec{P}_-| \langle \cos \theta \rangle_{B-} \right], \quad (\text{K.7})$$

where  $\varepsilon_{T/B}(E_\beta)$  are the (top/bottom) detector efficiencies,  $\Omega_{T/B}$  are the fractional solid angles for the (top/bottom) detector from the trap position,  $N_{+/-}$  are the number of atoms trapped in each (+/-) polarization state, and  $|\vec{P}_{+/-}|$  are the magnitudes of the polarization along the detector axis for each polarization state.  $\langle \cos \theta \rangle_{T/B,+/-}$  is the average of  $\cos \theta$  for *observed* outgoing betas, for each detector and polarization state combination. This latter term is approximately  $\pm 1$  as a result of our detector geometry, but contains important sign information. For a pointlike trap in the center of the chamber, 103.484 mm from either (DSSSD) detector, each of which is taken to be circular with a radius of 15.5 mm, we find that  $\langle |\cos \theta| \rangle_{T/B,+/-} \approx 0.994484$ , and is the same for all four cases. Note that a horizontally displaced trap will decrease the magnitude of  $\langle |\cos \theta| \rangle$ , but as it is an expectation value of an absolute value, all four will remain equal to one another. In the case of a vertically displaced trap,

Not quite true. Some strips are missing.

This is only true if we neglect (back-)scatter. This is not actually a good approximation. But we have pretty good simulations to give us the real numbers, anyway.

these four values will no longer all be equal, however it will still be the case that  $\langle |\cos \theta| \rangle_{T+} = \langle |\cos \theta| \rangle_{T-}$ , and  $\langle |\cos \theta| \rangle_{B+} = \langle |\cos \theta| \rangle_{B-}$ .

In the case of the present experiment, we note that  $|\vec{P}_+| = |\vec{P}_-|$  is correct to a high degree of precision.

Is that definitely true,  
or is it only true to  
lowest order?

## Appendix L

# Derivation of the $b_{\text{Fierz}}$ Dependence of the Superratio Asymmetry (Old)

See content at Appendix (K). After reading that stuff, continue [here](#).

For simplicity, we will henceforth assume that the trap is vertically centered, and take  $|\vec{P}_+| = |\vec{P}_-|$ .

The trap is \*not\* centered, but the polarizations in the two states are equal, to a very high level of precision. More than we need for  $b_{\text{Fierz}}$  anyway, and probably more than we'd need for  $A_\beta$ .

We also define the following:

$$A' = A'(E_\beta) \equiv A_\beta \frac{v}{c} |\vec{P}| \langle |\cos \theta| \rangle \quad (\text{L.1})$$

$$b' = b'(E_\beta) \equiv b_{\text{Fierz}} \frac{mc^2}{E_\beta}, \quad (\text{L.2})$$

and choose a coordinate system in which the + polarization state is, in some sense, ‘pointing up’ toward the top detector, such that

$$\langle \cos \theta \rangle_{T+} \approx +1 \quad (\text{L.3})$$

$$\langle \cos \theta \rangle_{B+} \approx -1 \quad (\text{L.4})$$

$$\langle \cos \theta \rangle_{T-} \approx -1 \quad (\text{L.5})$$

$$\langle \cos \theta \rangle_{B-} \approx +1. \quad (\text{L.6})$$

This allows us to rewrite the four count rates in simplified notation, as:

$$r_{T+} = \varepsilon_T N_+ (1 + b' + A') \quad (\text{L.7})$$

$$r_{B+} = \varepsilon_B N_+ (1 + b' - A') \quad (\text{L.8})$$

$$r_{T-} = \varepsilon_T N_- (1 + b' - A') \quad (\text{L.9})$$

$$r_{B-} = \varepsilon_B N_- (1 + b' + A') . \quad (\text{L.10})$$

We further define the ‘superratio’,  $s$ , to be:

$$s = \frac{r_{T-} r_{B+}}{r_{T+} r_{B-}}. \quad (\text{L.11})$$

We are now in a position to define the ‘superratio asymmetry’,  $A_{\text{super}}$ , as

$$A_{\text{super}} = A_{\text{super}}(E_\beta) \equiv \frac{1 - \sqrt{s}}{1 + \sqrt{s}}. \quad (\text{L.12})$$

This is explicitly an experimental quantity that is measured directly by the above combination of count rates.

