projectSiegen | Summary

26th June 2015

Abstract

Summarized progress report of my summers 2015 I spent in Siegen, Germany. Main themes of study were Modular Variables, AB effect, Bell Test in continuous variables.

1 May 7-13 [week 0 and 1]

1.1 Impressions

- I started with reading about Modular Variables from 'Dynamical quantum non-locality', a perspective paper from Nature Physics. The paper basically talks about how modular variables capture the essence of a quantum state in a way that no other variable we've considered so far does. It relates this to non-locality in quantum mechanics. This non-locality however is not the one usually considered in the context of say the singlet state. The point they make is that this non-locality arises from the equations of motion (Heisenberg picture), which are non-local themselves (since operators are involved). I found the paper is simple, subtle and interesting.
- Next I started reading a paper titled 'Quantum interference experiments, modular variables and weak measurements' from IOP Science. This I was told is an elaboration of the perspective paper. However, I didn't complete this for I hadn't frozen the topic yet.
- I talked to a person named 'Roope' at the group and his work came across as rather fascinating. I was impressed by his work; it is related to 'measurement equivalent of mixed state'. You make a certain kind of measurment with a certain classical probability. With this type of measruement operators, he was able to show that for compatiblity, commutation is not the best criterion. Of course compatability means that the two measurement can be done simultaneously without affecting each other. He gave a good example from his paper 'Joint Measureablity of Generalized Measruements Implies Classicality', PRL to illustrate the points. His main result was unification of the concept of steering with that of his test of joint measurement, viz. compatibility.
- Modular variables are discussed quite neatly in the book by Aharonov et. al., titled 'Quantm Paradoxes: Quantum Theory for the Perplexed'. I read the first few pages, which are a delight to read (about how paradoxes help, classification of paradoxes etc.). I read the main chapter related to modular variables. I still have some small doubts which I'll clarify soon. Other than that, I have a good basic idea of the concept.
- I talked to 'Costentino' who basically told me that his work revolves around looking at Bells inequalities in more complicated systems. I didn't find that particularly attractive. I also talked to 'Nicolai' and he told me he works on finding interesting states. The kind of states he/they look for are such that the sub systems (partially traced) are separable but the entire state is 'genuinely entangled' (this is the region of state space excluding separable and after tracing entangled states). He talked about witnesses and an algorithm to find an optimal witness and an optimal state correspondingly, recursively, starting from a random initial state matrix. This was ok, but again, not very appealing to me. And last person for the (that) day, I talked to 'Marius' and he told me about how he studies bell's inequalities in decaying particles, and his system of choice was Kions. Again, it maybe non trivial and hard, but didn't come accross as worth pursuing.
- Discussed various topics with Roope including
  - How contextuality is expected from usual understanding of QM, but it is essential to characterize the quantum feature of the system (later Dr. Guehne explained how in Bell's case, local hidden variable is the assumption, whereas here, the assumption is non-contextuality [since we're talking about only one system])
  - Discussed how one can exponentiate an unbounded operator. He seemed to have some big mathematical machinary, but I am still not convinced that it is necessary.
  - I wasn't able to understand how to prove (especially after the new definition of exponentiation) $[e^A, e^B] = 0 \implies [A, B] = 0$. We tried some things, but they didn't help much.
  - Implementing causality etc.

- Maria: She works on hypergraph states. The idea is to represent quantum states as graphs. One can show the limits of usual graphs of this form. The limit is that one can represent say the GHZ state, but not a related less entagled state (forgot the name). She had made a lot of new progress in barely 3 months. She found states which maximally violate the bell inequality in 3 qubits, which weren't known earlier. She was able to even derive conditions on probablities of certain outcomes which consequently rule out various local hidden variable models. In the course of doing this, she'd made various conjectures and proven them, by looking at patterns in certain calculations. I found her work rather interesting and impressive, given that she did it all in barely 3 months (and that she is/was a computer science student), but not something I'd pursue.

- Frank: He started with explaining various usual quantum optics subtopics, such as action of a beam splitter in terms of creation anhilation operators, how its action is similar to that of CNOT, how it can easily convert superposition to entanglement, how a coherent state would pass through it etc. Then he mentioned what's called a P distribution, (stands for P something and Sudarshan distribution) which is given for a state ρ as $P_\rho(\alpha)$, with $\alpha \in C$, and s.t. $\rho = \int P_\rho(\alpha) |\alpha\rangle \langle\alpha|$. This he said is more useful in characterizing quantum properties. Then he discussed how like in the one qubit case, we have a bloch sphere, we can construct similar objects with more qubits also. Then he talked about relating $|\alpha\rangle = D(\alpha)|0\rangle$ with creating states from the extremum state, like $|j,j\rangle$ by applying similarly constructed D equivalents, except in this case with $J_\pm$ instead of a, $a^\dagger$. He claimed that the states in the orbit of this group (group of transformations), don't span the full space. He said that mixed states so produced can then be mapped to entangled state like was done with the beam splitter. The details he said are involving. This also looked interesting, but again, not the direction in which I'd like to work at the moment.

1.2 Return of Ali

- Read chapter five with a microscope, the way I usually read. I came accross various difficulties consequently. One of them was the fact that they seem to have shown that if we assume a periodic potential and apply perturbation, then the angle of diffraction must correspond to that of a maxima. Else the probability of an electron having any other momenta vanishes. Spent a lot of time reading the first section properly, but it isn't quite worth it, it seemed.

- Ali had returned and I sat and explained to him what it is that I had read in the days he wasn't here. He said he'll be away next week again for another conference. In any case, I explained to him the theme of chapter 4. It had to do with the Aharonov Bohm effect; this relates the gauge freedom in $(V, \vec{A})$ with that of ψ to be overall gauge invariant. This consequently shows up as a phase in an interference experiment. The important thing again is that the interaction is non-local (the EM field is zero where the electrons are present).
  - Next I tried to explain to him the details as given the book, but it was soon concluded that it is not a very good idea to continue in that direction
  - He then described his work on exploiting the relation of the form $D(x)D(p) = e^{i\text{some phase}}D(x+ip)$ to get non local effects. He talked about how plotting wigner functions help visualize the interference patterns etc. and then went on to talk about doing bell type tests on these. He has written Phys Rev papers on these topics.
  - Finally, he showed me some work that he had started working on, but hadn't completed which he feels is new and can be a good project for me to extend/establish.

- Finally, I was convinced that to understand various settings, I must be able to visualize, which requires MATLAB. Unfortunately or fortunately, MATLAB wasn't available on the computer and neither was Marius

- Then I thought of some ideas related to observing nonlocal effects etc. No good though.

2 May 18 - 22 [week 2]

2.1 Monday | Multiple Clarifications and Ambiguities

- The contextuality assumption is not equivalent to the assumption of determinism [this clarification was due to Otfried]
  - The idea is that there can be deterministic contextuality. My claim was that if this is so, then it must violate causality. The counter was based on an explicit construction. The idea was simply that the contextuality can be based on the past and doesn't have to rely on the future.
  - Let's quickly understand this. Assume the following matrix of observables, such that along a column all observables can be measured simultanesouly. Similarly, along the row all observables can be measured simultanesouly.

$$\begin{matrix} A & B & C \\ a & b & c \\ \alpha & \beta & \gamma \end{matrix}$$

    * My argument was that if the assignment is deterministic and contextual, then say A has a value depending on the context. However, say I just measure A and nothing else at the moment. Since the value of A had to depend on the context, which I haven't picked yet, therefore it is equivalently the statement that the value of A depended on a future event. Which is in clear violation of causality.
    * This is countered by constructing an explicit example. I say that say I picked B next. Eventually I pick C. Now the values of B and C will depend on the fact that A had been chosen to start with. The context is created by the past, not the future. Apparently something called Mealy machines can be constructed which use memory of the kind, which measurement was made and violate the inequality set by assuming non-contextuality
  - Conclusion: Non contextuality is a stronger condition than determinism, which is violated. Which means that we still haven't ruled out deterministic contextual theories.

