## Abstract

## Acknowledgements

Well, this is about the time I start collecting some nice things to say to people who’ve stuck around. There will no doubt be more to add in future as I reflect on people’s contributions. I guess I don’t need to mention ALL THE FRIENDS. Likely they won’t read them. So maybe just include in the final draft submitted for feedback.

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* Josh - for reminding me that my various panics are not only normal but survivable

To my parents # Overview

## Thesis Outline

This thesis documents three projects that were undertaken in the ANU Helium BEC laboratory over the period of 2018-2019 and one project, which remains unfinished, that was undertaken in 2016-2018. A number of other graduate students were in residence at the time, and each of them contributed variously to the experiments. These experiments constitute a chapter each, and conclude with acknowlegements of the contributions of each person involved. The chapters are arranged in such a way as to provide a progression through subjects of increasing complexity, and are separated into three parts.

The first part includes a general introduction to ultracold atomic physics and an overview of the apparatus used to complete the experiments described in this thesis.

The second part includes chapters 3 and 4, and concentrates on a pair of experiments regarding the atomic structure of Helium. The experiments in this section are motivated by open questions in atomic structure theory, and includes a review of essential concepts. Chapter 3 concerns a set of measurements of electronic absorption lines, which in some sense is the most elementary concept encountered in this thesis. Chapter 4 describes the measurement of a tune-out frequency in Helium, at which there is a null response of the atomic dipole induced by an oscillating electric field. Such tune-out frequencies are determined by the interplay of an array of atomic transitions, and so this chapter represents a marginal increase in complexity. Conversely, the signals sought in chapters 3 and 4 are presented in sequence of decreasing signal power, ultimately converging on a measurement of a null response, attempting to determine when *nothing* happens.

Following these studies of the internal structure of atoms, the third part, consisting of chapters 5 and 6, is concerned with interacting systems. Chapter 5 describes a third completed project concerning the effects of weak interactions on the density and correlations of the momentum spectrum of ultracold Bose gases. This section includes an overview of the relevant physics of ultracold gases. Chapter 6 discusses the motivation for, and progress towards, an optical lattice trap for ultracold Helium. This was the initial project of my PhD, but after two years of work the decision was made to discontinue working to construct this apparatus. Chapter 7 presents a summary of the findings of all the projects in this thesis, presents future directions of research using ultracold helium, and concludes by unraveling the narrative thread of this thesis.

### Themes

Transitions - simplicity? Tuneout - measuring zero - the only constant is change, so can anything really have *no* effect? Depletion - stillness, and the impossibility thereof - vacuum fluctuations Lattice - complexity

## Introduction

Democritus

Ok so somewhere in here I wax lyrical. We tend to try to understand things. Why? Well. There is something advantageuos about being able to make predictions bout the world. This is something that has enhanced our survival prospects. But in humans something seems to be running on overdrive. And, sure, this isn't really relevant to the thesis, but you want to start from somewhere that naturally lreads to a framing of atomic theory.   
Matter is inescapable. Except perhaps in the dream state - we are surrounded by substance. Some two or so thousand years ago, Democritus posited that there was a smallest thing. That one could break mountains down into boulders, boulders down to stones, stones to sand, and sand... To something indivisible. He called them, literally, atomos, for indivisible. This was astoundingly prescient: The atomic theory, as it came to be known, would not find empirical validation for another millenia or so. And, like all theories that prove to be correct, it too reached its point of failure a few hundred years thereafter. The atomic theory was outlandish at the time, breaking with the notion that things were ultimately continuous. The exploration of the atomic world eventualyl yielded a new kind of understanding of everyday matter.

Kinetic theory of gases, thermodynamics, ‘absolute zero’

The first profound successes were had with gases, laying the foundations for thermodynamics and the understanding of the extraction of energy from storage in chemical bonds, via heating a working fluid, say - and powering the steam revolution. Among the findings of the kinetic theory of gases was what later became known as an equation of state - an apparently universal relationship between macroscopic quantities - pressure, temperature, volume, and mass - expressing the balance of energy in gases. Among the findings that stem from this understanding was the notion of an absolute temperature scale: That if one could extract enough energy from a gas, by cooling it, then its internal energy would vanish. It would have no volume. It would be motionless. This prediction predated Einstein's formulation of the mass-energy equivalence, but even at the time, it was appreciated that gas molecules had to have a size. So they could not vanish simply by getting colder. This paradox took some years to resolve, and it was only possible by completely overturning the picture of the atom. While understanding thus far had stemmed from the investigation of matter, the first quantum revolution was to come from the study of light.

Spectra & old quantum theory

Spectra are typically obtained by taking a beam of light - say, from a pinhole or slit in a mask - and passing it through a medium, such as glass, which disperses the light according to its colour. In modern parlance, there is a difference between the momentum (direction of travel) of light with different energies (related to frequency). It was by WHO? that spectra were first resolved with enough detail to distinguish more or less intense lines against an apparent continuum. And in particular, that pure elemental sources created different colours. A standard prism and screen would show different results with different elements. Wait - but by this point, we must have had an understanding of electrons, right? Because there was this Bohr model, where the electrons were orbiting the nucleus. And there was an understanding that moving charges radiated light - this was post-Maxwell, surely. Better go revise that history. So the upshot I guess would be that, anyway, there was a finding that the lines of light - oh also, the photoelectric effect which predicts that light are particles and have energy proportional to their wavelength, hey. So, this is how people worked out the energy gaps between the internal states of atoms that were later found to be related to inverse ratios of square integers: That led to the positing of a classical model, with a 1/r potential, that was inspired by the knowledge that potential energy fell off like 1/r^2. But did we get this backwards? Would have to revise the classical mechanics too, yikes. But yeah. The wise thing to do would be to talk angular momentum, and then from spectra we introduce Planck's idea of supposing that it \*comes in units\*. And then we add in the postulate of de broglie - where was this first verified? - and the double slit experiment, then the whole world turns just about on its head. And so was born the old quantum theory. Or something like that, go read Born and that Disney book for starters.

Modern QM, QFT? Presumably there will need to be some historical preamble, but the idea is now that we lay out the foundational pieces of quantum theory as we now understand it, or at least as it will be used in this thesis. This includes, and perhaps is motivated by an example, leading to a small exegesis of the thesis Hilbert spaces and quantum states Probability amplitudes Operators and observables Time evolution Composite systems Density matrices Interactions Correlations & Entanglement And a survey of the present state: Formulation of QED Lamb shift, Rydberg constant, etc etc? Extension to QFT Experimental successes Extension to condensed matter What’s after the standard model? Unifying HEP/condensed matter/QI? Following the math doesn’t seem to have worked for SUSY (and so one might take this as a cautionary tale for Everrett).

Quantum technology is a young term, but has been growing exponentially since used in print for the first time in about 1970 according to Google Ngram. Some have said that we live in the ‘silicon age’, in light of the pervasiveness of computing technology based on silicon substrates. What is perhaps less conventionally appreciated is that modern semiconductor technologies, including the transistor which is essential to the miniaturized computing devices available today, are a direct outcome of the first quantum revolution - that is, the conception of quantum mechanics and its associated ontological metamorphosis. Understanding gained since the conception of QM has led to myriad other technologies that foundationally depend on the quantum world. Nuclear magnetic resonance, for example, underpins the life-altering technology of magnetic resonance imaging and finds use in the study of biomolecular structure and industrial applications, forming multibillion dollar industries. A second prominent example is the laser, whose functioning depends on the quantum theory of light and matter. Of course, lasers will be extensively used through this thesis for the purposes of preparing and investigating ultracold samples of Helium, but outside of the atomic physics laboratory lasers are now used in fields as diverse as electromagnetic and gravitational astronomy, medicine, self-driving vehicles and robotics, cosmetics, manufacturing, remote sensing, and miltary use. Despite their foundational reliance on the quantum picture of the world, these technolgies may one day be seen as ‘primitive’ in the same way that a typewriter or vacuum tube is now. A second quantum revolution began with the creation and manipulation of single quantum states, for which Haroche and Wineland were awarede a Nobel, but includes technologies such as ion traps and coherent control mechanisms, and the still nascent technolgies of single-system state determination. These are still developing but have laid the foundations for the third quantum revolution, the large-scale engineering of quantum states by controllable interactions between multiple subsystems. The posterchild for such technologies is, of course, quantum computing. Notwithstanding ongoing controversy over the viability and usefulness of quantum computing, the challenge of large-scale quantum engineering has spurred an explosion of technical developments. Moreover, the growing prominence of quantum technology has drawn the curious eyes of computer scientists, who now join forces with physicists in attempts to unravel the basic structure of the cosmos from process-theoretic perspectives. Why is it, for example, so difficult to efficiently simulate quantum processes? Where is the border between efficient and intractable? The proof of genuine quantum advantages in certain processes may be one of the most profound statements about the nature of reality of this generation. Wherefore the nature of this advantage? Perhaps we will know before the century is out - perhaps, if ongoing crises arenot addressed - we will never know, and the cosmos may miss its chance to delve most deeply into its own self-awareness.

Digressions aside, a parallel stream of large-scale quantum engineering exists not in silico but in vacuo. The techniques of laser cooling to quantum degeneracy, established at the turn of the millenium, make quantum coherence (a topic to return to later) readily available and amenable to almost routine study. In the early days, cold gases were fantastic resources for studying atomic structure and basic interactions such as dimerization, because their low kinetic energies and densities dramaticall reduced the homogenous broadening effects that spectroscopy would otherwise be susceptible to. The development of advanced atomic clocks was a logical extension. In the later 90s though, Jaksch and Zoller proposed quantum simulation in optical lattices. Lattices themselves had been used before for some studies - check out Orzel for the work that *almost* got to the quantum phase transition - but yeah. That field is exploding now, with advances into microscopy and stuff.

Both of these topics - metrology and many body physics - form the spine of this dissertation. Also known as - internal structure and interactions - precision measurement and quantum engineering - stuff like this.

