**Response to examiner’s comments**

J Ross

Below are the compiled suggestions from the three examiners’ reports, omitting their summaries of the thesis itself. My responses are interleaved with references to the locations where I have made changes in the text. The minor changes such as typographical errors are collected in a table at the end of this document.

**General comments**

**Examiner 1**

While I understand the references in the overview would be out of order, I don’t understand why the references start at [144] in chapter 1, personally I dislike this, as it feels clumsy. I would suggest re-ordering these, unless there is some underlying reason that I’ve not picked up on.

**JR:** This is because the current bibliographic style is to sort by first-author surname. I find this much better when seeking references from within a bibliography. Admittedly, being able to search PDFs makes this less advantageous, and I do agree with that is clunky to read in the present form. Therefore, I have changed the referencing style.

Personally, I think chapter 3 is not required, as it adds no new science. Instead, it shows that the student worked on another project for some time, which is unnecessary considering the excellent results. I feel it breaks the overall flow of the thesis and could be relegated to an appendix or excluded entirely without harming the outcome of the thesis. Whether to change this or not is left to the author and supervisor.

**JR:** I have moved the entire chapter to an appendix after considering this remark in contrast to the suggestion of reviewer 3 to break up the chapter. I think the first two of sections are only in service of the latter two, and so are not required to set the stage for the other chapters in the thesis. The content is sufficiently self-contained (and the rest of the thesis sufficiently independent) that the chapter is better moved to an appendix in its entirety. I have rearranged the abstract and précis (pages 12 & 13) to reflect the new structure. Note that this means the page and section numbers used by the examiners are no longer correct. When I indicate the in-text location of a change, I refer by default to the updated manuscript, unless where otherwise indicated. Further, I have promoted the Overview to a numbered chapter. This means that chapters 1 and 2 are now chapters 2 and 3, and chapters thereafter have the same indices.

Throughout the thesis, the author is missing a space between numbers and their associated units, e.g. 50 nm. This should be corrected.

**JR:** I have corrected these instances wherever I found them.

Page 50 figure 2.2: This figure is unclear, other than showing the figure four configuration it is unclear in the context of where the beams come from, go to etc. Better labelling is required

**JR:** On reflection, I don’t think this figure is especially helpful. Rather than puzzle over how to improve it, I have simply removed it.

**Examiner 2**

Page 50: In the footnote the statement is a bit confusing as worded – it seems to imply that a collimated beam will converge to a focus.

**JR:** No beam is perfectly collimated – all beams diverge eventually. The only question is whether a given beam shrinks to a minimum waist after the final optic during free propagation. In any case, this distinction adds little to the thesis, so I have removed the footnote.

page 54: I would add a figure here from the Ref. [108] showing the magnetic coil configuration. Given the other illustrative diagrams in this chapter, it seems out of place to leave out a diagram of the magnetic trapping, especially given its prominence in the later results chapters.

**JR:** I have added the diagram from Dall & Truscott 2007 (Fig 3.3, page 55, with permission)

page 133: I thought that similar to the previous chapter, where the publication is cited, you should mention the arXiv paper here in a footnote. In particular, I thought it necessary to highlight the “equal contributions” of the first and second authors (this could also apply to the original listing of the publication at the beginning of the thesis as well).

**JR:** The examiner makes a good point and I haveadded similar references to each chapter as appropriate. The list of publications in the front matter is now up to date.

page 136: “We achieve a 20-fold improvement in the precision over the sole previous measurement”. It took me some time to realise that these were earlier results from the same group (although I was familiar with the previous measurement of the tuneout wavelength done at ANU). This should be better highlighted here (it does end up being more transparent in the conclusion chapter).

