Jess Brewer's comments on Hassan Saadaoui's PhD thesis

Most of what comes below is critical, because that's how we make things better. So I just want to start off by saying that this was a nice easy read for the most part, and obviously represents some excellent work! — Jess

1. **Chapter 1: Introduction**

   (a) Page 1 (and probably throughout the thesis): you can have many "phenomen**a**", but only one "phenomen**on**".

   (b) The past tense of "lead" is "led".

   (c) I am seeing a lot of grammatical errors, in particular the mixing of singular with plural; I will stop listing them after p. 1, as I don't have time to play copy editor.

   (d) ** The problem with Fig. 1.1 is that it does not correctly predict the state of a field-cooled superconductor with strong flux pinning. As T is decreased from above T_c in a field $B < B_{c_1}(0)$, the first superconducting phase reached will be the vortex state at some temperature T_v where the $B_{c_2}(T)$ curve crosses B. It remains in the vortex state until a lower temperature T_M where the $B_{c_1}(T)$ curve crosses B. It is then nominally in the Meissner state, *but*, if the pinning is strong enough at T_M, the vortices will remain "frozen in". This is how early "decoration" experiments were accomplished in fields well below B_{c_2}. So the question is, how strong is the pinning at T_M? See also p. 34 and pp. 40-41. Also Fig. 2.4 on p. 64 and especially on p. 66!

   (e) * Pages 2 & 29: Can you explain in simple terms why, *in general*, local magnetic fields in ZF are considered evidence for spontaneous time-reversal symmetry breaking?

   (f) Page 8: Compl**e**mentary, not compl**i**mentary.

   (g) Page 10 (Fig. 1.5): is the angular distribution really an off-center circle? What would it look like for 100% asymmetry?

   (h) Page 11, Eq. (1.3): Any particular reason why you used the **ratio** of L & R to construct the asymmetry, rather than the more common (at least recently) form

$$\frac{L(t) - R(t)}{L(t) + R(t)}\ ?$$

On p. 80 you refer to $(F - B)/(F + B)$. Did you use one form in the low-field spectrometer and the other in the high-field one?

(i) Page 13: The comment about ^{11}Be development is sort of stapled onto the end of a paragraph comparing β-NMR and μSR. Why?

(j) * Page 14: You say that μSR "only measures in the time domain, and a Fourier transform is needed to find the field distribution in the frequency domain." Do you recognize any *advantages* of a time-domain measurement over a frequency-domain one?

(k) Page 17 (Fig. 1.8): You show a gap between the AF and SC phases on the hope-doped side of the phase diagram. Do you believe this?

(l) * Page 23: Can you explain in simple terms why time reversal turns an order parameter into its complex conjugate?

(m) Page 27: Quasiparticle Reflection yadda yadda: do you actually understand this stuff? I sure don't.

(n) Throughout: Do you know about the `\boldmath` command in LaTeX?

(o) Page 35: "Four**n**ier components"?

(p) Page 38 (Fig. 1.15): The caption does not seem to describe the Figure. There are no (a), (b) or (c) parts!

(q) * Page 44 (Fig 1.19): The third moment is related to the skewness, but they are not the same thing. Can you explain the difference?

Subsequent Chapters: Who actually *wrote* these papers?

2. Chapter 2: Search for TRSB in YBCO

(a) Page 60: Was the frequency-randomized pulsed RF technique your invention?

(b) Page 63 (Fig 2.3): Why does the caption refer to the open circles as just "Ag film" when the legend shows them as "Ag/STO"?

(c) * The arguments on p. 66 for why the observed broadening can't be due to TRSB would seem also to argue that TRSB cannot be detected by this technique, even if it present. Would you agree?

3. **Chapter 3: VL in YBCO**

(a) Page 72: Missing reference *re* NMR.