Ultracold atoms   
 A brief history  
 Cooling and trapping - who, when, why?  
 BEC - a complete surprise and experimental triumph  
  
 Ultracold metrology  
 Ultracold many-body physics

# BEC recipe

## TODO

Read up history of Bose - talk to Mukunda?

## Tabula rasa: Vaccum system

Why vacuum?  
  
 Physical models often make simplifying assumptions, like ignoring air resistance or friction, or collisions etc. Idealized models are only solvable in certain special cases, and introducing nonlinear effects like friction can make them mathematically intractable. This is partially resolved by sophisticated modelling these days, but the approximations will always remain. So when it came to studying microscopic systems, one would like to remove the background. so that the thing you are examining becomes not only the foreground, but the only thing in the image, and so your signal is not obfuscated. So people have tried to make vacuum for quite some time! Indeed this is one way atmospheric pressure was measured - Lavoisier used pumps from teh fire station to evacuate a chamber, and since then vacuum has advanced considerably. Vacuum manufacture is a massive industry now, and I wonder if I could find a history of vacuum pressures over time to track the best known vacua. In the case of ultracold atoms, one needs to maintain a vacuum because the forces that hold the atoms in their optical or magnetic traps are so frail that capturing particles from the atmosphere would be all but impossible - what fraction would have sufficiently low energy to capture, and how long would they last? So, yeah, yuo need vacuum, and the lighter your atoms, the more worried you should be about vacuum. It also provided benefits like making sure lines stay narrow to mitigate pressure broadening et cetera - trapping atoms in vacuum is in some sense the ideal tool for the physicist - to study things in almost perfect isolation, to extricate the subject of study with complete mastery over its environment. But nature abhors a vacuum, and so will find ways to ruin your day. So there are a ton of ways to get a chamber close to empty. Let's talk about a few of them.   
Chamber architectures  
Scales  
 Best vacua  
 Mean free path & collision time

## Cooling and trapping

Optical methods & limits  
 In this section I give a hueristic introduction to laser cooling. The detailed theory of atom-light interactions will be saved until the chapter on atomic transitions where the theory becomes necessary to understand the work. If the next couple of secgtions aren;t sat leatst.   
   
 Consider a two level atom. It is probably possible to get away without using the word hamiltonian, so I would give a basic picture. Atoms can be thought aof as having two states: One where the electron is further away from the nuclers? state (as the sxcited state0) and that the energy level flips up or down with the adition of a photo. Oh ok so I would have to say why it is quantized perhaps, so there would be something to draw on in the chatper perfio previously, where I talk history of QM, which would probably include something about Planck's u idea of quantized areas phase space. But then again - there could well be a QM primer in the Moern er modern QM chapter - perhaps with the flag that 'this is the only math in the introduction chapter' but then there are things like the doppler cooling et cetera. I think perhaps a differnt structure would suffice: a non-technical summary at the top of each section/cahpter we with a digression into deeper and depper detail as you get towards the next section. Then people can tap out and push on to the next section whe   
Magnetic traps & evaporative cooling  
  
The Doppler limit means that, for Helium, optical cooling to the ground state is not possible. This is when the velocity selectivity of the cooling light matches the velocity width of the sample. To reach the condensation threshold at the recoil limit, how big or dense would your trap have to be? Not reliable. This threshold was the limit for cold atom experiments until the development of evaporative cooling and polarization gradient cooling, which enabled the first production of bose-einstein condensates and resulted in the awarding of the 2001 Nobel prize in physics. Polarization gradient cooling is unavailable in Helium-4 because the spinless nucleus means there is no hyperfine structure, but fortunately evaporative cooling can achieve the low temperatures required for condensation in experimentally feasible traps. See any number of cold atom textbooks for an introduction or Ketterle's paper for a great deal of detail. Here we provide a summary explanation only. Additional details are discussed in chapter 3, where the evaporative cooling process is part of the relevant experimental method.   
  
Absolute limits of cooling  
 Thermodynamic limits  
 Third law & quantum proof  
 Trap losses  
Modern methods  
 Cooling fermions  
 Prospects for feedback cooling?  
 Quantized refrigerators  
 Algorithmic cooling  
 Other techniques: dilution fridges etc  
  
Degenerate matter  
 In this secion I will try to gvie a description of ultracold degnerate matter that even py a parents can understand! What a challenge. So I think it will be done in a cuple of parts. oNe where one considered a parabolic bucket with atoms rolling aroudn in it - at high energy they don't see each other. As they lose energy they get closer and clsoer to each other eeventually such that their interactions cant be ignored - they keep pinging off each other. They won't let the other balls take up ther space they are occupying. But there are two kinds of matter hey, there are bosons and fermoins. So at this pojt one would be able to intotduce the communtation relations in the technical part but fur the moment just accept: There is a wave-particle duality, you can see it with photon.s That would be a find introduction - well I guess it lives up in the itnro about when QM was born and all that. The double slit experiment would be a great example 0 maybe actualyl just the single slit, you know, that illustrates the diea of diffraction (does it?) and particles - depends what one is trying to show. I think yes the doule slit ecnompasses most of quantum (except entanglement lel) but hey. So yeah, I guess in this part would be something like a toy model, and then one can introduce the critical points like:  
 That there is a ground state of traps, that the numbe rof particles in this state can be more than one when yuo are working with bosons, but not fermions. ANd they ahve to be cold - heuristically the picture could be that (because trying to avoid this idea of microstates) actually don't avoid them, but I really wonder how simple ane xplanation one can make here. But then again what's the idea behind going for simple? The're is the idea that someone will bea ble to read the thsis as a motivatedd undergrad, or as my parents so they sort of would be able to see what happened, or to new students in the lab, who could be able to read quickly but hey if you get to the point of joining th lab you must pbe pretty math-savvy. So. Yeah. What do we talk about?   
 History of degenerate matter: Well that was sorta covered in the super intro.   
  
 Ok. Revising the content of this section. It will be the brief introduction to BEC. Consider a gas of non-interacting bosons in a harmonic oscillator potential. This can be treated in the grand canonical ensemble - plz clarify what the reservoir is - and so do you need to talk density matrix? I mean the Penrose-Onsager criterion is one way to do it. Seems relativistically fine - all good for the ground state biz but extends to nonzero momenta (moving frames). Blah. Bose-Einstein statistics. Bose enhancement re: scattering into the ground state. Point to references. Blah. No more words arriving. Let's fuck it I will see if I can get some formatting done here...  
  
BEC three ways  
  
The title is an illusion to Prithvi and my sketchy plan for pear, three ways. First, the heuristic.  
  
While the industry of European statistical physics was in its infancy, a young admirer of Einstein began asking questions would plant the seed of a flurry of work culminating in a technical triumph over the next seven decades. Satyendra Nath Bose made postulates about distinguishability - and noticed that such particles had dramatically fewer distinguishable microstates than they would if they were distinguishable. This, of course, has dramatic consequences for the statistical physics of systems constituted of these particles, which are now known as Bosons in his honour. Among the predictions that follow from postulating the indistinguishability of particles is that in the low-temperature limit, the velocity distribution diverges from the more familiar maxwell-boltzmann distribution. The threshold here can be thought of as the point where the intrinsic wavelike nature of particles becomes pronounced enough that the wavepackets of neighbouring particles begin to overlap, heralding a regime where the particle picture breaks down. Explicitly, Louis de Broglie's postulated that the relationship of momentum and wavelength of photons, \(\lambda\_{dB} = h/p\), was exactly true for all particles. The reason we do not see particles interfere at everyday scales is that the so-called de Broglie wavelegth is smaller than the particles themselves. Taken in concert with the equipartition theorem, one can assign the (mean?) de Broglie wavelength of particles in a gas of temperature T,   
  
\[\lambda\_{T} = \frac{\hbar}{\sqrt{2\pi m k\_B T}}\],  
  
and in three dimensions one can therefore ascribe a the particles a'quantum volume' of \(\lambda\_T^3\). For a gas of density $n$, the volume per particle is $1/n$. From this argument, the quantum nature of gas particles cannot be ignored when \(\lambda\_T^3 \approx 1/n\), or when \(n\lambda\_T^3\approx 1\). The latter quantity is called the phase space density, referring to the concentration of the likely atomic states into a small region of phase space, consisting of the position-momentum conjugate variables (footnote: Phase space is not just x/p but could refer to any set of conjugate variables, mathematically speaking). Below this point, the distinguishability of particles becomes crucially important.   
  
In the case of indistinguishable particles, at a given temperature the probability that a single particle will occupy a given state of energy E is given by the Bose-Einstein distribution. Another way to read this is the number of particles that occupy a given state, on average, is given by the BE statistics. Remarkably, as the temperature vanishes, the population of particles falls overwhelmingly into the lowest-energy state. The temperature at which a macroscopic fraction of the atoms occupy the ground state simultaneously is called the critical temperature, and coincides with the temperature given above. This heralds the phase transition from a 'normal' gas to quantum degenerate matter, or Bose-Einstein condensation.  
  
More formally, the critical temperature depends on the spectrum of the Hamiltonian, which must have a local minimum to allow for bound states. In this thesis, all experiments are performed in a magnetic trap which is described by a harmonic oscillator potential   
  
\[V = \sum\_i\frac{m\omega\_{x\_i} x\_{i}^2}{2}\]  
  
for which the critical temperature for condensation is   
\[k\_B T\_c = \hbar\omega\_{ho} = \left(\N/\zeta(3)\right)^{1/3} = 0.94 \hbar\omega\_{ho}N^{1/3}\],  
where \(\omega\_{ho} = (\omega\_x\omega\_y\omega\_z)^{1/3}\) is the geometric oscillator frequency and \(\zeta\) is the Riemann zeta function. The fraction of particles in the ground state, or the condnesate fraction, is given by   
  
\[\frac{N\_0}{N} = 1-\left(\frac{T}{T\_c}\right)^3\]   
  
when the gas is below the critical temperature. For the traps used in experiments in this thesis, the critical temperature is VERY LOW and the temperatures we reach are generally EVEN LOWER, producing condensate fractions about 95 per cent.   
  