**JR**: Done: The text (top of page 100) now reads: *… improvement in the precision over the sole previous measurement (which was also undertaken in the ANU He\* lab) and make a definitive comparison …*

page 141/142: General comment, as above: It would be great to emphasize the experimental improvements that have led to the improvement in this measurement, as it is not very transparent how these measurements are a refinement of the previous ones at ANU. As written, it seems like there is worry that it will make the results of this section appear incremental – but that is the essence of precision measurement anyways, and it is clear from the subsequent sections that a meticulous study was undertaken. This could be done with a few brief sentences in these sections (or on page 136), similar to (but with a bit more detail) to how this is discussed in the conclusion section. As written at the moment, the detail of each of the techniques is well documented, but there is no high-level summary of how these collective improvements that led to the 20-fold improvement in the measurement. As a final comment, the improvement is alternately noted as “25 times” in the conclusion, perhaps this should be consistent throughout.

**JR:** I have added a new subsection in the conclusion (5.5.1 Improvements upon past measurements, page 143) which discusses the factors leading to an improved precision and made sure to clarify where relevant that the previous measurement was done in the same laboratory (c.f. the previous comment). I double-checked the calculation of the improvement in performance and was surprised to find that neither of these figures are accurate – the true improvement was more like a 15-fold improvement. I have amended the text accordingly (pages 100 and 143).

page 190: section 6.2.1: Throughout this section I was unclear on how the condensate is excluded from the fitting analysis. Perhaps I have missed this, but some clearer explanation is needed.

**JR**: The bounds were determined manually. I have added text to this section to explain (now 6.2.1, page 159).

page 208: Figure 6.8: From the text, it appears that these results are from the

theory collaborators. They should be attributed in the figure caption.

**JR:** I have acknowledged Piotr’s contribution of the simulation work at the start of the section, as it is essentially a summary of his part of the work (footnote 8 on section 6.3, page 174)

There are several errors in the bibliography: [Details presented in table below]

**JR:** I have corrected these, although in one instance the examiner mistook two similarly-named papers for a duplicate.

**Examiner 3**

**chapter 1**

I believe some discussions of the real and the imaginary parts of the polarizability would be appropriate, especially their functional dependence on light detuning from an atomic resonance.

**JR:** The roles of the real and imaginary parts of the polarizability as determinants of the interaction (dipole) energy and scattering rates, including functional forms, are already present on pp. 23-24.

An atomic energy level diagram with singlet and triplet states would be very appropriate in this section as a reference point to fall back to.

**JR:**  I have added a diagram, Fig. 2.1, in section 2.2 (page 27).

A more diagrammatic approach would probably be more useful.

**JR**: Well, sure. However, I believe it is sufficient to add only the level diagram, as the examiner has not made any other specific requests.

**chapter 2**

The caption for Figure 2.1 is too long and very hard to follow- in particular in the absence of any subplot label, I did not know which of the beamlines I should be looking at while reading it.

**JR:** I have moved most of the caption to the main text. The figure has also been reworked for clarity: The beamlines have been split into distinct figures (3.1 and 3.2, pages 48 and 49 respectively) and given their own captions.

I also suggest that the theory of laser cooling given in the subsection named ‘Cooling Techniques’ is moved to Chapter 1

**JR:** Nice idea. The content is now in section 2.4.1 (page 42).

Although generally quite wordy, and there would certainly be scope to use diagrams to explain things better and save words, I still enjoyed reading this section [2.3].

**JR**: Certainly – as above, though, I have just added what the examiner implies is essential.

**Chapter 3**

Unfortunately I think that this chapter undermines the flow of the thesis. The motivation for optical lattices is given is the first two sections, which I think should have found their place in chapter 1 (theoretical background). Section 3 describes the painstaking work undertaken to realise the optically trapped helium atoms with a great deal of experimental details, while section 4 gives a brief outlook. I think sections 3 and 4 of this chapter would be best given as a chapter before the Conclusion (Chapter 7), as a prelude to the future experiments in this setup

**JR**: Moved to appendix as discussed above.

**chapter 4**

In the measurement technique section, it is not clearly stated that probe and the pump beams are on simultaneously to induce atom losses. I think it should be made clear. Also the duration of the pulse (100ms) seems excessively long to me for the scattering rates (is the pump beam resonant?). It would be good to check.