- The claim that there're no observables that measure the phase in the expression

$$|\psi\rangle = |\psi_1\rangle + e^{i\phi}|\psi_2\rangle$$

  subjected to $\langle\psi_1||\psi_2\rangle = 0$ was challenged by the following [this was due to Zanna]

  - One could take each of these through a fibre optic and measure its phase wrt another reference laser and find the phase difference OR one could shine a third light $|\psi_3\rangle$ and look at the interference it produces. $|\psi_3\rangle$ is assumed to s.t. $\langle\psi_i||\psi_3\rangle \neq 0$ for $i \in \{1,2\}$.
  - This is a serious issue because it had been proven that no moment of x and p can be dependent on ϕ and therefore (ignoring for the moment infinite series etc.) ϕ should not be observable.

- I further realized a crisis in the understanding of the word *non local dynamics*. I initiated reading (fortunately or unfortunately) the paper titled 'Quantum interference experiments, modular variables and weak measruements'. It does seem to have more detail, although I am not certain how relevant and how precise.

- Other than this, I talked to Zanna and she told me she works on something called Quantum Metrology. This is built on the idea that under a Hamiltonian, an entangled state evolves a lot faster and therefore is a lot more sensitive to the external environment. Consequently, this can be used to amplify detection! The catch is that these entangled states are also highly sensitive to noise because of the same reason. One work around is that you have say $2n$ qubits and let n of them evolve under the Hamiltonian whose properties you want to measure (could be a magnetic field say for instance), viz. $H + H_{\text{nosie}}$ and let the other n evolve (without the magnetic field), viz. under H_{noise}. You can then subtract the results (not being precise but that's the idea) and get a neater signal.

2.2 Tuesday

- Objective was to plot the wigner function for various possible states. To this end, I read (like one should) sections 3.5, 3.6, 3.7 and glanced through 3.8 from Gerry and Knight. I covered topics such as

  - $\int |\alpha\rangle\langle\alpha| d^2\alpha = 1$
  - $\hat{F} = \frac{1}{\pi^2}\int d^2\beta \int d^2\alpha e^{-1/2(|\beta|^2+|\alpha|^2)}.F(\beta^*,\alpha)|\beta\rangle\langle\alpha|$
  - P-distribution, Q-distribution, Wigner distribution etc. and various things related to their derivations which aren't worth typing incomplete
  - Glanced through the idea of why and how all these distributions are related

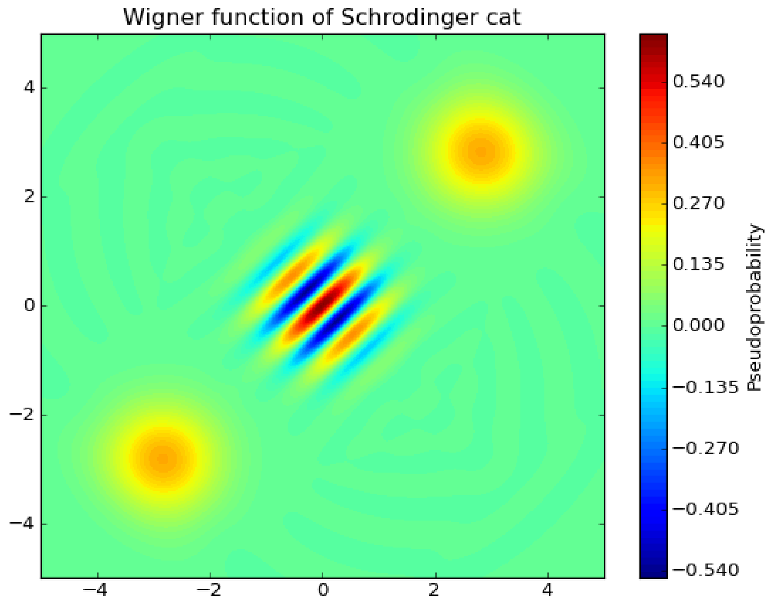
- I attempted getting an expression for the wigner function for the state $|\alpha\rangle$ and what I finally obtained, didn't seem to be real.

  - This I realized while attempting to plot the wigner function using MATLAB. Here's the code for it.

```
%-- 05/19/2015 03:14:58 PM --%
a=linspace(1+i,100+i,100)
clear
lambda=linspace(-100+i,100+i,1000);
alpha=5+0i
f=e^(-0.5*( lambda*conj(lambda) + conj(lambda)*alpha + 3*conj(alpha)*lambda))
f=exp(-0.5*( lambda*conj(lambda) + conj(lambda)*alpha + 3*conj(alpha)*lambda))
a=linspace(-1,1,100)
b=0.5*a
b=exp(a)
```

```
b=exp(0.5*a)
f=exp(lambda)
f=exp((-0.5)*( lambda*conj(lambda) + conj(lambda)*alpha + 3*conj(alpha)*lambda))
f=exp((-0.5)*(lambda*conj(lambda)))
f=exp(lambda*conj(lambda)))
f=exp(lambda*conj(lambda))
b=f*f
b=f.*f
f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha + 3*conj(alpha).*lambda))
lambda=linspace(-10+i,10+i,1000);
f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha + 3*conj(alpha).*lambda))
f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda))
w=fft(f)
plot(w)
plot3(mod(w),real(w),imag(w))
plot3(abs(w),real(w),imag(w))
plot3(abs(w),real(lambda),imag(lambda))
plot3(real(w),real(lambda),imag(lambda))
plot3(im(w),real(lambda),imag(lambda))
plot3(imag(w),real(lambda),imag(lambda))
w=fft(f)
plot3(imag(w),real(lambda),imag(lambda))
plot3(1,real(lambda),imag(lambda))
plot3(ones(1000),real(lambda),imag(lambda))
plot(real(lambda),imag(lambda))
x=linspace(-10,10,1000)
y=linspace(-10i,10i,1000)
r_d=linspace(-10,10,1000);
x=repmat(r_d,1000);
x=repmat(r_d,1000,1);
y=repmat(r_d',1,1000);
real(lambda)=x
lambda=x
lambda=x+iy
lambda=x+i*y
lambda=x+i*y;
lambdaMat=x+i*y;
lambda=reshape(lambdaMat,1,1000000)
lambda=reshape(lambdaMat,1,1000000);
f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda))
f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda));
w=fft(f)
commandhistory
```

- I found that plotting these and other distributions has been made rather simple
  - `http://qutip.googlecode.com/svn/doc/2.0.0/html/examples/basic/ex-10.html`



where their state was $|\psi\rangle = |\alpha\rangle - |-\alpha\rangle$ with $\alpha = 2 + i2$

- Tried thinking about some new things.
  - Is there a Quantum Optics explanation for Heygon's principle?
  - How is splitting of a single photon different from splitting of light (say a thermal or coherent) – [I think this just follows from the beamsplitter's operator]
  - Wavefunctions of 2 neutral particles; how do they explain something like rigid body collisions? – [I think most rigid body phenomena known are microscopically based on EM interactions, so..]
    - In electron interference experiments, we don't quite put repulsion do we? – [I think we usually discuss single particle experiments, where it is kind of unnecessary]
  - Can we in Quantum Optics, measure the overall phase of a wavefunction by using a reference light? If yes, well, then aren't we taught that we can't?
  - BQ: (Bigger Questions) How/Why does suddenly the interpretation of the wavefunction break when we switch to relativistic hamiltonians?