Writing the superratio out explicitly in terms of  $A'$  and  $b'$ , factors of  $\varepsilon_{T/B}$  and  $N_{+/-}$  cancel out entirely, and we find that

$$s = \frac{(1 + b' - A')^2}{(1 + b' + A')^2}. \quad (\text{L.13})$$

From here it is immediately clear that in absence of other corrections (*e.g.* backscattering, unpolarized background, ...), if  $b' = 0$  it follows that  $A_{\text{super}} = A'$ . In the case where  $b' \neq 0$ , we find that

$$A_{\text{super}} = \frac{A'}{1 + b'} \quad (\text{L.14})$$

$$\approx A' (1 - b' + b'^2), \quad (\text{L.15})$$

where we have utilized the assumption that  $b' \ll 1$ . Thus,

$$A_{\text{super}} \approx A_\beta \frac{v}{c} |\vec{P}| \langle |\cos \theta| \rangle - A_\beta \frac{v}{c} |\vec{P}| \langle |\cos \theta| \rangle \left( b_{\text{Fierz}} \frac{mc^2}{E_\beta} \right) + A_\beta \frac{v}{c} |\vec{P}| \langle |\cos \theta| \rangle \left( b_{\text{Fierz}} \frac{mc^2}{E_\beta} \right)^2. \quad (\text{L.16})$$

# Appendix M

## Some Corrections to the Superratio Stuff (Old)

We consider further modifications to the rates described in Eqs. (K.4-K.7). In particular, we consider the effect of non-identical polarization magnitudes for the two polarization states and a trap displaced from the center.

We define for the polarization states:

$$P \equiv \frac{1}{2} (|\vec{P}_+| + |\vec{P}_-|) \quad (\text{M.1})$$

$$\Delta P \equiv \frac{1}{2} (|\vec{P}_+| - |\vec{P}_-|) \quad (\text{M.2})$$

and immediately find that

$$|\vec{P}_+| = P + \Delta P \quad (\text{M.3})$$

$$|\vec{P}_-| = P - \Delta P. \quad (\text{M.4})$$

Further, we also define:

$$\langle |\cos \theta| \rangle_T \equiv \langle |\cos \theta| \rangle_{T+} = \langle |\cos \theta| \rangle_{T-} \quad (\text{M.5})$$

$$\langle |\cos \theta| \rangle_B \equiv \langle |\cos \theta| \rangle_{B+} = \langle |\cos \theta| \rangle_{B-}, \quad (\text{M.6})$$

and

$$\langle |\cos \theta| \rangle \equiv \frac{1}{2} (\langle |\cos \theta| \rangle_T + \langle |\cos \theta| \rangle_B) \quad (\text{M.7})$$

$$\Delta \langle |\cos \theta| \rangle \equiv \frac{1}{2} (\langle |\cos \theta| \rangle_T - \langle |\cos \theta| \rangle_B). \quad (\text{M.8})$$

It immediately follows that

$$\langle |\cos \theta| \rangle_T = \langle |\cos \theta| \rangle + \Delta \langle |\cos \theta| \rangle \quad (\text{M.9})$$

$$\langle |\cos \theta| \rangle_B = \langle |\cos \theta| \rangle - \Delta \langle |\cos \theta| \rangle. \quad (\text{M.10})$$

With this new set of variables defined, we can re-write Eqs. (K.4-K.7) as

$$r_{T+}(E_\beta) = \varepsilon_T(E_\beta) N_+ \left[ 1 + b' + (A_\beta \frac{v}{c})(P + \Delta P) (\langle |\cos \theta| \rangle + \Delta \langle |\cos \theta| \rangle) \right] \quad (\text{M.11})$$

$$r_{B+}(E_\beta) = \varepsilon_B(E_\beta) N_+ \left[ 1 + b' - (A_\beta \frac{v}{c})(P + \Delta P) (\langle |\cos \theta| \rangle - \Delta \langle |\cos \theta| \rangle) \right] \quad (\text{M.12})$$