**JR:** I have clarified a parenthetical statement in the second paragraph of section 4.2 (page 82) which now states that the beams were on simultaneously. The exposure times may seem long, but they were tuned empirically to obtain the desired signal strength. This is mentioned at the start of the second-last paragraph in section 3.2 (page 83). The main reason for such low absolute excitation rates is that the pump beam was applied with extremely low power (O(nW) across a beam with a spot size of around 0.5cm), and thus excitation into the n=5 states were limited by the population of the intermediate state (2 3P2).

Is there an advantage of the measurement technique used here over direct optical detection of the atom number post/prior to evaporation? This should be discussed**.**

**JR:** I have discussed this in a new paragraph at the end of section 4.4 (page 93-94).

Are BECs required, or just a convenient means to reduce broadening? This should be discussed.

**JR:** I agree that the choice to use BEC or thermal clouds would warrant discussion were the experiment performed with a BEC as the probe target, but this is not the case here. In fact, in the described experiment the clouds were illuminated prior to evaporative cooling.

‘Calibration shots’ are mentioned. The sensitivity of the final atom number with respect to the initial atom number can be tested by varying the former while keeping the evaporation sequence fixed. Is that what is meant by the calibration? Please explain.

**JR:** The purpose of the calibration shots is to compensate for drift in the atom number. This is explained later in the section, but I have brought the text forward (to page 82):

Can anything be said about the line strengths, maybe even the relative line strengths, of the transitions from these measurements?

**JR:** I do not think so. We do not have a model of the signal strength in terms of photon absorption, and indeed are not even certain that is it linear. Therefore, the signal amplitude is not readily related to the absorption probability, which could be ultimately used (along with some assumptions about the laser line shape and) to calculate the oscillator strengths. This was mentioned in passing in section 4. but was not explicitly connected to the difficulty of measuring line strengths. I have added text to this part (page 85)

**Chapter 5**

The theory of the polarizability near the tune out point and its polarization dependence is given in two subsections, which I found to damage the flow of the chapter. I suggest that these discussions are either moved to chapter 1 (theoretical background) or given in a separate section in this chapter

**JR** I have opted for the latter, as this theory is not really required for the other chapters (whereas chapter 1 is meant to present material that is used in multiple chapters). The subsection 5.2.3 (Polarizability near f\_TO) is now included in the Background section (5.1), while the Experimental implementation section (5.2) now proceeds directly from measuring the trapping frequency (5.2.2) to Measuring Polarization.

It is not initially obvious why a purely optical trap, rather than a combined hybrid trap is more suited for these measurements. My understanding is that this is related to the spin-sensitive pulsed detection of outcoupled atoms from the magnetic trap. This should be motivated in the measurement technique section.

**JR** Given the examiner clearly understood that a combined trap was used in the experiment, I suspect the comment to the contrary here is a slip of the pen. I have added text to the end of the first paragraph of section 5.2 (page 117) to explain the motivation.

Similarly the reason why BECs are used as opposed to cold thermal samples in these measurements should be explained.

**JR:** I have added text in section 5.2.1, just before the subheading ‘Measuring the trapping frequency’ (page 121). The text explainsthat the precision of the pulsed atom laser-based measurement is mainly limited by the damping of in-trap oscillations, which are much more significant for thermal clouds (see e.g., Henson *et al.*, Optics Express **30** 13252 (2022), <https://doi.org/10.1364/OE.455009>). This is a major factor in the precision of the experiment, thus using thermal clouds is certainly possible but would compromise the accuracy of the measurement

Is the anharmonicity of the dipole trap (inevitable for shallow dipole traps) a relevant factor?