2.3 Wednesday

- Continued fine reading of chapter 5 from Aharnov's book. It has started to make some sense finally. Although I am still not clear about the doubts from last time, the new sections (with the modifications such as $e^{i\hat{p}L/\hbar}$ as opposed to $e^{ipL/\hbar}$) have started making sense. I also seem to have found a flaw in one of the calculations, where it is shown that $e^{ipL/\hbar}f(x) = f(x+L)$ and so on. The important point I learnt this time however, was the following statement: p_{mod} (defined always with $e^{i\hat{p}L/\hbar}$ at the back of the mind) is completely uncertain iff (if and only if) $\left\langle e^{in\hat{p}L/\hbar}\right\rangle = 0\,\forall\, n \in \mathcal{N}$. If one thinks of $\hat{p}$ as a number, and the expectation as these numbers occurring with classical probabilities, then one can easily prove this. In any case, if taken as a definition also (because it is rather sensible: it is possible that $\langle\hat{p}\rangle \neq 0$ but $\left\langle e^{in\hat{p}L/\hbar}\right\rangle = 0 \iff p_{mod} = 0$) then one can show quite easily that detecting the particle at one hole, leads to loss of information about the modular variable! This is at the very least now makes some sense in doing.
  In my CT I thought about how it is possible for p_{mod} to evolve with time, viz. $\left\langle e^{in\hat{p}L/\hbar}\right\rangle$ to evolve with time, but when looked at in the Schrodinger picture, since the wavefunction $|\psi\rangle$ doesn't change (assume the potential to be locally constant), then how will $\left\langle e^{in\hat{p}L/\hbar}\right\rangle$ evolve?
- Next I tried plotting the wigner function for the state $\rho = |\alpha\rangle\langle\alpha|$
  - I used the method where $W(\gamma) = \frac{1}{(2\pi\hbar)^2}\int d^2\lambda e^{(\lambda\gamma^* - \gamma\lambda^*)/2}\mathrm{tr}\left(\rho D(\lambda)\right)$
  - I converted this to two fourier transforms as $W(\gamma_1 - i\gamma_2) = \frac{1}{(2\pi\hbar)^2}\int d\lambda_1 d\lambda_2 e^{i(\lambda_2\gamma_1 + \lambda_1\gamma_2)}\mathrm{tr}\left(\rho D(\lambda)\right)$
  - I proved that analytically, W must be real in general.
  - I evaluated analytically the expression for $\mathrm{tr}\left(\rho D(\lambda)\right)$ for the given coherent state

– I attempted plotting using this script

```
max=40
half=max/2
alpha=-1.5i;
r_d=linspace(-2*pi,2*pi,max);
x=repmat(r_d,max);
x=repmat(r_d,max,1);
y=repmat(r_d',1,max);
lambda=x+1i*y;
% f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda));
% f=exp(-abs(lambda - alpha).^2);
f=exp(-0.5*(abs(alpha).^2) -0.5*(abs(alpha + lambda).^2) - conj(alpha).*(lambda+alpha));
subplot(2,2,1);
contourf(abs(f),10);
subplot(2,2,2);
contourf(angle(f),10);

w=fft2(f);
subplot(2,2,3);
contourf(abs(w([half:max 1:half],[half:max 1:half])),10)
subplot(2,2,4);
contourf(angle(w([half:max 1:half],[half:max 1:half])),10)
% contourf(angle(w(1:100,1:100)),10)

% subplot(2,2,3);
% contourf(abs(w(1:100,1:100)),10)
% subplot(2,2,4);
% contourf(angle(w(1:100,1:100)),10)

% plot3(w)



% lambdaMat=x+i*y;
% lambda=reshape(lambdaMat,1,1000000)
% lambda=reshape(lambdaMat,1,1000000);
% f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda))
% f=exp(-0.5*( lambda.*conj(lambda) + conj(lambda).*alpha - conj(alpha).*lambda));
% w=fft(f)
```

  However, numerically W was not evaluating to be even close to real

  – I then calculated the trace for $|\psi\rangle = |\alpha\rangle + |\beta\rangle$ and attempted plotting again with an improved script, still the results weren't correct. I didn't get the kind of interference I am expected to
  – I proved analytically that W for the coherent state case must be real explicitly.

- Some results I used to cross check my answers:
  – Fourier transform of shifted guassian: `http://www.thefouriertransform.com/applications/gaussian.php`
  – Analytic expression for W for cat states: Gerry and Knight

- Tried thinking about some new things.
  – Generalize the beam splitter to splitting into 3 beams and then n beams
  – Quantum mechanical collision b/w particles
  – Ideas about conservation laws in quantum mechanics
  – If ψ doesn't change over time (schrodinger picture ofcourse), no observable of the system should change with time *however* here (double slit, modular variables, heisenberg picture), $e^{i\hat{p}L/\hbar}$ seems to change with time! Figure out exactly why
  – In Quantum Optics, see how interference of 2 is different from that of 1 [this is perhaps just the beam splitter with $|2\rangle$ instead of $|1\rangle$]
  – Entanglement between particles + Field Theory (Quantum Optics) equivalent to 1 particle delocalized?

2.4 Thursday

- Group Meeting
  - Zanna presented one paper which was titled 'Non Locality with Ultra Cold Atoms in a Lattice', 1505.02902. This was an experimental paper that discussed an algorithm to initialize states and perform measurements to show that there was indeed entanglement with generalization of Bells inequality to multiple particles
  - The next paper she wanted to discuss had already been presented earlier, therefore she omitted it (she was away then)
  - Finally, she made a remark about the third paper titled 'Hate and Love', 1505.04326 which is related to physics somehow.
  - Dr. Otfreid got a cake for everyone (his Birthday :)
- Continued Reading chapter 5 from Aharnov's book [look at your own notes for full details]
  - was able to derive most of the commutation relations etc.
  - wasn't able to understand conservation of modular momentum among certain other things
- Tried thinking about new ideas while was 'forced' to attend this seminar on quantum fourier transform. The one new thing I learnt was that it still hasn't been implemented on a large enough system
  - Tried proving that the equation of motion (heisenberg picture) of $x^m p^k$ will always be local. Then again got into trouble wondering what it is that the exponential does to make it non local?
  - Idea was to either find other non local equations or prove that exponential and its linear combinations are the only ones
- Fixing the FFT
  - Things weren't functioning so went to the 1D case to test with known analytic results. The fourier transform of a real guassian was returning numerically a gaussian with imaginary part
    - The issue was ofcourse the labelling. The system was not assuming the origin to be in the centre.
    - Fixed this by explicitly moving things in an array etc.
    - After more struggle, found that there're functions by the name fftshift and ifftshift to do just that (except that I thought they do the fft and the shift)
    - Since a gaussian was supposed to return a gaussian (and I had part of the translation code still there), got fooled into believing that the shift map is indeed the right fourier transform
    - Spent some time with putting this in the coherent state code (which also seemed to work for obvious reasons) and then finally for the cat state which had to fail ofcourse.
  - Went home and beat my head over it, until I figured my mistake. You're supposed to do fftshift(fft(ifftshift(f))) to get the right result.
    - Apparently I had to do fft2 because its 2d
    - That also didn't fix it
    - The only thing I suspected could go wrong, was the
- Had this final talk by a person from MPI to attend. That also didn't help much. There were some interesting things, but nothing quite related to my field of work.

2.5 Friday

- Roope's lecture (2 + 1.5 hours)
  - He talked about how by simply assuming the usual about ρ and about measurements being linear convex, one can arrive at the $p(x|E,\rho) = \text{tr}(\hat{E}_x \rho)$
  - He also had an exercise session for the problems ofcourse.
- Tried fixing the FFT issue
  - It was the same problem, so I attempted to do the following
    - Plot better graphs (figured how to do 3d graphs) to check whether the imaginary part was indeed small compared to the real part
    - Implemented a function to calculate the inner product $\langle\alpha|\beta\rangle$ (had some minor issues with matlab)

    * Attempted a reformulation of the result of the inner product (that also didn't help)
  - Have decided to leave it for now, until Ali returns (he'd be able to help)

- Chapter 5 | Aharnov
  - Basically figured today exactly how the knowledge of p_{mod} destroys certainty in N_p, resulting in the old $L\sin\theta = (N + \text{fraction})\lambda$, which predicts the same pattern on the screen.
  - This took a little longer than I had expected, but I figured it in the end. Only issue however is the first claim that says if I know transverse x_{mod} because the particle passes through the screen. Not quite clear why.

- Misc
  - Summarizing etc. for yesterday also (didn't have the time then because of the colliqium and seminar and group meet)

2.6 May 23, 24 [Weekend]

- Nothing much academically relevant
  - Thought of a possible issue with the code (fft and ifft) but that wasn't a problem, as I had suspected based on analytic grounds

- Found the Aldi store, cooked, cleaned etc.