$$r_{T-}(E_\beta) = \varepsilon_T(E_\beta) N_- \left[ 1 + b' - (A_\beta \frac{v}{c})(P - \Delta P) (\langle |\cos \theta| \rangle + \Delta \langle |\cos \theta| \rangle) \right] \quad (\text{M.13})$$

$$r_{B-}(E_\beta) = \varepsilon_B(E_\beta) N_- \left[ 1 + b' + (A_\beta \frac{v}{c})(P - \Delta P) (\langle |\cos \theta| \rangle - \Delta \langle |\cos \theta| \rangle) \right] \quad (\text{M.14})$$

and the superratio becomes

$$s = \frac{(1 + b' - (A_\beta \frac{v}{c})(P - \Delta P) (\langle |\cos \theta| \rangle + \Delta \langle |\cos \theta| \rangle)) (1 + b' - (A_\beta \frac{v}{c})(P + \Delta P) (\langle |\cos \theta| \rangle - \Delta \langle |\cos \theta| \rangle))}{(1 + b' + (A_\beta \frac{v}{c})(P + \Delta P) (\langle |\cos \theta| \rangle + \Delta \langle |\cos \theta| \rangle)) (1 + b' + (A_\beta \frac{v}{c})(P - \Delta P) (\langle |\cos \theta| \rangle - \Delta \langle |\cos \theta| \rangle))} \quad (\text{M.15})$$

where  $\varepsilon_{T/B}$  and  $N_{+/-}$  still completely cancel out. After a bit of algebra, this simplifies to:

$$s = \frac{(1 + b' - A' + A_\beta \frac{v}{c} \Delta P \Delta \langle |\cos \theta| \rangle)^2 - (A_\beta \frac{v}{c})^2 (\Delta P \langle |\cos \theta| \rangle - P \Delta \langle |\cos \theta| \rangle)^2}{(1 + b' + A' + A_\beta \frac{v}{c} \Delta P \Delta \langle |\cos \theta| \rangle)^2 - (A_\beta \frac{v}{c})^2 (\Delta P \langle |\cos \theta| \rangle + P \Delta \langle |\cos \theta| \rangle)^2} \quad (\text{M.16})$$

Note that in the superratio  $\Delta P$  and  $\Delta \langle |\cos \theta| \rangle$  have cancelled out to first order, and the remaining dependencies are quadratic only. Eq. (M.16) is exact, but still a huge enough expression to be pretty unwieldy to work with. Let's introduce some

shorthand notation to make this less painful:

$$A'' := A_\beta \frac{v}{c} \quad (\text{M.17})$$

$$c := \langle |\cos \theta| \rangle \quad (\text{M.18})$$

$$\Delta c := \Delta \langle |\cos \theta| \rangle \quad (\text{M.19})$$

$$r' := 1 + b' + A_\beta \frac{v}{c} \Delta P \Delta \langle |\cos \theta| \rangle \quad (\text{M.20})$$

In this notation, we find that

$$s = \frac{(r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2}{(r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2}, \quad (\text{M.21})$$

which hurts a lot less to look at. Then, the superratio asymmetry is

$$A_{\text{super}} = \frac{1 - \sqrt{\frac{(r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2}{(r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2}}}{1 + \sqrt{\frac{(r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2}{(r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2}}} \quad (\text{M.22})$$

$$= \frac{\left[ \sqrt{(r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2} - \sqrt{(r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2} \right]^2}{\left[ (r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2 \right] - \left[ (r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2 \right]} \quad (\text{M.23})$$

$$= \frac{\left( \begin{array}{l} 2 \left[ (r')^2 + (A')^2 - (A'')^2 ((P \Delta c)^2 + (\Delta P c)^2) \right] \\ -2 \left[ (r' + A')^2 - (A'')^2 (\Delta P c + P \Delta c)^2 \right]^{1/2} \left[ (r' - A')^2 - (A'')^2 (\Delta P c - P \Delta c)^2 \right]^{1/2} \end{array} \right)}{4 \left[ (r' A') - (A'')^2 (P c \Delta P \Delta c) \right]} \quad (\text{M.24})$$