Condensation is a bona fide phase transition. The associated order parameter is sometimes known as the \*mean field\* and is defined as a complex parameter \(\sqrt{n\_0(r)}e^{i\theta(r)}\), where $n$ and $\theta$ are the (potentially inhomogenous) ground state density and local phase of the macroscopic wavefunction. The imaginary part of the order parameter is nonzero probably because of stimulated scattering or something, so you wind up with constructively interfering particles. I guess. Either way, in this sense the BEC displays coherence at macroscopic scales - admittedly most condensates are only a few tens of microns across, but during our experiments they expand in freefall to an ellipsoidal volume of XXX - I wonder what the largest coherent volume is otherwise? How big is the biggest superconductor? Hm, I guess the magnets at CERN have us beat.   
  
Macroscopic coherence is also manifest as off-diagonal long range order in the density matrix, as described by Penrose and Onsager (and leading to their definition of condensation in terms of the eigenvalues of the density matrix), and also has close analogies with Glauber's theory of optical coherence. Glauber's theory was extended by Sudarshan (?) to matter waves, which are distinct from the photonic case by ???. The theory of coherence makes predictions about the arrival-time correlations and distinguishes the g(2) function of the condensate (FUNCTION) from the thermal state (FUNCTION). These predictions were borne out by early experiments with metastable Helium, conducted in this laboratory using the same machine described in this thesis. For these reasons, the BEC is often referred to as a coherent state of matter, and the resulting pulses of atomic matter waves are called atom lasers in analogy with the coherent light sources, or lasers.   
  
This, along with the various analogies between the optical propagator (the Huygens' equation) and the quantum mechanical one (the Schrodinger equation?), especially in the advent of techniques for reflection and dispersion of the momentum of coherent matter waves, led to the emergence of the term \*atom optics\*, and heralded a slew of experiments with matter waves that demonstrated the equivalence of optical and atomic systems, including matter wave interferometers and foundational experiments like Wheeler's delayed choice experiment. A distinguishing feature of atoms from light is that the atoms have intrinsic rest mass, and hence interact with each other gravitationally. This is the root of ongoing experimental campaigns to harness this distinguishing feature for applications such as gravimetry, and also to probe the interface of quantum mechanics and gravity, a central outstanding problem in modern physics.

## Diagnostics & detection

Fluorescence  
 Theory, some example plots  
 Why the extension of lifetime? Off-resonant scattering rate lowered eh, plot curve vs wavelength?  
 Calculation of population from signal  
 Precision  
Absorption imaging  
 Theory  
 Sure, it's a momentum measurement, but let's calculate its limits  
 Limited resolution and sensitivity - make some reasonable assumptions  
 There is also fluorescence imaging in gas microscopes but we don't have the optics for this  
Delay-line detector  
 System diagram  
 Resolution in TXY  
 Field of view in K-space  
 Optics analogy  
 Dark counts  
 Saturation  
Atom lasers: CW, Pulsed & trap freq  
 CW   
 Sweep model  
 temperature fitting?  
 Correlations?  
 Pulsed  
 Outcoupling spectra  
 Fourier broadening  
 BCR  
 'Weak' number measurements & single-shot precision  
 Trap frequency measurements  
 Compressed sensing  
 Aliasing  
 Optimal sampling methods?

## Laser systems

ECDL  
 System diagram  
 How dither locking works  
 Experiment insertion  
 Lattice build described in lattice chapter, stick to downstairs architecture to begin with  
TiS  
 Seed & pump for 532nm  
 TiSaf  
 Cavity, crystal, etalon   
 Locking system  
 Doubler  
 Mechanism  
 Locking  
Calibration  
 Software lock loop  
 MATLAB architecture - appendix?  
 Wavemeter   
 How it works  
 Cs crossover transition  
 PMT setup -   
 dither lock  
 Stability of lock  
 Bounds on accuracy of calibration  
Monitoring  
 AI importing, intensity & SFP checks, WM lock check...  
 Camera/mirror setup   
 Reproducibility issues

# Metrology

Metrology may be reasonably defined as the art of measurement.

## Section introduction

Quantum Electrodynamics, or QED, describes the interaction of charged particles with the electromagnetic field, whose fundamental excitations are identified with the more familiar photons, or particles of light. QED therefore describes the physics that governs all we see with our eyes, the interatomic forces from which arise the various familiar phases of matter, prevent solids from passing through one another, almost all technology (even nuclear physicists use electronic control and diagonistic technology), and indeed the dynamics of the action potentials in neurons. Hence, the purview of QED may well include the physics underlying the most intriguing of phenomena, perception. The detailed connection between quantum field theory and subjective self-awareness are beyond the scope of this thesis. For decades, quantum electrodynamics has stood unchallenged as the most accurate quantitative description of the world to date. Among its triumps include the prediction of the Rydberg constant to absurd precision and the correct prediction of the existence of antimatter. As the first synthesis of special relativity and quantum mechanics, QED laid foundations for more general quantum field theories, ultimately leading us to the standard model of particle physics. Undoubtedly, QED is a foundation stone in one of the great pillars of our understanding of the cosmos. However, as any sensible applied scientist will tell you: All models are wrong. QED, and QFT in general, presently has no formulation that is consistent with general relativity (other than in string theory, which despite its ambition and elegance has yet to satisfy experimental physicists). However, until we have the technology to synthesize black holes or other extreme gravitational conditions, we may not have experimental access to the high energy densities required to probe the Planck scale where quantum and gravitational effects are expected to be of comparable magnitude. Fortunately, we may not have to wait so long: The infamous proton radius puzzle, regarding the disagreement between experimental determinations of the proton charge radius, remains unresolved. Further, there remain statistically significant disagreements between predicted and measured energy levels in Helium. If there is an identifiable bias in theoretical predictions then, optimistically, one may find a legitimate need for physics beyond the standard model to explain these results. Therefore, experimental atomic physicists may find themselves prospectors for the fundamental discovery of the century. The experiments described in the following two chapters constitute searches for evidence to constrain the search space of theories that purport to resolve the ongoing disagreements. Before describing the aims, findings, and methods of the experiments, I will provide a short refresher on atomic theory, terminology, and notation that is relevant to the following results.

## Atomic theory

Refresher - Levels & spherical harmonics  
  
Transitions: Dipole approx  
forbidden transitions  
Oscillator strengths & driving

Helium Helium exhibits a uniquely large gap between the ground and excited states (is it the biggest? What other electronic transitions have higher energy. The lowest-lying excited state differs in electron spin from the ground state, and is also an S state, hence the transition is *doubly* forbidden. This confers a lifetime of 7900 seconds, as measured by Hodgman et al in the lab in which I started my thesis. The gap of 19.8eV is a few times the work function of most metals, which is the key enabler of the use of the delay-line detector in our experiment. However, interatomic collisions may also distribute this energy between atoms, ionizing them. What other decay channels can you have? Can both atoms remain neutral and yet disperse the 20eV in kinetic energy? This is known as Penning ionization (box: Penning and his ionization) and can lead to atom loss or sample heating. Fortunately it can be suppressed if the sample is spin-polarized, because then the scattering process no longer conserves angular momentum. The suppression, oddly, fails at very high magnetic fields - maybe because of deformation of atomic orbitals. As Penning ionization is collision-driven, the ion production scales with the square of the atomic density. This means that monitoring the ion production rate can be used as a measure for the atomic density, and indeed has been used to detect the condensation of Helium previously by this group. Also, I think people tried to use it as a feedback signal, but I dunno how that went.

The metastable gap is the agony and the ecstasy of Helium. Other noble gases also exhibit large gaps and long metastable lifetimes, but the lifetime decreases with increasing mass, and the penning ionization rate is harder to suppress. As they have larger masses, also, the condensation temperature is lower. Given the technical difficulty of condensing Helium, it may remain the only noble gas ever condensed for the foreseeable future.   
LEVEL DIAGRAM GOES HERE

## History of spectroscopy

Early uses of spectra  
Discovery of Helium?

Types of spectroscopy Emission & absorption spectroscopy State of the art methods Landmark results Lamb shift proton radius

# Transition measurements

## TODO

Read:   
 Vassen 1557nm  
 Martin 1960  
 Luo et al & refs  
 Drake etc theory papers for outstanding differences  
 Ketterle: Evaporative cooling of neutral atoms (for rough model)  
 Motivation & methods/state of the art for transition rate measurements  
 Kieran's Forbidden code  
 WTH actually drives forbidden transitions?  
Calculate  
 Stark shift  
 Three-level model  
 Evap toy model  
 Anticrossings  
 Number unc  
 Forbidden SNR redux  
 Precision needed for isotope shift measurements?  
Figures  
 Sequence diagram

This chapter describes two sets of measurements of electronic transition energies in Helium. First, I recount the method and used to measure the energy splittings between the 23P2 state and a collection of states in the n=5 manifold. Second, I describe two methods used to measure the splitting and the transition lifetime of the forbidden 23S1-33S1 transition.

## Gap, aim, and scope

Some of the 2P-5L transitions were observed by Martin in 1960, but his measurements are in stark disagreement with predictions made by Drake in 20xx. Further, in the NIST database, the transition energies to the 53D states are all identical. Indeed, in Martin’s original paper, he only quotes measurements from 23P2-5D, indicating that his equipment did not have the resolving power to distinguish the lines from the 23P to the 53D states, and so obtained a weighted average of the energy splittings that I resolved individually for the first time. Martin was also unable to distinguish the 51D2 transition line, perhaps because of its proximity to the 53D lines. Even so, the transition rate is five orders of magnitude less than the n=5, L=3 transitions, so even if he had the resolving power he probably wouldn’t have been able to detect the line? I wonder what the weakest line he found is. Given the unerring accuracy of QED in most other arenas, it is prima facie not obvious that this disagreement points to a calculation error or a domain where QED is not valid. That said, the method for measuring excited-state transitions extends to the first observation of this singlet-triplet line, which is of interest given the outstanding disagreement on the singlet-triplet interval as constrained by the precision measurement by Vassen’s group.