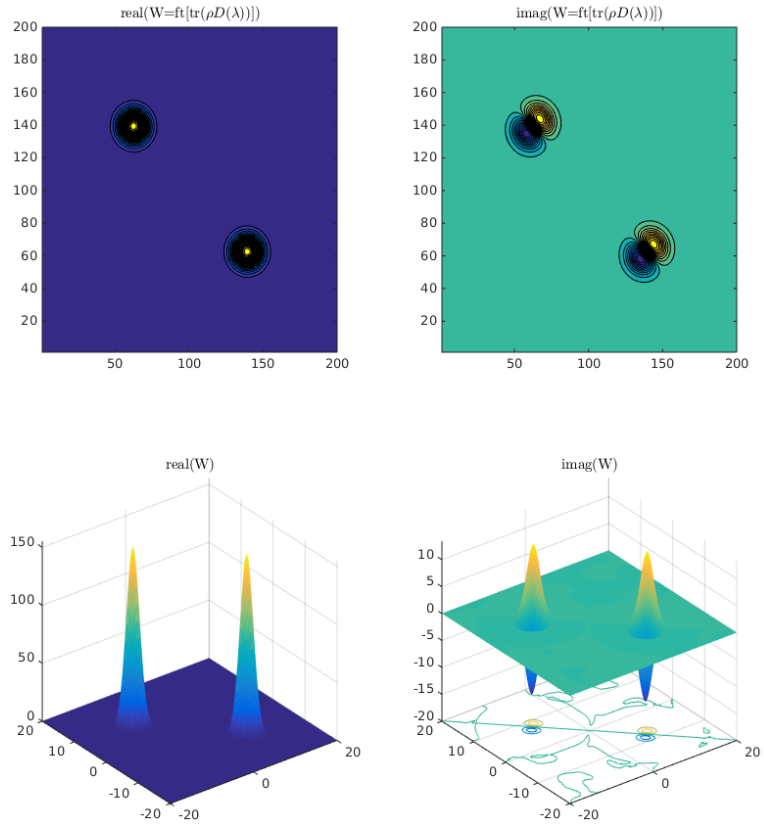
3 May 25-29 [Week 3]

3.1 Monday, May 25

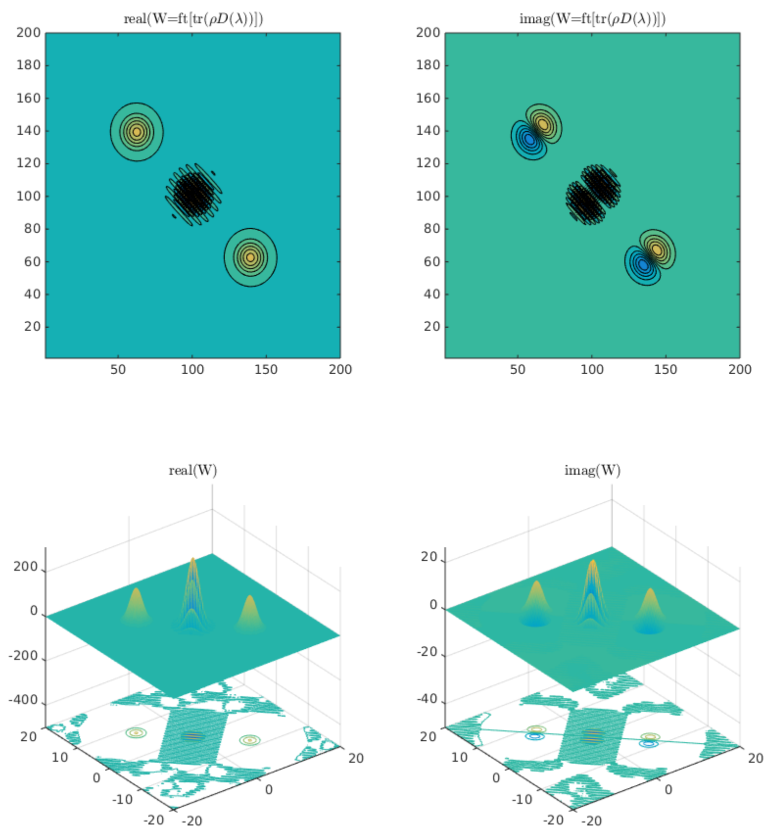
- Essentially spent time reading the NJP paper by Aharnov et al
  - It starts by building on the idea of what changes when we do a which way measurement (WWM), in the context of a double slit experiment
    * Talks about debates over momentum changes before and after, claims some results about how to measure them and so on
    * Discusses basic strategies of measurement, the moments and the fourier transform approach, justifies how the latter is better
  - Talks about modular variables (this is essentially the same as that in the book) except that he discusses
    * How the parity operator may be used to yield the value of the local phase
    * Explains conservation of modular momentum slightly better than it is done in the book (basically using the Heisenberg picture and time evolution etc.)
    * Explains the consequence of a WWM on the modular variable
    * Justifies why it is impossible to measure these non local interactions
  - Discusses how indeed the non local effect may be observed using weak measurements
    * <This I didn't read completely, however I read about weak measurements from this link [http://quanta.ws/ojs/index.php/ till page 5 or so>
      · Weak measurements are based on the idea that the quantum system is coupled to a measurement device, which is itself quantum mechanical.
      · The usual projective measurements are done on the measurement device (which is quantum) after letting it interact with the quantum system
      · The outcomes then are used to determine properties of the quantum system, with disturbance which can be made arbitrarily small
      · Then there're more advanced methods where you pre and post select etc. which I haven't fully read about
    * Apparently, weak values have interpretation which is not yet fully accepted and I was advised not to pursue this just yet.

3.2 Tuesday, May 26

- Roope's Lecture(s)
  - Roope today discussed how to generalize evolution of states, following a similar minimalistic assumption approach
    * He defined Channels and Operations (based on whether they preserve the trace or decrease it) as maps from $S(\mathcal{H}) \to T(\mathcal{H})^+$ (meaning from the space of ρs to that where the trace can be ≤ 1 but must be ≥ 0)
    * He put various constraints on the map and added an extra assumption about complete positivity
    * He then discussed the Schrodinger and Heisenberg picture in the context of these operators. The statement is basically that we consider $T^* = \mathcal{L}$, where T^* is the dual space of S (in finite dimensions, apparently they're same). Then $\forall\, N : T \to T$, $\exists\, N^* : \mathcal{L} \to \mathcal{L}$ which is defined by $tr[N(T)E] = tr[TN^*(E)]$. Afterwards, during the exercise session, he derived various results using just this statement
    * Then he went on to describe Steinspring's dialation thm, which is essentially a statement about how to characterise the most general 'evolution' of a quantum system, in terms of a unitary. The claim is that for a given channel $\epsilon : T \to T$, $\exists\, (\mathcal{H}_E, \xi, U) : \epsilon(\rho) = tr_E(U(\rho \otimes \xi)U^\dagger)$ which is saying that the channel is just a unitary evolution of the state in a larger space, which is later traced out.
    * Finally, he went on to talking about the Kraus Decomposition, which characterizes a channel by giving an explicit decomposition. [I'm skipping the details now]
    * And in the end, he described a way for testing/characterising Completely Positive maps.
  - Exercises
    * There're various things he did (TODO: complete this section)
- Discussions with Ali
  - Decided to discuss the Aharnov material tomorrow
  - I talked to him about the old issues, which I thought I had clarified, but I hadn't indeed. He clarified in specific that you can't infact measure the overall phase, even with another quantum particle (I need to do some calculations to be absolutely certain though)
  - While explaining the calculations of the procedure for finding the wigner function using fourier transform, the error in calculation popped out.
  - Talked about how an ambitious but worthy problem is to come up with a test that can probe dynamical non locality, as opposed to non locality caused by quantum correlations
  - Decided that the small achievable goals should involve implementing various situations and various tests and see how dynamical non locality violates these inequalities etc.
- Plotting Wigner function
  - This finally worked today



This is for the mixed state $\rho = |\alpha\rangle\langle\alpha| + |-\alpha\rangle\langle-\alpha|$



And this is for the superposition state $\rho = |\psi\rangle\langle\psi|$ with $|\psi\rangle = |\alpha\rangle + |-\alpha\rangle$ (with the normalization messed up though)