**JR:** This is certainly an interesting question which we did study in several ways (at least theoretically) in the course of the work. There are many ways in which the Gaussian profile eventually becomes relevant (in the cases of very large detuning, beam power, or misalignment), but in short, the answer is ‘not to a significant level’. It would be a substantial (although stimulating) exercise to detail these factors and show that, ultimately, the approximations employed in the analysis do not confer a systematic shift in the final estimate of the tune-out point for a given set of Stokes parameters*.* Thereafter, the profile is not a relevant factor as the analysis is conducted only on the tune-out points as a function of Stokes parameters.

I believe it is unnecessary to extend the thesis to cover this topic for these reasons. I note that examiner already comments that “the thesis meets the international standard both in the contents of the research work and the presentation thereof”, and furthermore offers this question as a suggestion, not an essential improvement.

Why was the probe dipole beam power not stabilized?

**JR:** It was, and this is stated in several places (eg pages 162 in the original thesis, now page 116, and in the description of the laser system in Chapter 2). Nonetheless, I have added a reference to the laser system schematic (Fig 2.4) and added text in the Measurement Technique section: *In our measurement, we quantified the shift in the oscillation frequency of a harmonically trapped BEC when the magnetic trap was overlapped with a power-stabilized probe laser beam* (page 116)

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| Section 5.2.2: The first sentence is a bit unintelligible.  **JR** Fair enough. I absorbed this section (measuring the trapping frequency) as a subsection of the previous one (Experimental implementation), which does away with the need for the bridging sentence. | |
| Section 5.3.1. The exclusion criteria. Only one in 10000 shots was rejected, so was this necessary or useful?  **JR:** It is plausible that this was of marginal to no benefit, however we did not compare the results of running the analysis with and without the exclusion. In the end, it is likely that most of the failed shots would have been detected by the outlier-removal step in the fitting procedure. I have added comments to this effect at the end of section 5.3.1 (page 127). | | |
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**Chapter 6**

One small comment about physical models. The section of the power law fitting, perhaps unknowingly, gives the impression that one has to rule out all of the innumerably large alternative functional forms to establish one single functional form. This of course is not true, as there is usually a very small of set of physically motivated functional form.

**JR:** Indeed, this was not the intended message of this section, and I have removed the ambiguous text. I have opted to simplify rather than embellish the content because the argument I meant to make is not essential to the chapter.

The term “far-field” should be defined somewhere early.

**JR** I have added text to the second paragraph of the chapter (page 150) defining the term.

**Minor corrections: Figures**

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| Fig.2.1 could be broken into two, each with a smaller caption.  **JR** Done | |  |
| Fig 3.10 the caption and the diagram are too close to each other  **JR:** Fixed | | |
| Fig. 4.3 Caption “Theory lines” do you mean the dotted lines? Also what are the vertical bars? Please mention that in the caption.  **JR:** No, the bars are the theory lines. This got dropped from the legends, so I have added text to the caption of figure 4.2 which has the same issue: Vertical bars indicate theoretical predictions for the line centres, with the uncertainty (mostly due to uncertainty in the magnetic field) indicated by the width of the bars. | | |
| Fig 4.3, 4.4, instead of cumbersome omega – 744…, could it be omega – omega\_m, with omega\_m defined in the caption and text to be 744…?  **JR:** It could, but I’m not going to change these figures as this is a stylistic change. | | |
| Fig. 4.5. (b) axis label, there is a THz, MHz mix-up.  **JR:** Actually, there is no mix-up, the axis label is correct – it shows the difference from 276THz on a MHz scale. This is potentially confusing, so I’ve clarified in the caption. |  |  |
| Fig.4.6 The description in the main text could be more elaborate.  **JR:** I have expanded the final paragraph in Section 4.4 to provide more detail. | | |
| Fig 5.5 \omega\_{net} is presumably an angular frequency, so one needs a 2\pi there.  **JR:** Done | | |
| Fig 6.1: the caption encroaches into the diagram.  **JR:** Fixed |  |  |
| Fig 6.1: where is the inset stated in the caption?  **JR:** It got removed. Referred to fig 6.2 instead. |  |  |
| Fig 6.2: Vertical scale unit should it be um^-3?  **JR:** No – the units of k are um^-1. Thus the dimensions of a 3D region in k-space are um^-3. The number of particles per k-volume is therefore 1/(um^-3) = um^3. Yes, it’s confusing. I’ve added text to clarify this as a footnote to the opening paragraph of 6.2.1. |  |  |