The 1557nm transition was of particular interest because of its incredibly low transition rate. Predictions of these require corrections from QED. Later in this chapter I will discuss the first measurement of the 427nm transition, whose transition rate is XXX times slower than Vassen’s, and a second method to measure the same transition from which we calculate the Einstein A coefficient of this transition. To our knowledge is the weakest transition observed in a neutral atom to date. Include Lach and Pachucki comment.

## Contribution

The table below displays the results of the measurements, including predicted linewidths. In the case of the 23S1-33S1, the measured Einstein A coefficient is also listed. These measurements are accurate to a few hundred parts per billion, and their accuracy is limited by the absolute accuracy of the wavemeter we used as a reference for the laser lock. Within the accuracy stated by the manufacturer, these results are consistent with the predictions of QED. Of the six lines measured, three have been resolved individually for the first time, and two have not been recorded elsewhere. The centre frequencies are obtained by fitting Lorentzian profiles to the atom loss spectra. Have a look at residuals; what are the expected broadening effects? What are the expected systematic errors? Error budget goes here also.

## Method: 5L

To drive the transitions from the 23P2 state to the states in the n=5 manifold, I use the probe beam to disturb the near-resonant optical molasses cooling stage of the experiment. This follows the MOT loading and precedes evaporative cooling, and operates with XYZ beam parameters for XXX ms, and then with ABC beam parameters for YYY ms. I calculate that during these stages, the excited-state population is ZZZ per cent, which are then susceptible to scattering photons from the probe beam.   
  
The beam was initially aligned by tuning the probe beam to the predicted value of the 53S1 transition and operating with the maximum available power. Although the uncertainy in wavemeter accuracy was larger than the transition linewidth, by operating well above the saturation intensity, the transition was broaded by tens of MHz and so the WM error was less significant. When scanning the beam pointing across the expected target region, approximate alignment was signaled by a dramatic loss of atom number. When the signal saturated, the power was lowered until the signal was just above the noise floor, and then scanning resumed. This process iterated a few times until the maximum attainable signal was below saturation - that is, when for fixed power one could not completely destroy the trap. Notice there are three kinds of saturation here: Atomic population saturation, detector saturation, and signal saturation when you run out of atoms. perhaps the term 'dynamic range' would be more suited to the latter... Something to think about.   
  
I used the evaporative cooling sequence as a transducer between scattering-induced heating of the cloud and the final condensed number. I will present a quantitative sketch of the mechanism below, but one can also take a heuristic understanding from the following argument:  
  
The evaporative cooling we use to achieve Bose-Einstein condensation has stringent tolerances on initial phase space density, which increases with number and at lower temperatures. Tuning a radio chirp to the spin-flip transition from a trapped to an untrapped state and sweeping down to lower energies, higher-energy atoms are removed, the cloud rethermalizes at a lower temperature, and the phase space density increases. Higher-energy atoms spend time further from the centre of a harmonic magnetic trap. So, scattering photons from the probe beam heat the cloud, leading widening the velocity distribution, which drives more of the atoms into resonance with the radio chirp. The final temperature is determined by the endpoint of the radio chirp. Resonance with the probe light manifests as a signal in a reduction of the final population of the condensate.   
  
The method therefore consists of alternating measurement trials with control trials, wherein the laser beam is blocked before the fibre coupler with a flipper mirror. At the moment I use an interpolation, but I might want to try using a model-based estimate of the atom number. Either way, there is going to be some error in the number estimation. It's probably small. The difference between the interpolated, unperturbed atom number and the detected number in the measurement trials is affected by the quantum efficiency and the introduced uncertainty in atom number.  
  
The polarization of the light was fixed with the waveplates before the chamber. Can you tell handedness just by relative angle of waveplates? We hypothesized that the initial state of the atoms during the cooling phase was in the m=2 state, as the optics are configured to drive with a sigma+ beam during the in-trap cooling stage. I verified this by driving with plane-polarized light (in the atom frame - put some trap sim in to show where the field points), which is a linear combination of sigma+ and sigma- light. If there were atoms in initial states other than the m=2, then when driving to the 53S1 state, one would observe multiple peaks. Instead, only one peak was observed, which vanished when the probe beam was set to sigma+ light. (I think check the data).  
  
The measurements are taken at two different background field strengths. Therefore the detuning from cooling resonance is X and Y MHz in each stage, respectively. These values were calibrated independently by an RF spectroscopy technique. This allows empirical extrapolation to the field-free transition energy by correcting for the calculated Zeeman shift of the centre frequency of each measured line.   
  
QUANTITATIVE MODELS  
 Evaporative cooling  
 Scattering rate and heating - can you determine the oscillator strength at all?  
  
Analysis

## Method: Forbidden 1

After our measurement of the 2P-5L transitions, our eyes turned to the 427nm transition. To our knowledge, nobody had measured it. We found that we required ten orders of magnitude greater sensitivity in order to detect this transition - a few mW over a few ms wasn't going to cut it. After discussion we decided that the most promising method might be to look for heating or loss by directly illuminating a BEC - having found previously that weak trap lifetimes can in fact be several minutes. However, before we embarked on the measurement, I performed some simple calculations to estimate the order of magnitude of the best signal-to-noise ratio we could expect.  
  
We determined that this SNR would be sufficient to warrant making an attempt at the measurement.   
  
RECOUNT CALCULATION   
 3 level measurement  
 Clebsch-gordan coefficients and net transition rates  
 Collection efficiency monte carlo  
  
Sequence diagram  
  
For this measurement, the data processing methods were the similar to the 5L transitions - a drift model was created to predict the undisturbed atom number in a given shot, based on atom number measurements from the calibration shots. Wait - did the outcoupled fraction get used, counting the number dropped versus the number left in the trap? Were these things tried? Talk to Kieran.  
  
This method allows for the extraction of the line centre and width, which determines the state lifetime. However, the state lifetime is dominated by a fast decay to the 3P state, the oscillator strength of which is many orders of magnitude larger. The oscillator strength - which is proportional to the Einstein A coefficient - of the transition can be obtained by another method, described in the next section.

## Method: Forbidden 2

We develop a second method to determine the Einstein A coefficient of the specific forbidden transition. We perform time-dependent thermometry of the a thermal cloud (above the critical temperature) while alternating shots with and without the probe beam blocked. During these sequences, we use RF pulses as per standard procedure (although in this case the term 'laser' is especially misleading as the source is incoherent) and fit a Gaussian profile to each pulse. During the 25 second hold time, the cloud heats at a rate of X K/sec. This is possibly due to: Penning ionization, magnetic field noise, background collisions, majorana leaks? How does it depend on number? Anyway. With the probe beam applied, the calculated scattering rate of up to Y Hz corresponds to a peak additional heating rate of Y J/sec. We fit the time evolution of the temperature of the thermal cloud with a linear model and obtain the change in heating rate with respect to the probe-free shots. We can then back-calculate via the specific heat of a harmonically trapped Bose gas to determine the energy transfer rate (which should just be proportional I think, when above the critical temperature?), hence the photon scattering rate. And so lo and behold we can determine the A coefficient, and look, it's really weak! What a great job we did. I wonder whether Kieran's method is a bit sketch because does heassume a certain density distribution??

## Systematic effects

Power & curvature measurements - what actually drives Quadrupole transitions?

## Next?

Isotope shifts & better reference  
Different target transitions?

Link to next chapter # Tuneout  
## Poetic license. Silence - nothingness - stillness. A subject of fascination for humans over centuries. Entire schools of meditation practise, and pursuits of divinity, seem to be pointed towards realizing perfect stillness. The space between the breaths. The stillness of the mind and the insights that follow - from simple clarity. The universe, however, is never motionless. Whatever zero-point energy happens to be - suggests that true stationarity is impossible. And likewise, in depletion, nothing can be truly empty, thanks to vacuum fluctuations. But there is the resounding theme throug thsi thesis, right of approaching the motionless, of approaching the definitive, the ability to say precisely waht something is. To distill an element of truth, and the effort it takes to wrest away so much of the noise of the world is titanic. The continual drive to split the subject from the object - by ensconcing it in vacuum, by isolating something with laser precision, a *pure* sample in *stable* conditions - in some sense perfectly well-defined, perfectly characterized, and perfectly frozen. But nothing in reality is so simple - even at the ground state, there is endless detail - field noise, et cetera.

The condensate aquiver in the ground state - depletion. As ketterle said ‘exquisitely isolated’ The interplay of internal structure aligning perfectly to isolate the atom from the world. THere’s poetic beauty in this. There’s an allure. But elusive, and so is the condensate - the QM description works but there are always going to be traces of the initial conditions, right?

So, tuneout.

Electronic transitions can be violent events! THe electron cloud completely reconfigures. A disturbance in the electric field, the energy ripping out from the cloud (go read the attoclock/electron tunnelling papers maybe) - we continue the journey down to weaker signals in pursuit of the measurement of nothing. That nothing is almost always impossible to measure - detector noise, for instance, always present, and the ever-pervasive background. Best we can do is to account for all these - but zero crossings are easier to meaure.

Blah. Even in TO we don’t realize a state of perfect stillness - it’s done by trying to shift a frequency. But to make it indistinguishable. We tried the trap driving method but that didn’t really work.

## Topic intro

Intuition: Classical oscillator  
Polarization & Polarizability   
Sketch of method  
 Trap freq theory  
 Nonlinearity & anharmonicity - why did our traps chirp? Model never quite nailed the same chirp did it? Was it trap ringing in the end?  
Uses of Tuneouts  
Previous measurement

## Gap, Aim & scope

Have TO been used for QED?  
Measure the TO with sufficient precision to test QED

## Contribution

We did it.  
Error budget.