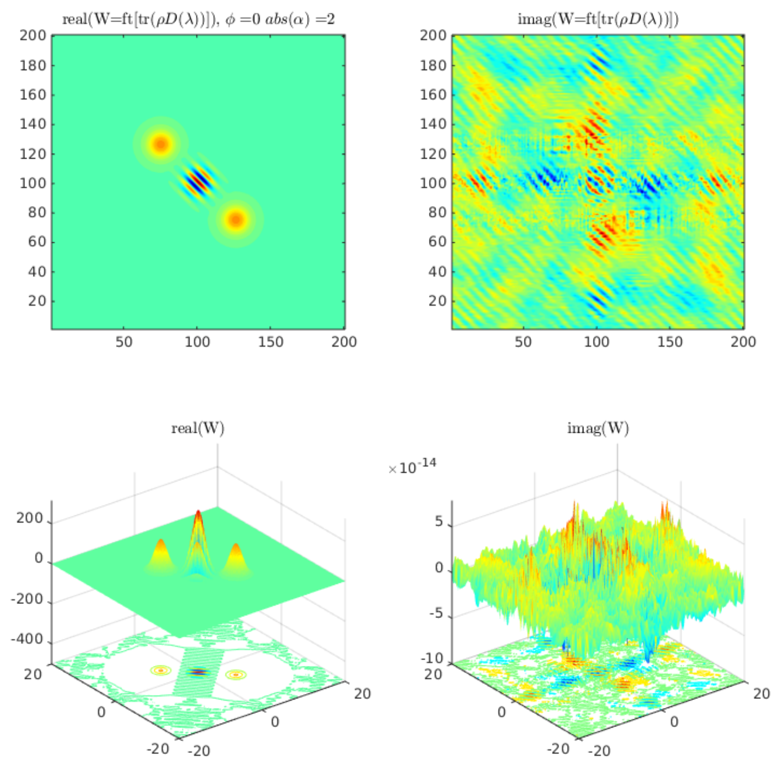
- The issue was essentially that I had messed up one step. I failed to put the extra phase when I evaluated $D(\lambda)|\alpha\rangle \neq |\lambda+\alpha\rangle$. There's a phase which I had missed.
- Here's the working code

```
function wigner()
    res=20;
    max=res*10;

    par=3.0;
    xMax=res;
    phi=0; %pi/5.0;
    phiPhase=exp(phi*i);
    alpha=par + par*1i;
    beta=-par - par*1i;
    r_d=linspace(-xMax,xMax,max);
    x=repmat(r_d,max,1);
    y=repmat(r_d',1,max);
    lambda=x+1i*y;

    f1=cohIn(alpha,alpha+lambda).*dPhase(lambda,alpha);
    f2=cohIn(beta,alpha+lambda).*dPhase(lambda,alpha).*phiPhase;
    f3=cohIn(alpha,beta+lambda).*dPhase(lambda,beta).*conj(phiPhase);
    f4=cohIn(beta,beta+lambda).*dPhase(lambda,beta);

    %mixed state
    %f=f1+f4;
    %superposition state
    f=f1+f2+f3+f4;

    w = fftshift(fft2(ifftshift(f)));

    figure
    subplot(2,2,1)
    contourf(real(w),40,'EdgeColor','none','LineStyle','none');
    title('real(W=ft[tr($\rho D(\lambda )$)])','interpreter','latex')
    subplot(2,2,2)
    contourf(imag(w),40,'EdgeColor','none','LineStyle','none');
    title('imag(W=ft[tr($\rho D(\lambda )$)])','interpreter','latex')



    subplot(2,2,3)
    surfc(real(lambda),imag(lambda),real(w),'EdgeColor','none','LineStyle','none','FaceLig
    title('real(W)','interpreter','latex')

    subplot(2,2,4)
    surfc(real(lambda),imag(lambda),imag(w),'EdgeColor','none','LineStyle','none','FaceLig
    title('imag(W)','interpreter','latex')


end


function out=dPhase(alpha,beta)
    out=exp(1i*imag(alpha*conj(beta)));
end

function out = cohIn(alpha,beta)
    %both versions work fine :)
    out=exp(-0.5*(abs(alpha).^2)-0.5*(abs(beta).^2) + conj(alpha).*(beta));
    %out=exp(-0.5*(abs(alpha-beta).^2) + i*(imag(conj(alpha)*beta)));
```

end

3.3 Wednesday, May 27

- Improving yesterday's code
  - Implemented animations to see the effect of adding a phase ϕ as $|\psi\rangle = |\alpha\rangle + e^{i\phi}|-\alpha\rangle$
    - The maxima and minima must swap when we put $\phi = \pi$ and this was verified
    - Doing the animations required some more streamlining of the code
  - Analytically, the imaginary part should be zero (of the Wigner function, which is a result in our implementation, we get after fft).
    - This was not working well. The imaginary part was about $\frac{1}{10}^{\text{th}}$ of the real part, which is not good at all. Worse still, increasing the resolution had seemingly no effect.
    - This was fixed by realizing that FT for a real symmetric function should be real. Now even for my 1D Gaussian, this was not working well. Which meant that the gird wasn't aligned well. Putting the number of cells from n (say) to $n+1$ fixed the issue! The imaginary part wouldn't go beyond 10^{-14}, while the real part was of the order 10^2.
    - Here's the output



      I'm not putting the code this time. Its there in the directory.
  - Talking to Ali
    - Building on post discussion with Otfried, the idea was to understand better why while the moments are phase independent, how $\left\langle e^{\frac{i\hat{p}L}{\hbar}} \right\rangle$ was phase dependent (phase similar to the one in the previous context, further assuming that ψ_1 and ψ_2 are not overlapping, if $\psi = \psi_1 + e^{i\phi}\psi_2$). The idea basically is that,
      - the assumption, as stated above forces us to consider non analytic functions only, viz. $f(x) \neq \sum \frac{f^n(0)x^n}{n!}$.[1]
      - since $\psi(x)$ is non analytic, $\psi(x + x_0)$ is also non analytic.
      - From the definition of $\hat{p}$ as the generator of infinitesimal translations, we have $(1 - \frac{i\hat{p}\delta L}{\hbar})\psi(x) = \psi(x + \delta L)$. If I put $\delta L = L/N$, and apply this operation N times, s.t. $N \to \infty$, then I can write $(1 - \frac{i\hat{p}L}{\hbar N})^N \psi(x) = e^{\frac{i\hat{p}L}{\hbar}}\psi(x+L)$. For this, I didn't have to assume that $\psi(x)$ is analytic. Call this definition 1. (TODO: Prove this)

[1] Essentially you're saying that you can't find a convergent sequence for f around some point x_0, viz. $\nexists a_n \; s.t. \; f(x) = a_0 + a_1(x - x_0) + a_2(x - x_0)^2 + \dots$ which essentially means that the taylor expansion doesn't exist for f. Easiest example is $1/x$.

· If I assume $\psi(x)$ is analytic, I can write $e^{\frac{i\hat{p}L}{\hbar}}\psi(x) = \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \dots\right]\psi(x) = \frac{i}{\hbar}\sum \frac{\psi^n(x)L^n}{n!} = \psi(x+L)$, which is perfectly valid. However, since $\psi(x)$ is not analytic, $\psi(x+L) \neq \frac{i}{\hbar}\sum \frac{\psi^n(x)L^n}{n!}$. Yet, by definition 1, the RHS can be obtained even for non analytic functions. I call this definition 2: $e^{\frac{i\hat{p}L}{\hbar}} = \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \dots\right]$.

· Conclusion: Since
(a) the action of $e^{\frac{i\hat{p}L}{\hbar}}$ by definition 1 holds for non analytic functions and
(b) an operator is defined by it's action on its domain,

it is safe to conclude that when extended to the domain of non analytic functions, $e^{\frac{i\hat{p}L}{\hbar}} \neq \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \dots\right]$, viz. definition 2 doesn't always hold.