**Minor corrections: Text**

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| --- | --- | --- |
| Page | Examiner comment | Response or text change |
| 0 | Page numbering starts on “0” instead of “1”, seems odd. | 0 is even |
| 0 | Spectrometry --- > spectrometry. | Fixed |
| 3 | Footnote 3, “Planck” not capitalised. | Fixed |
| 11 | extra space after He\*. | Fixed |
| 17 | “Hydrogen”, “Helium” written with caps, should be lowercase. | Fixed |
| 26 | footnote – “we consider -- Doubly” --> “we consider. Doubly…”. | Fixed |
| 28 | “coefficiencts” misspelled. | Fixed |
| 39 | typo in GPE, should be “i \hbar” on left, rather than “-\hbar”. | Fixed |
| 44 | “bell-type” --> “Bell-type”. | Fixed |
| 44 | “413nm” --> “413 nm”. | Fixed |
| 44 | line 5, “Bell-type” typo | Fixed |
| 45 | line 4, “ molecular pumps” (extra fullstop) | Fixed |
| 45 | line 5, fullstop missing | Fixed |
| 45 | line 10, “faraday” should be Faraday. There are several more instances of this typo in this chapter. | Fixed |
| 45 | line 13, LVIS should be defined. | Fixed |
| 50 | In the footnote the statement is a bit confusing as worded – it seems to imply that a collimated beam will converge to a focus. | FIxed |
| 51 | Eq. (2.2), the subscript “lab” should not be italicised. | Fixed |
| 53 | Missing space between number and unit in “2mm” and “1cm”. | Fixed |
| 58 | In the footnote, “analogoous” misspelled. | Fixed |
| 59 | Bad latex reference resulting in “??” partway down the page. | Fixed |
| 59 | line “…chapters ??..” | Fixed |
| 63 | “19.8eV” missing the space before the unit. | Fixed |
| 80 | Latex float issue(?) leading to a large space in the page. | Fixed |
| 84 | similarly, a large space here. | Fixed |
| 88 | “One of the most accessible lattice systems to realize with ultracold atoms is the Bose-Hubbard model.” – the wording is strange, just a typo(?) Bose-Hubbard is not a lattice system? Reword. | Replaced with 'One of the most accessible many-body systems to realize with ultracold bosons in an optical lattice is the Bose-Hubbard model.' |
| 91 | “a.k.a.” | Fixed |
| 95 | “faraday” should be “Faraday” | Fixed |
| 95 | Is ETP the name of the manufacturer of the part? | The former, amended. |
| 103 | “..1 0 ms delay ..” extra space in 10 | Fixed |
| 104 | “Unforunately..”, second para, second line, typo | Fixed |
| 104 | “ NIM crate..” Is NIM defined? | Removed acronym and left only references to the system components |
| 106 | First sentence: a larger range of momentum states does not necessarily mean a large phase space density. | Given a fixed atomic density, the statement is accurate, but I have amended to "Deeper optical dipole traps can contain more atoms at a given temperature by virtue of trapping over a larger range of particle momenta" |
| 107 | “dipole fibers” is too colloquial. | "The dipole beams were first aligned to overlap with the magnetic trap by piping resonant light (at 1083 nm) through the dipole beam delivery fibres instead of 1550 nm." |
| 119 | “…photon scattering signal…” what is it? | It's defined later in the sentence. Still, changed to "In both cases the effect of photon scattering manifests as a reduction of the total trapped final number $N$ relative to the final number $N\_c$ in calibration shots." |
| 121 | The issue of the limitation of the dynamic range should be explained. Also the contradictory issues of SNR and saturation can be dealt with two different sets of measurements, right? | I have added more detail to the first paragraph of section 4.3 explaining the upper- and lower-bounds on detectable signal, and a comment agreeing with the examiner's suggestion. |