## Method

Trap configuration

Alignment We employed three stages of successively increasing precision to align the probe beam with the magnetic trap, using a 2W? 532nm laser, followed by a 300mW 450nm beam, and finally using the tunable laser at approx 405nm?. The first beam was used for coarse alignment by scanning the vertical position of the focusing lens and dropping the BEC onto the phosphor detector. This technique has been used previously to align beams - reason being that the repulsive dipole potential of the 532nm beam creates a fissure in the BEC as the fallinc condensate diffracts around the beam. The effect is weak but visible as a dark stripe through the BEC - at least, ideally. Sadly, our ingenuity held us back (yet again). We used a pickoff plate - a large spherical optic which is weakly reflective at the target wavelength - for initial scans, deflecting a fraction of the beam onto a CCD (as described in the Laser System chapter). Unfortunately, Bryce dropped this optic at point point and the rim of the glass lost a chip. We did not notice for quite some time that this had altered the strain distribution on the transmitting surface of the optic, and actually completely destroyed the beam profile. How did we find this out, again? This led us to replace the optic with a mirror on a hinged mount, so we could remove the mirror and return it to a controllable position. Once we removed the damaged optic, we were quite quickly able to find a signal in the disturbed BEC. I think we eventually used the atom laser for a better visible signal - although the phosphor had a better dynamic range, the brightness difference was hard to see by eye, but integrating over several PALs gave us a density profile we could use. We aligned the beam with the fall path of the condensate by ensuring the destruction was in the centre of the falling condensate, and then raising the beam step by step until the signal vanished. At this point we figured we’d overshot the trap, so stepped back down and changed to another beam after marking beam position on the CCD. We then changed to the high power 450nm beam because it would produce a strong polarization response in the condensed atoms. We then ran successive trap frequency measurements while adjusting the beam position, looking for disturbances in the oscillation frequency under the same mechanism by which our measurement method works. When this signal reached a maximum with respect to position, we iterated adjustments in lens position along the beam axis with adjustments in pointing (as imperfectly aligned optics would couple these degrees of freedom). When this signal was maximized, we switched to the probe beam at 405nm. At this wavelength the atomic polarizability is positive so the beam is attractive. We therefore adjusted the sequence by switching off the beam at XXX ms after the trap release. When the beam was aligned we observed a second peak in the detection rate (picture), from the release of atoms trapped in the beam. We iterated this alignment procedure until the number of trapped atoms saturated - assuming this to be pointing at the BEC. Then we switched to alternating shots measuring the trap frequency, as in our measurement method, and adjusted the lens configuration until the frequency difference between the measurement and reference shots reached a maximum. Then, because the optical dispersion would be such that the beam pointing and focus would vary with wavelength, we repeated this procedure after taking steps of a few nm at a time towards the tuneout wavelength, eventually settling within a few MHz of the transition on the assumption that a few ppb change in frequency wouldn’t bother us. We measured the distance from the focus lens to the chamber centre with reference to a technical diagram, and positioned the CCD at this distance away from the focus lens (including the reflection off the alignment mirror) but of course the beam before the focus lens would not have been perfectly collimated which might have affected the outcome.

Tune-out for fixed polarization

As described in the section above, for fixed laser intensity, the polarizability of the atom in the neighbourhood of the tuneout is proportional to the detuning from the tuneout. We used the machine control interface to automatically iterate the laser setpoint in steps of XX MHz for total scan sizes of YY MHz about the Tuneout. Alternating shots between measurement and control, where the laser was blocked on the pre-fibre side (even though the AO could be set to zero, light leakage had been observed. Why? Probably the zero offset of the photodiode. And also leaking fundamental light? I mean, we characterized that anyhow, will have to dig up that measurement). We use the calibration shots to produce a drift model to predict the underlying magnetic trap frequency (details?), then take the difference of squares of the measured frequency and the predicted frequency. Below is a plot of the squared difference versus wavelength for a single scan (PIC). The zero crossing is determined by fitting a linear model and solving for y=0. There was not a statistically significant quadratic contribution to the signal. The stat error in the zero determination is X. Sys contributions at this level are Y.  
  
The gradient of the line was found to vary, and in some cases invert in sign. While initially puzzling, this turned out to be a useful validation of the trap frequency picture came from the inadvertent observation of a change of sign of the trap frequency change. This was eventually ascribed to the sign change in the second derivative of a Gaussian function, which shows that the small-amplitude oscillation picture described above is actually quite accurate despite all the approximations (like, how big is the BEC?). (PIC)

Polarization dependence

We measured the dependence on polarization by adjusting the waveplate optics. We took wide scans (several GHz) about the TO for a few WP values, and produced an empirical model to predict the frequency of the tuneout as a function of waveplate angles. This helped us search for appropriate scan regions as we iterated. We did not rely purely on predictions based on waveplate configs as the birefringence would have been wavelength dependent (and we were about a nm from the spec wavelength - estimated error here? These are zero-order, so could perhaps estimate this, assuming no change in refractive index, just from the physical size diff maybe...). Not to mention there was the unfortunate fact that we put the focus lenses and mirrors after the optics - will be a tricky thing to put into the thesis, how we screwed this up and spent so long trying to correct it...   
  
As we have numerous scans across the tuneout in any given run, we have some options for processing. One would be to bin the shots by set wavelength, and fit the average values. However, this would lead to issues with trap frequency or alignment drift? But the solution we took was to determine the zero crossing each scan separately, then take an average over a given run weighted inversely by the statistical error in that shot. This is because deviations from perfet alignment would manifest as a lower gradient and hence reduced sensitivity - so did the gradient or the stat error get used as the weighting function? Should go check. This introduced a stat/sys error in the fixed-polarization tuneout of XXX MHz.  
  
We did not, in the end, know what the final state of the polarization was. We used a beamsplitter to measure the min/max transmitted power as a function of QWP angles (go revise the method) to determine the degree of linear polarization, leaving the sign of the circular component undetermined (can possibly calculate from WPs but wound up using the empirical model anyhow). We also passed the beam thruogh a few optics and a vacuum window in order to actually hit the atoms, and the effect of the optical birefringence, angle-dependent polarization shifts from reflective mirrors, et cetera, would mess with this. To estimate the polarization we set up a Rochon prism after the beam exited the vacuum chamber through the LVIS hole and calculated the circularity of the light. We compared with measurements taken at the centre of the beam axis before the window and found they were - how different? - this introduces an error margin of the light polarization of, well, some amount, and this manifests as a variable systematic error in some silly way.  
  
Unfortunately we did not have the ability to measure the pointing of the magnetic field in the trap. This meant we were unable to find the precise angle between the probe beam wave vector and the quantization axis - hence the precise polarization in the atomic frame was not possible to determine as a function of measured parameters before the experiment. We constrained the direction of pointing to within 4 degrees (in polar and azimuthal angles)? Or we estimate that it was BLAH degrees based on simulations of the magnetic trap (estimate systematic error here). Fortunately, somehow this is absorbed into the fit so it doesn't matter in the end. Will need to revise this procedure.

## Systematic errors

What goes in here?

## Issues

Hyperpolz?

##Next? Isotope shifts? Precision required?

## Chapter wrap

Key findings  
Section wrap  
Link to next part

# Many-body physics

Section introduction   
 Statistical physics   
 Phase transitions   
 Thermal, quantum, dynamical, computable, semantic  
 Emergent complexity

The third section of this thesis transitions from the study of atomic structure to the emergent dynamics of interacting systems. In teh old essay ‘More is different’, there is an argument that when you put a lot of thigns together they start acting in genuinely novel ways. A single water molecule is not wet - nor does it mean anything to say it is any given phase, or that is has a temperature. (this is of course problematic because it ascribes a specific state but the idea’s there I guess). So ya. One of the watershed(?) moments in the history of physics was the *derivation* of thermodynamics from statistical mechancics - the assumption of a set of postulates about the microscopic nature of the world that led through the law of large numbers to a new understanding of empirical laws of the past; this gave a framework to understand and extend thermo, and it was a triumph to provide systematic ways to determine macro physics from teh nature of interactions. What was more profouond was the discovery of universality classes - that at phase boundaries there were unifying properties across disparate systems, things that tied together quite general phenomena in terms of these scaling relationships. This is kind of garbled. But yeah, look, we have agaaghagha this is just insane rambling - I wonder if I can push to 10k words in the process of smooshing out all this. Not an entirely honest drive if I’m honest but w/e the thing is I’m here and I’m writing even if it’s completely useless and will get trashed. This is a start, even if it’s bogus and hard. Anyhow back to progress. So ya - statistical mechnics also provided an actual understanding of temperature as a sublime phenomenon in all the emergent phenomenon - that really, at the end of the day, thermal equlibrium and the ‘spontaneous’ processes we see in nature are just outcomes of, basically, the law of large numbers. Phase transitions have been noted in all kinds of places now; the BEC transitiion is a classical phase transition. The idea is that an *order parameter* changes value from zero - representing a kind of disorganization - to something nonzero, representing an emergent order. These come in different flavours and in different systems but tied together by their scaling laws - universality classes. Something really beautifully profound there. And of course, pepole have tried to take the numinous idea of the phase transition, of the *qualitative* change in the emergent properties of otherwise unchanged constituents, just by fiddling how they relate to each other - and staple it to all kinds of things. People talk of phase transitions in general network theory, presumably in social dynamics, and more recently even in the study of grammars, arguing that meaningful language corresponds to a marked phase transition in lexical trees. But turning back from flights of fancy - thermodynamics unified these ideas (in the earlier days) with the idea of *Free energy* which has since run rampant. But the idea is that - well, if you have tunable parameters, then are there different solutions to some equation? How do you wind up with these multiple free-energy landscapes from a microscopic basis? Or do you need to staple models toether? Idk, this would have been a great topics for a PhD, hey?