* He suggested I read the paper by Aharnov on using modular variables in the Aharnov Bohm effect to explain when it happens
* Some of the ideas that came up include
  · Figuring out how a guassian state (analytic) produces results different from non analytic states using the numerics, for expectation values of the displacement operator
  · Device a way of seeing the effect of non local potential change in the interference pattern, perhaps using the simulation somehow
* On Friday, I have to discuss both the papers by Aharnov and the material from the book with him. I have to review my doubts about Ali's own manuscript about testing non locality
* The general direction of the project must be to device simpler questions such as the following, and in the process, come up with more intuition to answer the basic question, what exactly is the definition of non locality and how can one come up with a test which responds to dynamical non locality but not to quantum correlation type non locality. The contextuality idea is particularly encouraging.
  · identifying what all inequalities are violated by say a single particle state with this non locality thing, which were earlier only used to study entanglement

– Reading the second Aharnov paper on identifying when exactly does the AB effect takes place
  * Revised electrodynamics partly from CTF, Landau. This was to understand the difference between the two momenta (I get confused otherwise)
  * Revised how the Hamiltonian and $\dot{x}$, $\dot{P}$ for a particle interacting with the EM field is written in QM (from Sakurai)
  * I have some doubts regarding them (such as quantization of EM fields and their relation/analogy with the modular momentum etc. | will perhaps have to read Knight, but I'm not sure if it discusses this)

3.4 Thursday, May 28

- Group meeting in the morning
  – Couldn't understand too much. Costentino discussed two papers. First was about another way of proving the KS theorem. The other was more fancy and related to a specific model of quantum computation. In this model, they were attempting to find out what exactly it is that gives this method its power (characterizing the states). One said it is contextuality, the other negativity of the Wigner distribution. Infact the definition of Wigner distribution for the discrete case is not simple and universal. It had something to do with CSS and magic states as well.

- Resumed reading the paper by Aharnov
  – The initial parts were almost the same, but he has skipped a lot of details
  – The section where he defines $O(0) = \cos[1/\hbar p_x(0)L + 2k_0x(0)]$, I wasn't able to understand the reason for this definition. Moreover, I couldn't understand why he had then defined $\langle O(t)\rangle \equiv \frac{1}{2}\cos\alpha$ while according to me it should be a consequence
    * In this section, I couldn't understand how this relates to signalling interference
  – Next he discusses the AB effect and uses a funny gauge to describe the magnetic flux. He obtains some results in that picture. [I am not sure about exactly how he writes the expression for the phase]. Then he goes on to talk about the same thing in Coulomb gauge, with the calculations given at the end in the appendix. I was able to follow it after some struggle upto the point where he finds the phases. Thereafter, when he evaluates the expectation values of e^{imv_yL}, I am unable to understand exactly how he's working it out. I spent quite some time on figuring it. I couldn't figure the proof of this statement either: $e^{i\left(\vec{p}-\frac{e}{c}\vec{A}\right).\vec{L}} = e^{i\frac{e}{c}\int_{r+L}^{r} A.dl}e^{i\vec{p}.\vec{L}}$ (I know I left some vector signs) which is apparently used somewhere. I also suspect that they haven't, atleast on the face of it, used the right displacement operator. They should use $p - \frac{e}{c}A = P$ instead of $p = \Pi = mv$. This is because as I had figured yesterday, P is the generator of translations, not p.

- I skipped the final section that discusses angular velocity distribution
- In any case, I have an overall idea of what is being done. I must now begin thinking out of the box. I have enough information about the state of the art in this area.

• To read the Aharnov paper, I had to look at chapter 4 of his book
  - Read from the printed copy and made in notes comments
  - Some statements such as multivaluedness of the scalar function Λ and subtlties about where it can be assumed single valued need to be looked at, if there's time
  - It took a few hours but it wasn't too hard to understand
  - Left section 4.5 onwards since it wasn't relevant for the paper

• Also, while eating, I started looking at the '50 years of the AB effect' seminar series on youtube. One of them had this paper explained, as I had found even before starting reading this. The explanations may not be easy enough to understand from the seminar, but with some pictures and talking, you get the idea
  - I will continue watching these as they give ideas

• Idea thinking time
  - Couldn't do much. I thought you could somehow use the AB effect or it's modification to detect objects without touching them (like the interference experiment)
  - I liked the idea of assuming contextuality and locality
  - I figured there're two directions to think now. One is the tests direction and the other is this, thinking of experimental setups with no classical analogues, which show some novel quantum feature.
  - I realized that even when EM fields are absent, $\phi = 0$, $A = 0$ we continue to have the gauge freedom in the wavefunction. Was thinking if this can be somehow used to detect neutral particles at a distance
  - I also thought how radiation is explained classically as a particle accelerating (say going in circles). I was wondering if we can answer questions like two lumps of a particle rotating, with the hope to understand why having angular momentum is not enough to radiate (eg. atoms etc.).
  - I was also thinking if we can make two particles talk by this gauge freedom, instead of one particle. Perhaps we can see if there's an analogous AB effect with photons interacting with atoms, so maybe then we can detect atoms with AB like effect, without touching them. That'll be insane! :)

3.5 Friday, May 29

Today I was working all day, but I didn't write much, I couldn't come up with anything new (not even worthy ideas) and yet, as I said, I was working.

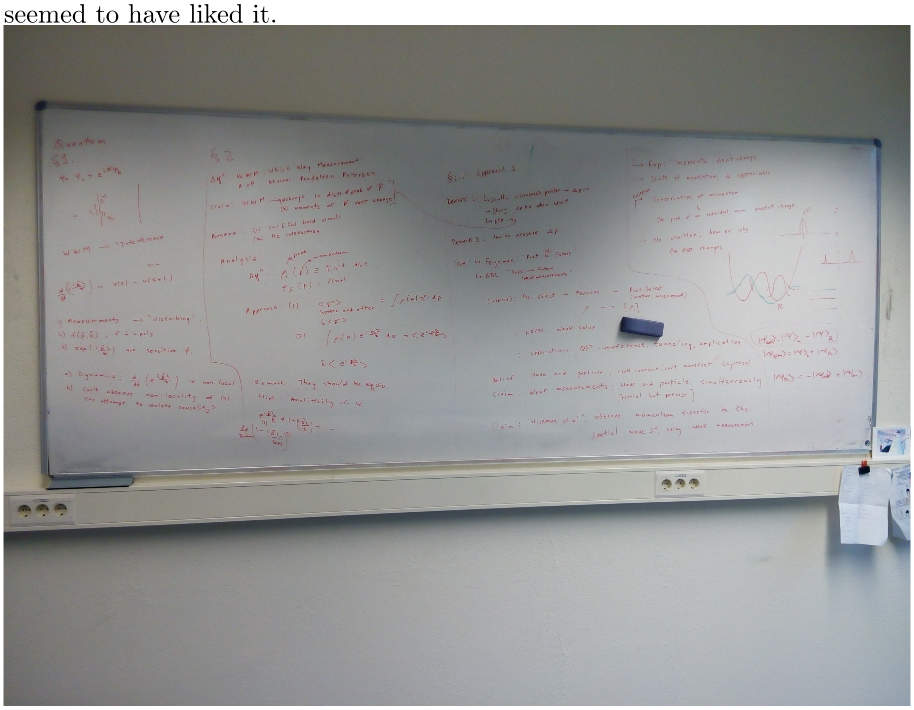
• AB effect for Photons: I had this idea yesterday and today I started looking for prior work on the field. Here's what I found.
  - I realized that I had read some discussion on AB effect in Ryder. This wasn't although directly related to what I was looking for, but was interesting nonethless. This is because he introduces some interesting mathematics to describe the topology of the space, that is attributed to the effect.
  - One paper (phys rev) which was rather old, and this had this insane idea of using a solenoid inside the feynman diagram for photon photon scattering.
  - There was one that I found first, which had some effective gauge potential generated in some specific setup. This was a nature paper.
  - Then I found auxiliary papers like
    * No signalling in AB effect (something I was thinking is worth proving), again PRL | this is a barely 2 page paper!
    * Some new non local effect (it said AB is not needed for showing that type of non locality)
  - [after Roope's lecture etc.] I realized that in the whole scheme of AB, we used gauge invariance with classical EM fields and quantized particles, fermions.
    * I then started wondering how must one think of potentials, when I am talking about photons themselves. To that end I looked at Knight and found that in the atom field interaction, they choose a specific gauge and then proceed. So that's useless for me
    * I started wondering about other things such as what must the experimental setup be to even investigate the effect

    * I settled eventually with the conclusion that I must somehow first see the effect of gauge freedom on the atom field interaction and then proceed from there somehow

- Then there was Roope's lecture
  - We had already discussed that given a state ρ we had found that we can evolve them through channels or operations and then the classical probability of a measurement can be obtained by using POVMs as described last time.
  - Today the aim was to obtain the state of the system after all of this has happened. To that end, he defined a further generalization: The measurement Model, and later defined something called Instrument.
  - The details are there in the notes, but I couldn't quite keep up today.
  - It may be mentioned here that the instrument part is much like the von-neumann measurement, because in this, they consider the measurement operator also as quantum.