(b) * Pages 75-77: Why are you convoluting with a *Lorentzian* disorder of width Δ_D rather than a *Gaussian* disorder of width σ_D (as everyone else has done, starting with Brandt)? Do you have reason to believe that this is a better model of the effects of vortex lattice disorder? Have you actually modeled the $p(B)$ produced by the pattern shown in the inset of Fig. 3.1(b)? This would seem to be implied by Fig. 3.2, but I don't see any explanation for what is meant exactly by D in that Figure. . . . Aha! You do describe this, but not until p. 78, 3 pages after Fig. 3.1 is displayed. You need to coordinate Figures and text better! In any case, it still is not so clear to me how D translates into Δ_D, or why the result is Lorentzian rather than Gaussian.

(c) Pages 77-78: You refer to Figs. 3.2(a) and 3.2(b) when you clearly mean Figs. 3.1(a) and 3.1(b). This doesn't help the confusion I just mentioned.

(d) * Pages 78-79 and Fig. 3.2: You translate the extra broadening (due to disorder on a scale D) into a revised T-dependence, but I don't see any mention of the effect of *depinning* as a function of increasing *temperature* — only of depinning caused by increasing *field*. We know that depinning sets in around $0.7T_c$ (lower in higher field) so this would surely modify the $\sigma(T)$ curves in Fig. 3.2. I am inclined to suspect that the general conclusion is, "If you don't use high quality, aligned single crystals, you might get just about anything you can imagine!"

(e) Page 86 (Fig. 3.6): The caption says the dashed lines are ideal VL lineshapes convoluted with a Lorentzian, but this appears to be the case only for the film sample.

(f) Page 87: "Eq. (3.9))"?

(g) Pages 89-90 seem to make the crucial point of this paper: that the non-d-wave T-dependence of the linewidth can be explained in terms of VL disorder and is therefore not evidence for s-wave SC, as a handful of intransigent folks keep insisting. I believe this is correct, but I'm not convinced that the particular form of said disorder effects show here is by any means exhaustive.

4. **Chapter 4: PCCO**

(a) Pages 100-103: The paramagnetic (positive) frequency shift in PCCO below T_c is touted as distinctive, and so it is; but not necessarily unique: Fig. 3.4 on p. 83 looks to me as if the large diamagnetic (negative) shift in YBCO below T_c is suppressed at the lowest T (4.5 K) relative to that at 20 K. This would suggest a paramagnetic contribution as $T \to 0$, but this is never mentioned that I can recall. Is it just my imagination? Obviously you have the fits, so you'll know for sure.

5. **Chapter 5: Summary & Conclusions**

(a) ** Page 106: Of the reasons listed for employing β-NMR in a Ag overlayer, most are pragmatic issues (basically, it wouldn't work inside the SC) but (*ii*) seems to me to be the central focus of this work: outside the SC one quickly loses the field irregularities due to the vortex lattice itself, leaving only those due to larger-scale disorder effects. Loss of information about the VL itself seemed like a disadvantage to me until this sank in: when you can see the VL, you *can't* see the disorder (or not very directly). So this is the main *strength* of this technique. You may have said this earlier, but as I said, it only sank in for me on this page. I think it deserves a higher profile. You could have entitled your thesis, "Studies of large-scale VL disorder using β-NMR in overlayers" (or something a little catchier) to emphasize this advantage. Modesty is rarely rewarded in science.

(b) Page 107: The caption to Fig. 2.4 on p. 64 states that the (100) film was measured after ZFC, giving $\Delta \propto B_0$, but the (110) film was only measured after FC, and only at two low fields, so the statements that $\Delta \propto B_0$ for both, and that (110) and looks the same as (100), are rather poorly justified, IMHO.

(c) * Pages 107-108: That you get different results with a poor ohmic contact between the SC and the metal overlayer from those with a good ohmic contact is, as you say, expected; however, it is an overly generous extrapolation from this to say that this rules out the proximity effect as an important source of your observed effects. Couldn't you still have a reduced, or otherwise altered, proximity effect?