Regardless. The idea is that we should probably add more signposts to this meandering mess. I want to lead from classical phase transitions to order parameters - using BEC as an example, identify its universality class, no need to list off a ton or ramble about them too much. Then say hey, once you’re in the ground state, you have different kinds of phase transitions. Quantum phase transitions, which aren’t driven thermally or by statistical laws but by the structure of the gruond state, and they were first observed in an optical lattice. Free energy comes up as should correlations, I think, as they both come hand in hand with order etc.

# Quantum depletion

## Section intro

## BEC detail

Bose einstein condensates are like really cool.   
BEC was predictedby Bose then translated by Einstein but there's some historical controversy here.  
BEC is a coherent state and connected intimately to superfluidity - the superfluid part of a SF   
BEC is predicted by lookig at BE statistics and taking the temp to zero.  
This means that you get lots of bosons in the ground state - wow, loo kat that, they're all doing the same thing!  
BEC is actually a measure of disorder versus distinguishability maybe? Like you could condense at a higher temperature if the gap is really big.  
BEC in a harmonic trap: Critical temperature, condensate fraction, chemical potential, peak density, thomas-fermi radius, momentum distribution, thermal fraction.  
BEC physics: Nonlinear Schrodinger equation approximated by GPE in the mean-field approximation  
BEC is actually, in practise, dependent on interactions between atoms. They need to thermalize as you cool the sample with evaporative cooling, although there are some folks who claim steady-state BEC by optical cooling which doens' t need th atoms to talk to ech other. But anyway, the meanfiled part of the condensate hamiltonian means your single particle states arent eigenstates

## Context & Gap

Bogoliubov theory & superfluidity  
Connecting contact, momentum distributions, and correlations.  
French disagreement  
Contact measurements  
 Probably about time to write a review paper on experiments, no?

## Aim & scope

To reproduce the Palaiseau experiment.

## Contribution & discussion.

Results, error budget, interpretation.

## Issues

Sys: Background  
Stat: Fitting power laws, weak signals

## Method

Sequence & Calibration stages Total number

Well this isn't anything new, I just use the pulsed atom laser in the same way as other chapters. Because I care more about number here I'd like to spend a bit of time thinknig about number errors that could crop up here which are perhaps less important to treat than they are in tuneout, which will be a busy chapter in its own right.   
Raw profiles  
  
 Centering algorithm to align BECs and compile to single shot.  
 Create histogram in spherical coordinates.  
 Compile and apply calibration corrections.   
 Do some fancy fitting - theory-driven pre guesses  
 T from fits and consistnency   
Trap leakage  
  
 Trap leakage is surely number-dependent. Who knows what the law actually is - there is probably some combination of majorana and ??? - perhaps could ballpark what the leak rate should be, see if it squares with what I observe. But yeah it results in an increased flux rate, you can see it in the time trace. The best way to deal with it would be to extend the hold for ages, and then get heaps of statistics, but in the end I wound up using the 20-odd ms window of hold before drop in each case. This still gets to within ??? some margin of error when using all the data I have for a given run. Given the small count rate it isn;t at all obvious that I would be able to do this shot-for-shot; not sure what type of noise this is.   
Background  
  
 This one was easy. I just took the dark count rate from everywhere else; make a histogram displaced on the detector, assuming the dark counts and hotspots are time invariant. Are they? Worth a look I guess. But this is just like camera dark-fielding. That could be a great analogy actually! There are three channels, some background, and, well, the leakage but whatever let's sort of ignore that. Let's show it's Poissonian, maybe? Just to show that it really is uncorrelated events?  
Spin mixing  
  
 Well, this one was OK, I just ran the experiment without the transfer. It shows up fine, I used the center found in the AVERAGE (I think) which might be worth looking at - should it be better to use the centre fixed from the proximate shots? idk. Depends on whether there's genuine drift or just jitter in BEC arrival position. Lol, I guess one could do an allan variance of this one also, that would tell you the best period to average across. Actually, yeah that is kind of cool. Perhaps including that much detail in the 'possible improvements' part. The origin of this isn't clear, hey. It is unlikely to be m=1 atoms, honestly, as they would probably be movable with the Z coil after the 'spray'. The spray moment suggests it's something in the fall path that seems to be an issue - one can control a little bit where it impacts by adjusting push coil timigns - but never quite entirely get rid of it. There is also the question of the m=-1 atoms floating around. But then, calibrating for that would involve using a different RF sequence, perhaps, and at the end of the day maybe the best thing to do would be to find a Rabi transfer setup that pushed everything into M=0 anyhow, and see whether the symmetry is a problem there. Something I'd like to do is to drop an entire BEC, center it very carefully and then reflect it in 3D, and subtract histograms. But that's a mini project for another time and might not really demonstrate much. I would need to include at this point an argument to gauge the systematic error, for example by spreading the entire population uniformly or worst-case just in the power law. Then see how much a difference that woudl make.  
Transfer fraction calibration  
  
 Verification that saturation is not a thing - also kinda pulls out the flux rate wehre saturation is a problem (could do this in 3D, obvs nonlinear but whatever, it's a ballpark)  
 Verifies that m=0 halo is not distorted significantly in the transfer process  
 Verifies that at least within the thermal part, no k-dependence of transfer  
 Model of sweep - would it have been practical to get the max transfer without any momentum-dependences? - what is the doppler shift of the depleted atoms, anyhow? Not to mention the RF field is certainly not spatially coherent hey  
 How accurately is this determined?   
Illustration  
  
 So I will probably include a diagram that shows these broken out - a master diagram of the full measreument stage, for example, and the time profiles that came from each section, and the calibration methods I used... Blah. So this is going to be like a very graphical section, as many of mine will be.

Processing

My general dream for these sections is a graph of the program or some kind of systems diagram, maybe nested screenshots or pseudocode would do the trick and then include the code as an appendix hey?  
  
## Definition}

In the image obtained from the MCP-DLD detector of the shots in which the quantum depletion is observed, the pointwise density of atom detection events in momentum space can be described by

[ n() = ^3 nm(,N,,T) + () + (N0,{})(-kz) ]

The first terms(nm)refer to the detected density of atoms from the(mJ=m)condensates, which depends on the momentum vector()on the total atom number(N), the temperature (T), and harmonic trapping frequencies(). The dark count rate()is momentum-dependent due to its non-uniformity on the detector. The measured trap leak rate()stops when the trap is released, hence the Heaviside()function ensures this term only contributes on the lower side of the falling BEC. Below we adopt the notation((+) = + ())where()is the elevation angle from the plane of the detector subtending from the centre of the condensate. Details of the measurement and calibration of each of these terms is discussed in section calibration.

## Contributions to the model}

The count density from the falling BECs depend explicitly on the trapped population and on the trapping frequencies. This is most obviously true for the condensate of atoms in the(mJ=0)state, and also for the(mJ=1)condensates when all three condensates land undisturbed on the detector during calibration of the transfer efficiency.

In the depletion detection shots, we have observed a remaining presence of(mJ=1)atoms, the cause of which is unclear but is suspected to be due to a collision process with a feature inside the chamber. Regardless of the cause, we calibrate for this contamination (also called elsewhere). The count density of the(mJ=1)states depends on the trapping frequencies also - this could be because the trap centre shifts with changing trap frequencies, changing the details of the spin mixing process during freefall. We cannot rule out the presence of(mJ=-1)on the detector also. Calibrating for this contribution is less straightforward, as discussed below. The temperature dependence of the condensate density is expected to manifest only as a change in the thermal fraction - calculation of the thermal depletion contributions are pending.

The contributions from the dark count rate are assumed to be time-invariant and independent of any properties of the condensate itself. There may be a change in the dark count rate while the detector is subject to high atom fluxes, but these are not expected to be a problem for the following reasons: One, we observe very similar thermal tails both above and below the condensate, suggesting that, at least, the quantum efficiency and dark count rates are not significantly different. Two, although there are some temporary hotspots on the detector during the peak BEC flux, these are only observed co-temporally with the falling BEC. The quantum depletion is detected far beyond the regions where this effect is noticable, and so they are not expected to contaminate the signal.

The leak rate should be expected to be dependent on the BEC number. The most simple model would be an exponential decay of BEC number with some collection efficiency by the detector. There may also be density-dependent collision effects feeding the trap leak rate driven by changing the trapping frequencies, which could also alter any spatial structure in the leak rate due to a shift in trap position.

## Obtaining the Tan constant}

The far-field density of a harmonically trapped condensate is described by contributions from the condensed, thermal, and depleted populations,

[ n() = n{BEC}() + n{T}(). ]

The momentum density of the thermal fraction is [Pitaevskii & Stringari] [ n{T}() = g{3/2}(e{-k2/2m^2}) ] where(T)is the temperature,(= h^2/)is the thermal de Broglie wavelength,({})is the geometric mean of the trapping frequencies,(=1/ kB T)is the thermodynamic Beta, and(g{3/2}(z))is the Bose integral [ g{3/2}(z) = 0=. ]

The short-wavelength density of the BEC momentum density is described by the Thomas-Fermi approximation. The asymptotic momentum distribution is that of the quantum depletion, [ n() = , ]

where(C)is the universal Tan constant, defined by(={||}(2)^3 ||^4 n()), and for a harmonically trapped Bose gas is(a{s}^{2}N0 n0)in the local density approximation [Chang et al].

The asymptotic behaviour of the single-particle probability density function for a particle in the ground state (not in the thermal fraction) is therefore(a{s}^{2} n0), hence the contact constant can be seen as a parameter defining a probability distribution, which we estimate using the procedure described in the next section.