- Bewildering Ideas
  - Now as it turns out, the issues I had associated with relativity, order swapping and measurments etc., were infact discussed in Aharnov's book.
  - Aharnov's book had also considered some kind of quantum fluxons, which were basically created to make the solenoids exist in superpositions etc. which was again similar to what I was trying to do
  - I watched this seminar (according to what I'd decided yesterday) titled "Phase Factors, Gauge Theories and Strings" and in this, the person systematically discussed the origin of potentials and gauges and their physicality.
    * The most interesting aspect was Maxwell's justification for introducing the A field. He was unhappy by the *non-locality* of the faraday induction law. To make this effect local, he introduced A. And so this non locality (if A is not accepted as physical) goes back all the way to classical electrodynamics. This makes it harder to understand the precise meaning of non-locality in quantum mechanics.
    * There was also this interesting aspect about how Dirac had predicted that the electromagnetic charge must be quantized, if there exist magnetic monopoles that we can't detect. Infact AB is a non-trivial example of where Dirac's approach proves A field has physical significance and observable effect.

- Discussing with Ali
  - [*possibly new*] AB effect for Bell Test: Ali suggested we do a double AB effect with entangled states and look at the correlation. He said this is a new result because in continuous variables, you're doing something you do with unitaries and spins. The math is nearly the same but the physical situations vastly different. Plus he said he hasn't quite seen it before. A little search on the net seems to support his claim.
  - He said this could be added to his paper/note as a physical example for non locality etc. and that we should build on this.
  - I started explaining the Aharnov paper (one in Journal of Physics) and finished till section 2.1 in about an hour. He seemed to have liked it.



- Ideas:
  - Umm, what if we take 2 electrons instead of 1, can we still have AB with a single solenoid?
  - In QFT, the gauge affects ψ which is a field. How does this affect the folk space etc?
  - What must we do in QO?
  - Can there be shades of symmetrization? Identical particles which are almost identical?
  - [repeating?] EM field by electron localized with angular momentum L and electron delocalized with L.

3.6 [Weekend]

Nothing to show for this time. It was a strange weekend.

4 June 1-7 [Week 4]

4.1 Monday, June 1

- Chapter 4 notes
  - This is from Aharnov's book. I started writing the full notes, and realized that only the kinematic momentum $\pi = p = P - \frac{e}{c}A$ is infact gauge invariant because

$$\int \psi^* \pi \psi = \int \psi^* (P - \frac{e}{c}A)\psi = \int \psi'^* (P - \frac{e}{c}A')\psi'$$

    infact, now that I think of it, it is essentially just $d_\mu \psi \to D_\mu \psi$ so that when $A \to A'$, $\psi \to \psi'$, we have $D_\mu \psi \to e^{i\lambda} D_\mu \psi$. This in turn ensures that

$$\int \psi^* D_\mu \psi \to \int \psi'^* D'_\mu \psi' = \int e^{-i\lambda} \psi^* e^{i\lambda} D_\mu \psi = \int \psi^* D_\mu \psi$$

    So that means that D_μ corresponds to the kinematic momentum (even in QFT, man I'm an idiot, didn't see it then) . P however is not gauge invariant.
  - I also had some doubts about how Newton's law wasn't holding. The electron is pulling/pushing the plates together, but there's no force being applied on it. I mean if I consider this situation classically also, no double slit, just an electron passing by a capacitor, mustn't Newton's law hold?
  - It's not complete yet, the next section I still have to write.

- Resumed presenting the paper to Ali
  - Today's session was particularly interesting. First, I have to learn about assumptions behind the Bell test more carefully.
  - There's a striking similarity between modular momentum and bloch vector formalism, brolloigne zone etc. from condensed matter physics
  - Doubts indeed
    * The statement that the probability changes but the moments don't, is self contradicting and needs to be understood well
    * How is it that even upon free evolution, at no time does any expectation depend on ϕ (or α whatever the relative phase is called) in the double slit. In fact this seems to contradict what happens physically.
  - Ali suggested that in the general repeating case, the eigenvalue of the displacement operator has (for an N lumps wavefunction), the value $\propto \frac{N-1}{N}$, as opposed to $\propto \frac{1}{2}$
  - [*possibly new*] The most important interesting thing we did today, we came up with a situation where changing the potential like a step function (non-differentiability is at the place where the wave function vanishes) plaussibly should cause a change in interference pattern, something you would not expect classically at all. So this might be an interesting way to test non locality of equations of motion. However, now that I think of it, I am slightly sceptical about whether the simulation will pick it.

- Ideas
  - Use the modular variable picture in the Bell state + AB effect setting
  - Cantor set and diffraction [http://arxiv.org/pdf/physics/9802007.pdf]

- How to think of one particle state as really, a two particle state say $|\psi\rangle = |\psi_1\rangle + |\psi_2\rangle$, then I can always write this as $|\psi\rangle = |\psi_1\rangle \otimes |0\rangle + |0\rangle \otimes |\psi_2\rangle$ and then this is essentially like a Bell state!

- Walking back
  - Figured analytically what the phase must be, quite naturally
- Misc/Health
  - Gym

4.2 Tuesday, June 2

- Documentation
  - Fixing the readme
    - Updated for the past week
    - Added an image from the wigner function plot
  - [*possibly new continued*] In addition to the following:
    "Hey! Where are you mate? I hope everything's good. While walking back yesterday I figured the action of the step potential. It is completely trivial. And yes, it'll yield a state with that phase difference you said. Besides, I realized that this is pretty much what happens in the electric AB effect. This aspect actually can be explained even without invoking the gauge argument it seems.
    Let me know when you return. We should discuss this.
    Sincerely – Atul"
    I realized that this means that our situation is infact an AB effect for photons! Which is certainly something interesting because it has apparently nothing to do with gauges.
    - Worked on documenting the idea in a proper note | its far from complete
- Ideas
  - Like in the case of Schrodinger picture, everything extractable about the system, is encoded in the wavefunction ψ. Similarly, can there be an observable in the Heisenberg picture, s.t. this operator and the initial state contain everything extractable about the system? [iThink: This is plagued with the non-analyticity issues]
  - Was wondering if the rest mass energy in the hamiltonian can be measured by a method like this. It requires superposition of particles with different masses. The only thing that comes to mind, is the W bosons. Apparently I'd also thought of asking Marius because he seemed to work on particles that live in superpositions, but his kayons decay model didn't quite fit (from what I could gather).
- Misc/Health
  - Gym

4.3 Wednesday, June 3

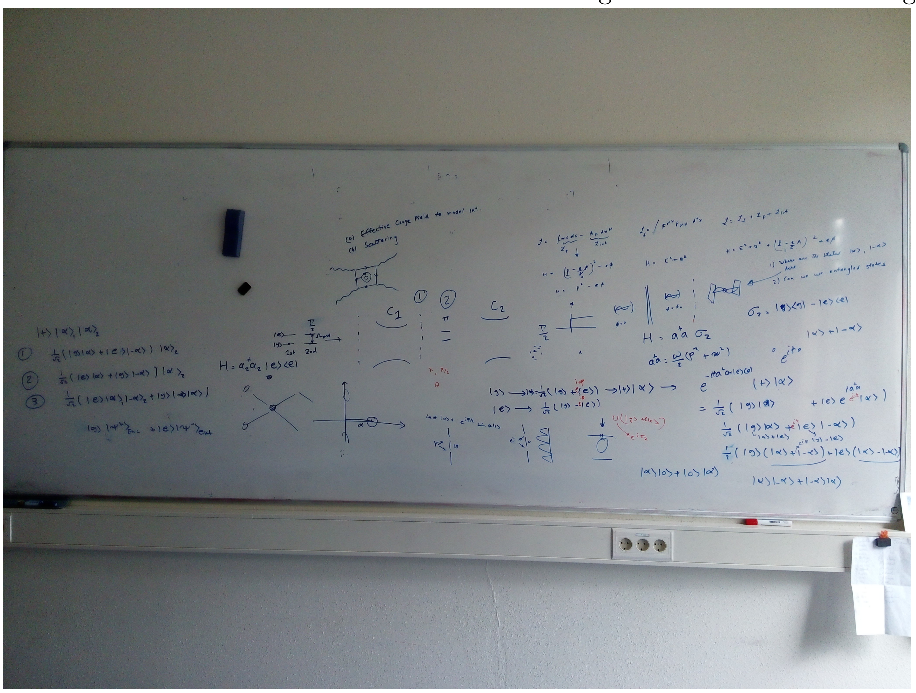
- Documenting/Thinking/Notes
  - Working on the Photon AB effect documentation
  - Although the idea is pretty simple, creating a step potential experimentally is sounding harder than I thought.
  - [not started yet] Working on the notes for section 4.5
- Ideas
  - Seminar: The Long Road to the Higgs Boson - and Beyond | https://www.youtube.com/watch?v=nU42nLyVuHI&list=PLg7f TkW11iWmYJ5F5WeNEmAphM_QpW9r
  - How exactly for a photon, will we add the step like potential? I can think of two approaches
    - So the energy is basically $H \propto E^2 + B^2$, can't do anything to that. However, we know if we introduce charges, we'll have in the action, in addition to $S = \int -m\sqrt{dx^\mu dx_\mu} - F^{\mu\nu}F_{\mu\nu}d^4x - eA^\mu dx_\mu$. So perhaps the same old capacitor can be used. Question is where and how? Since the where requires understanding of how the cat state is created, it brings us to