| 121 | “..transduction from photon scattering to…”, the word “transduction” means something different. | I think this is fine, but have changed to "The functional relationship between photon scattering and atom loss via evaporative cooling is complicated and not linear." |
| 126 | uK in Roman font | Fixed |
| 130 | The RF linewidth of the crystal oscillator of 300kHz sounds excessively large. | Fair assessment. It was actually in the RF driver output. I've added a sentence "In the first case, the linewidth is a result of frequency instability in the RF drive generation system. In the second, a newer drive system was in use which afforded better performance." It might be worth fixing this as it could affect in to the in-trap cooling performance. |
| 134 | “section 5.3gives” missing a space | Fixed |
| 134 | An energy level diagram would be apt to help follow the text. “Section 5.3 gives…” needs a space. | Fixed |
| 135 | “The tune-out frequency, where an atom does not interact with applied laser light…”. It is not true that at tune out frequency atoms do not interact with light, in fact, the imaginary part of the polarizability does not cancel at the tune out point. | Changed to 'The tune-out frequency, where an atom feels no force resulting from applied laser light, is an observable that tests QED independently of the conventional measurement of energy level differences.' |
| 136 | “An equivalent model can be constructed using an LC circuit - the object of ultimate interest is the equation of motion,…” sounds very strange. | Relegated LC comment to footnote |
| 151 | “Connection to the experiment” should be emboldened (maybe just quote the section number). | Fixed |
| 167 | \chi^2/dof ~ 1 needs a space. | Fixed |
| 169 | “in Tab 5.2” inconsistent with later usage down the page, “table 5.2” and also elsewhere where “Tab. XX” is used. | I think the XX here is a placeholder by the examiner. I have changed both instances to read 'Tab.' |
| 169 | “ 1140(20) MHz” should not italicized | Fixed |
| 171 | “also” twice.. | Fixed |
| 173 | figure shows power in mW, while the caption describes the powers in Watts. Would modify the figure axis to be consistent. | This seems unnecessary, the figure is perfectly legible. |
| 181 | “smallest precision” to “highest precision” | Fixed |
| 192 | “…alternative approach presented in section 6.2.1”. It is section 6.2.1 | Latex error. The source code refs to the right section. |
| 194 | “especially” spelt wrong. | Fixed |
| 197 | the significance of p-value should be stated. | The fit has $p=1\times10^{-3}$ which indicates it is extremely unlikely that the results could be obtained from a null model (i.e. one where the measured counts are independent of the independent variable $\N\_0 n\_0$ |
| 208 | Figure 6.8: From the text, it appears that these results are from the theory collaborators. They should be attributed in the figure caption. | Fixed |
| 224 | Ref. [18], “Bose-einstein”; Ref. [19] “structure of scince” | Fixed |
| 227 | Ref. [63], “evidence,” should be capitalised, “condesnation” | Fixed |
| 236 | Refs. [174, 175] duplicate. | Fixed |
| 237 | Refs. [187, 188] duplicate. | Fixed |
| 240 | Refs. [229, 230] duplicate. | Not duplicated, just very similar titles. Ref 229 is an experimental result, ref 230 is theory concerning the former. |
| 241 | Ref [237] “87RbD-line” missing space. | Fixed |
| 249 | Ref. [250], latex typo | Fixed |
| 250 | Ref. [351], typo “Optics COmmunications”; Ref. [353] formatting issues | Fixed |