## Calibration

We assume that the RF pulse and magnetic separation of the condensates does not affect their far-field distribution. This may be false if the inter-condensate scattering rate is high (calculation/reference pending). The RF pulse transfers atoms into the different(mJ)states with efficiency(), therefore we estimate the momentum density of the initial condensate via

[n(k) = ({n}0 - 1{n}1 - 1+1)(+))]

where ({n}{1}) refer respectively to the count densities obtained from the measurement and spin mixing calibration shots. We produce a background calibration for((+))by defining background density in the upper and lower hemisphere of({})to be the backgrounds obtained from the dark count and leak calibrations, respectively. We determined the transfer efficiencies()from the initially released condensate to the other(mJ)states by separating the clouds with a magnetic field gradient generated by auxiliary field coils. (Refinement pending). We calibrate the dark count rate by building a spherical histogram centred at the same position on the detector as the(mJ=0)condensate, but 2 seconds after the condensate impact and before the trap loading sequence begins, and calibrate the trap leak similarly but centering the histogram about a point earlier in time than the BEC impact. We calibrate the contamination by stray(mJ=1)counts by running the depletion measurement sequence without the RF transfer, obtaining the distribution of stray counts, plus the background count rate. By subtracting our empirical measurement of the background, we obtain the estimated density of the stray counts,(n1 = {n}1 - (+)).

The total atom number(N)and trapping frequencies()are determined by a pulsed atom-laser measurement, which we describe in detail in [another publication]. A fit of the empirical thermal fraction produces a measurement of the temperature, through which we may determine the condensed number(N0).

Average dark count rate is determined to be(5.6228E3 Hz/m^2), or(9.3713E-16 (m{-1}){-3}). The latter is calculated by converting 1 sec interval into a distance c.f. the centre of mass velocity of the BEC - about 4m or 4E6 micron.

##What next?  
I guess it depends a bit on what the findings are, if any, from the correlation study. Like, does it agree with theory, with the fit, or neither? What’s going on here? Bragg spectroscopy? Link to next chapter # Optical lattice  
## Section intro  
The previous chapter would have dealt with the case where interactions between atoms were present but weak, and so were amenable to solution by perturbative or approximate methods. This means taking a solution for non-interacting systems and making small corrections. The information about the microscopic behaviour can in some sense be averaged out, or accounted for in aggregate, producing a simpler picture that can be mathematically more tractable. There are many instances where the solvability of models is no longer guaranteed even approximately. These so-called strongly correlated systems are ones where the actual evolution is completely different from the non-interacting case.

Philosophica; digression: There is great power in mathematics, in teh ability to produce analytic results for a given model. That is, here is a set of assumptions about a description of the world, and so I can use this to do smoe efficient computation and predict the future state of the world, or some part of it. Models are paradigmatic in physics for this reason, and they represent a great deal of compression, of abstraction of patterns out of context. In some ways they - mathematical models - could be thought of as the quantitative storytelling part of humanity, the things that make it unique - and so there's something really sublime about the exactness of the correspondence with the world, written extensively about in things like 'the unreasonable effectiveness of mathematics' and this correspondence is still explored in the problems of foundations of QM: what exactly does this mathematics say abotu the nature of the world? Why does it run this way? This deep inquiry will always be part of the human spirit, I think, but yeah.  
So, models are great. But you can only get them somtimes - if the algebra is nice, which is usually the case for simple, linear things, symmetric things, or where we can take the large-N limit as in the case of thermodynamics. But once things get more complex we may not have so-called closed form solutions. So we would need approximate methods. The modern incarnation of this is by algorithmic means - given boundary conditions we can solve DEs which are the foundational tool of physics. For probability distributions, rather than pointlike coordinates or vector evolution, things are more complex. That is, you need to worry about things like stochastic master equations and such which are very computationally expensive for quantum systems. There are analytic approximation methods too, using Feynman diagrams or computational tools inspired by them - but yeah that's way beyond scope here. Suffice to say they work - see LHC - but they're expensive I guess.   
  
but the point I guess is that there is a wealth of methods that rely on the in-silico method: A digitized representation of the state of the system with some precision, and a method to evolve the state of the machine.  
THe problem is when you want to express a large - that is, many body and fairly strongly interacting - quantum system, and so the required memory is really big. That is, the state vector is exponentially large in the number of particles, and if you want to store the whole density matrix then things get much worse! And so we need other means. Approximations exist; consider only low-order correlations, for example, like the Gutzwiller methods, or find self-consistent solutions like in Hartree Fock. There are other methods of course - exact diagonalization is the best in the sense that it is, well, exact. And the scaling isn't \*too\* bad for matrix inversion, and once you're done you're done. The thing is you need the memory to store the matrix and thigns get bad pretty quick. And this is just for simulation of dynamics that evolve under a stationary hamiltionian. TIme-varying stuff is probably a lot worse - quenches are OK to model and correspond to strongly diabatic changes in stuff, but ya. The upshot is that using existing machines to simulate stuff is OK, but really limited.   
  
Why is this a problem? Well, large-scale systems are ones we'd like to understand. Superconductors for example. Or big proteins/drugs. Or photosynthesis. So what's the problem? Well, quantum is paradoxically complex; a state vector requires so much infomaiton to describe, but only gives us a sliver of that when we measure it. And nature doesn't seem to have any worries runnign these quantum systems itself. So why not just use these analogies? Use a quantum system to represent a quantum system? You're still faced with the input and output challenge - state preparation is expensive, readout is expensive - but for some processes you mgiht not need big superpositions or massive entangled states, which are much harder to produce (look at some of these scaling results hey), but output is always gonna be an issue I guess. Unless you're really judicious with your output and manage to transform the states you care abotu into ones that are orthogonal in yuor measurement basis I guess. But that's another story.  
The idea is old; back to Feynman in, what, the 80s? So I guess it's not that old and took off really fast! Best to get a date on that one.  
  
So the posterchild for this kind of solution is the general quantum computer - one who can initialize a many-body quantum system in the ground state, then apply arbitrary unitary operations to it. This could include simulating time evolution or by using the quantum state vector as a superposition of computing states, to run calculations. So this would be more like quantum logic. In the end it's all the same. But building an arbitrary hamiltonian is beyond us rn. Fortunately it turns out that all you need is to be able to generate a basis of the lie algebra of transformations that you could peform. And some smart buggers proved that for large quanutm states you can do it with a universal gate set - for universal digital quantum computing at least, which is capable of enacting any unitary operation, and so this includes time evolution of quantum systems. I wuold like to check the equivalen t for time evolution - trotterization isn't quite this, as it boils down to some fairly complicated stuff. There's a whole swath of computing architecture out there right now, and several compilers that will take an operation and break it down into the gate set, so you can just write out your inifinitesimal unitary and go for gold. But yeah, approximation errors. So i think this cahpter needs a lot more focus. And I would like to better understand the problem classes that are within simulation - it's not quite the same as the complexity classes, I guess, but i wonder if you could phrase them as SAT problems if you're trying to make predictions? Tough to generalize exactly what people look for.  
  
Alternatively, well, quantum simulation. Thatis, analogue simulation. Thisidea is a bit less sophisticated than full quantum computing but turns out that there's a map the other way for example some lattice models ahve been shown to be universal, which means that for special choices of the coupling constants one can actualyl get full universal computation - simulating a digital quantum comptuer. Of course this is a completely other technical challenge, requires ptoentially long-range coupling, so your hardware gets really tricky especially for long-range entangling operations, et cetera. So yeah, not super easy. But in the case of specific problems, well, you might nnot do so badly. I wodner if you can scale problems from interacting graphs to equally-coupled models with on-site potential variations. Seems hard, as you'd rapidly increase the number of atoms yuo need, and probably some bizarre modulation on the lattce depth but something worth having a bit of a think about wen writing this chapter anyways. So yeah. Uhm. Im supposed to be writnig abotu quantum simulation; the analogy bbeing a wind tunnel, say, rather than building a digital sim. That said, now the task is usually done extensively in silico because it's \*faster\* and also because it's usually \*cheaper\* - human time is still expensive when you could use -perhaps more- energy in silico for suepr cheap thanks to the economies of scale. Of course, it's not clear when QCs will reach this kind of scaling,a s they need to be error-corrected which is not currently possible at any kind of practical sense, but it's still something people will shoot for. And actually this underscores the issues or the advantage of runniing quantum simulation ebcause well, yeah, there is noise, but for some kinds of noise one hopes that the contribution is someqhat gaussian, right? So you're still gonna try to monte-carlo your way aruond, except you're sampling from physical analogs. The first example of this was Greiner & Bloch, good on 'em. the idea was pitched by Zoller. IDK why they didn't go straight for fermions, but thats' how it went. This would be a good thing to cehck out, the historical reasons. And the key papers therein I guess. but why? For the sake of a good read, and well, yeah, they're kind of the seminal papers in the field! So best go get 'em.   
Sho wat comes next? Um. I was on the tangetn about analog simulators, so there would be space for a survey of the field here. Starting without too much delving into specific models, but that would be a good look at the best in the world, or the first - various lattice geometries, magnetic lattices (how they doing?)  
And quantum gas microscopes - but then hey look ma, as we talked about in earlier chapters, they're not able to resolve single-bdoy momentum spectra. So this is why we would care about using BEC of He\* - currently the best way to do this at scale. Some people have done few-fermion correlators recently (See the thermal correlator/dipolar fermion paper is it?) - but this would be really hard to scale, right? I wonder what the limitations are here. Or could you use a kind of torque to take longitudinal momentum into a lattice-free potential? LIke, use a different dimension of a 2D lattice for this.  
It's also worth talking about the state of the art of QMC - people have done 3D bose hubbard right, and done exceptionally well. DOes this sort of shoot the project in the foot? I wodner. Something to check out.  
BUt then yeah we get to talk a bit about the Bose HUbbard model.

## Gap

[If I don't get to it in the previous chapter re: Bogoliubov and quasiparticles, thsi woul dbe a good plcae to dig into why we would care about momenum correlations. ]. But then yea some extra goodies might be lifted/inspired by the french papers...  
 1-body momentum measurements in optical lattices  
 metastable He: start with bosons  
Bose-hubbard model  
 Consdier a 2-well example; look at the quantum phase transition. Then what. Well, how abotu the 2-body momentum spectrum for BH coupled wells, huh? That'd be a nice illustration. There's probably room to talk about these as arrys of josephson junctions - a good chance to try and understand those if you want to. BUt that'd be an optional part I guess. Part of the survey. Maybe worth tracking down some of hte theses from the Greiner & Bloch labs.

## Aim & scope

To build the dang lattice

## Contribution

Maybe it's best to do this chronologically.  
\* Cut my teeth on AO alignment and eventually building a MOT with an old-school security camera  
\* Assembling new vacuum system  
 Construction  
 Bakeouts - logs?  
 Ti Sub and NEG improvements  
\* LVIS  
\* MOT 2  
\* Absorption Imaging  
\* Dipole alignment  
 \* Misadventures and eventual fluke - thanks to Patrick

## Progress thereafter

\* D  
  
  
Vacuum chamber  
  
Optics build inc dipole  
 So when I started, we actually didn't have a working MOT. THere were the optis but they were dirty and eyah. So they didn't have light either. Had to build a few AO arms - everything except the collimator and zeeman slower I think, and the ZS was set up according to the old paper about that lab, right? THe 'optimized' one. Altough one could certainly design a chirped system to increase yield, but then I wonder what the density/number limits for a MOT are. How much could we actually obtain, given that we then have to push through the feed hold into another chamber?  
 SO yeah. Two MOTs. Anythign exciting to write about? Maybe not - just ballpark the capture velocities maybe.   
 The dipole diagram and control woudl be worth describing. The theory of dipole trapping would belong here as it's not really relevant? Oh, no, I'll have to put it in the alignment chapter - well it realyl sits in the polz part of the tuneout chapter. So that'll cover that I guess. Look over the simulations of the dipole potential. Maybe some simulations of evaporative cooling in the dipole. Open source 'em of course. Would be a fun way to try some auto-optimization algorithms for path optimization. Another way to burn some time, I guess, there are a lot of things in here that are becoming big to-do items! Bu  
Sequence up to evap & dipole  
Imaging system  
 Diagram, some example images and calculation of number  
Small simulations

## Progress meanwhile

New coils, solder catastrophe, new plates

## Issues/What next

Stability: Vibration, temp, vacuum  
Optics: Power, profile  
Automatic optimization  
Broad goals  
 Fermions  
 Quasirandom lattices  
 Are these actually resources or what?  
 Next generation laser tech?

# Conclusion

## Revision of key findings

This thesis describes work that consists of the following scientific contributions: \* Martin was wrong \* New lines \* Weakest transition \* Test of QED \* Partial resolution of QD which didn’t answer much and isn’t decisive re: theory \* work towards a new regime of condensed matter simulation

## Outlook

* future specroscopy - prospects for helium 3
* Lattices - possible directions
* Quantum correlations & foundations experiments

## Afterword

A personal reflection. Was lured in to the academy by the promise of questions like ‘what is the difference between simplicity and complexity’ - when are things simple? When is something intrinsically hard to represent, even if it exists effortlessly in its own right? This drew me to lattice - to build something with the hands that would be so elusive for theory and computing. But yeah - that might have been possible, but I hadn’t the background to make reasonable progress in the three year timeframe of an Australian PhD. Also, frankly, I didn’t have the tuition or support and so was left alone for most of the time, crushing motivation and without praise and squirreled my time away in a simulation that was much more to my theoretical tastes. But hey. I have to find a nice way to dress this up. Most of these words won’t make it. So having got these ones out of the way, the hope is that I can move on from them and think more deliberately about what it is that I would say here.

The chronological order of work is reversed. I should perhaps detail the actual sequence of events - and frame them as pragmatic decisions from which I learned some useful things about scoping projects properly. There isn’t really much that window dressing can add here. But that’s okay - rather than telling the entire truth, just tell enough of it. But don’t let the complexity frighten you out of doing your best to be honest, at least in this tiny sliver, the final vestiage of the self-expression that you’d hoped you’d find the opportunity to explore. This has been an exercise in shedding the ego, however incomplete that process may yet remain.

###A critical reading of science Technology evidently brings us great things. But let us not lose sight of the implicit ‘right to know’ - that we still embody the notion of a conquest over nature, of mastery of the other, the greater, the ultimately incomprehensible. Let us not fall victim to our own arrogances, and recall that in our turbulent times, our investigations come at cost. The nature of reality will, according to our ultimate foundations, remain. But the conditions of society in which we have the ability to pursue them are not guaranteed. One can never predict the outcomes of discoveries, or what miraculous things may be born of new technologies, but in the problem of allocating compute power from human wetware - which still retains a certain je ne sais quois that has not been replicated by industrial-scale algorithms running in silico - we should be mindful of the hubris of endless pursuit of the mastery of nature. This remains ingrained in our mythology - that more advanced technology is always worth the price that is paid. In the limit of free, clean energy and post-scarcity manufacture whereby human labour is eliminated, this may be true. But, unlike the atoms in this thesis, we do not live in vacuum. # Misc

Sean’s thesis: 54k words.

# First writing task:

This thesis builds on and contributes to work in the area(s) of:

* Precision spectroscopy of neutral atoms
* Effects of weak interactions in superfluids
* Optical lattices for quantum simulation

The original contribution of my thesis is: \* Two methods for spectroscopy of ultracold atoms, specifically Helium \* A method to determine the oscillator strength of ultraweak transitions \* A measurement of a tuneout wavelength which validates the predictions of QED \* A demonstration that Bogoliubov theory is actually probably also OK, but then again the trap expansion dynamics aren’t obvious.

1. Although studies in ultracold spectroscopy have examined excited state transitions there has not been methods of this sensitivity or background?. As such, this study provides additional insight into Ways to measure weak things.
2. Although studies in optical lattices have examined spin and configurations there has not been study of 1-body momentum correlations. As such, this study provides additional insight into *insert reasons people care about momentum correlations in condensed matter*.

This study is important to my dsicpline/community because: \* We want to understand superfluidity, and if the depletion behaves differently in Helium that would be important. \* We want to understand wtf is going on with these proton radius measurements

But what excites ME the most about this study is: \* The opportunity to understand some more mathematics/statistical physics. \* The erstwhile chance to think about quantum/classical boundary and the complexity thing. But that’s kind of dead.

This thesis does NOT:

(And that’s okay)

# Exercise with Conal.

Write down a thought that’s been troubling you. I haven’t solved any of the interesting problems that our group has worked on. My work has been ancillary and is not recognized. I have more often been dragged away from opportunities to work on interesting problems. Strategies: Imagine a bully/mean person saying the thoughts. Replay the thought. Say to yourself “I’m having the thought that… I notice that I’m having the thought that…” “Lemons lemons lemons” Word art. Resilient folks: Tend to actively engage in physical and psychologial wellbeing. Clear relax and work times. Activate supports when needed. Hold flexible, balanced, and helpful views. Acknowledge and accept both their strenths and weaknesses non-judgmentally Connect with their values and goals, even at risk of failure.

# 2019-12-15

Bootcamp day 3. Things going ok. Still managing to find more words to plop out.

Taking Inger’s advice and taking some stock of things I’ve been putting off talking about with supervisors: ## Things to talk to supervisors about \* Content: Some expts (particularly the transitions) sorta feel like a sham. Bad scientific practise. This salesmanship feels dishonest, makes it hard to maintain motivation. There was no valuable contribution. \* Didn’t solve any interesting problems because of personal capacity of time to give - so in some senses a bit ashamed of the thesis in many levels, so far from goal/useful \* What other writing blocks do I have? Obvs feeling like there are more crucial/urgent issues to work on outside of physics - feels like I had one good shot at something, missed, and now to pursue it would be counter to my values so there is a sense of loss here. \* Exactly what goes in? For example, Kieran’s derivation of the TO equation or Bryce’s work on the fitting - they’re essential to the project, so how much goes in? Should I make a full recount and then include a pre/post-amble about who contributed what to the project? \* What to include about the lattice? \* why can’t I shake the feeling that you just exploited my labour - the independence I’ve gained is almost despite you rather than because of you

1300-1400 Basically nothing written. Fiddled with a few words. Growing discontent again. Considering the prospect that this would not be possible to put through, that it doesn’t demonstrate sufficient work/smarts/number of problems solved in interesting/useful ways, just doing it for credibility & unlocking ‘next thing’ which might be all too late anyway, at least I feel a little proud of the effort/resilience expended but this hasn’t seen me ‘go rogue’ in much sense, this is not hte physics i thought I wanted, this feels like staring at a wall, I’m blocked by something and maybe it’s a ltitle too much coffee, maybe it’s writing fatigue, maybe I overate, maybe I am genuinely running out of stuff to say in the thesis context, maybe need to read more, this was going to happen eventually but as soon as the flow state stops then the ugly head of self-doubt and self-criticism rears its head again. If I imagine speaking to myself, as another person say, what would I say? Mate you got ripped off for a couple of years then scraped through with a wooden fucking spoon no? Like this thesis is some kind of fucking consolation prize - of no consequence or use, I’ll never be a cold atom physicist and I’ll probably look back on the qualification as a silver fucking lining. Wow. I don’t know whether this is anger or just exasperation. I’m dragging my feet it seems, petulantly whining about how its’ not what I want it to be but there are real questions here about scientific integrity and intellectual honesty. I fell victim to a play by people who are good at the game but who share neither the curiosity nor the vision that I hoped to learn from. So what’s left to learn here? Well, some quant methods, sure. I’ll always be comparing myself to the leader and feeling like I’m falling behind. There

Fuck this. I don't think I should keep ranting forever, go outside man this is beyond uncomfortable this is probably seriously counterproductive. Get some fresh air.

# 2019-12-12

Fleshed out Excel planner on ride home from ANZCOP Overview sheet, segment sheets, word tracker