* The next idea (which) is then to first understand how to create cat states (for that also, naively I would need freedom to make arbitrary potentials) and then to exploit that method to implement a step potential.

- Another interesting direction: The splitting of one particle into two doesn't seem to produce the same results afterall! If you consider $|\psi\rangle = |\psi_1\rangle \otimes |0\rangle + |0\rangle \otimes |\psi_2\rangle$ instead of $|\psi\rangle = |\psi_1\rangle + |\psi_2\rangle$, and apply the step potential, then (assuming you apply the step potential to the vacuum also) the results are different. If you insist on the representation being accurate, you must supress the evolution of the vacuum. But what's the reason?

- Discussion with Ali and its consequences

  - So Ali described methods to both create a schrodinger cat state and an entangled cat state $|\psi\rangle \sim |\alpha\rangle |-\alpha\rangle + |\alpha\rangle |-\alpha\rangle$



  - It was almost trivial to see how an atom can be used to create the phase we wanted. The issue is however that this is not an effect of adding a step potential to the photons. The effect is similar to adding a step potential. So this is not a genuine AB effect setup.
  - To construct a setup where this emerges, it was concluded that since from $L = F^2$, we get $H \propto E^2 + B^2$, which has no freedom whatsoever, therefore we must include interactions.
    * For the fermion in classical field, we use $H = \frac{1}{2m}(P - eA)^2 + eV$ which arises from $L_p + L_{fp}$, we must have analogously therefore, $L_f + L_{fp}$ to obtain the kind of H that we really need.
    * This can be justified further by arguments such as 'given the field configuration, find the motion of the particle' requires us keep the L_f fixed, therefore it doesn't contribute to the dynamics. Similarly, we must 'keep the charges and currents fixed, and find field equations'.
    * However, one must realize that $H = p.v - L$ where L must include even parts that're not varied. This I have to figure out.
    * Assuming though that $L_f + L_{fp}$ are enough, I must obtain the relevant P and relevant H and see what kind of charge configuration I can use to construct a step like potential. So this has two steps
      · Construct H and find the appropriate charge distribution | one intuitive possibility is to use two cavities and place a capacitor in between them
      · If the intuitive possibility has merit (and this was anyway motivated by being closer to the classical result),

- Misc/health
  - Goto the bank to get cash for the rent | the bank got shut :(
  - Gym

4.4 Thursday, June 4 [Officially off in Germany]

- Social Protocols
- Health
  - Gym

4.5 Friday, June 5

- Ideas
  - Seminars:
    - There was one on neutrino oscillations and geometric phase relation
    - QFT related some specific solutions in special geometries etc.
    - AB effect in coherent states of electrons
  - Convinced that instead of 'quantize fermion and not the field', I have to 'quantize the field, not the fermion' with the very important difficulty: To quantize the EM field, one must fix the gauge. Thereafter, how would I adjust the potential? So this I think by itself can consume a month easily.
    - And come to think of it, this way

Usual AB	Reverse AB
Q: $e^{i\phi}$, C: V	Q: $e^{i\phi}, V$, C:?
Gauge not fixed for quantization	Gauge is fixed for quantization

- Ali
  - So he emailed
    - A review article on how to create the schrodiner cat states
    - A PRL that talks about a new entanglement test using modular variables (yes and I couldn't come up with it) \| plus it looks like I'll have to spend some time reading it
  - Presented the remainder of the NJP paper
    - Pre-discussion remark about 'Finding the right variables for the bell test' seemed to be interesting.
      - I figured that somehow non locality must arise from non analytic functions (such as the step potential etc.) because otherwise they would have usual classical effects
      - If we're talking about dynamic non locality, it must be the property of the time evolution operator
      - The test for non locality must admit two different states, related by time evolution (so it might as well be taken to be two different systems and linked to the bell test somehow)
    - Not much came out of the discussion today: He wants me to look at
      - interference/wigner function for states with superposition of non analytic functions
      - two particle wigner function and then plot $W(x_1, x_2)$ by fixing the value of p_1, p_2
      - come up with the precise way of figuring how to do a bell test with continuous variables using the AB setup etc.
- Reading/Notes
  - Completed the notes for chapter 4 from Aharnov's book
  - Making notes for the PRL paper on when the AB effect happens, investigated using modular variables
- Health
  - Had a headache in the morning

4.6 June 6, 7 [Weekend]

- Health/Gym appointment with the instructor
- Routine grocery shopping, cleaning etc.
- Academic
  - Read the photon-phonon paper (to figure what effective gauge potential they constructed), it was interesting, but in another sense, not as novel as I was thinking it was.
  - Took a glance at the entanglement paper (based on modular variables) and it had some things which I couldn't understand. Implication, I'd have to fine read it.

5 June 8-15 [Week 5]

5.1 Monday, June 8

- Ottfried's lecture
  - It was about Bell inequalities (continued)
  - The picture is a lot more clear now. I think although that there's more to be said
- Working on interesting things
  - [*possibly new*] Aim: Get a better understanding of Modular Variables
    Understand why observing a modular variable is different from observing the full variable and taking its modular part, viz. How modular variables affect the state of the system.
    * Sub aim & progress: Worked out the action of a modular variable on an arbitrary state; viz. precisely defined $\hat{p}\text{mod}L/\hbar$
      Figured a possible alternate definition (intuitively) | TODO: check consistency
    * Sub aim: Dissolve the doubt about $[p_{\text{mod}}, x_{\text{mod}}] = 0$ and $[D(\lambda), D(\beta)] \neq 0$
    * Sub aim: (a) Plot simultaneous eigenstates of x_{mod} and p_{mod}. (b) Are modular x, p complete? Prove or disprove it.
    * Sub aim: Verify construction: (a) Figure out why must $\hbar$ come in the picture? (b) Why is L and $1/L$ the scaling for the conjugate variables necessary? [LATER: You need $\hbar$ because of dimensionality. You need conjugate scaling for commutation to work]
  - [*possibly new*] Aim: Understand the relation between L, θ and N_x, p_{mod}
    * Sub aim: Figure the relation between the simplest L case (spin 1/2) and the double slit
      Progress: Made some. I ran into issues with unexpected things such as, analogue of interference pattern, $V(x)$, and so far, the time evolution of the displacement (angular rotation) seems to be local! So although looks trivial, but might have something deeper.
    * TODO: figure rest of the sub aims
    * What is the coulomb potential equivalent for spin 1/2?
  - [*possibly new continued*] Aim: AB effect for photons | inspired by photon phonon ab effect
    * Progress: Was able to construct an extremely simple setup (essentially double slit with a glass slab!) to claim that this is a valid effective version of the electric AB effect, much like the Nature paper on photon phonon is effectively a magnetic AB effect.
  - Seminars
    * https://www.youtube.com/watch?v=RGbpvKahbSY | Got the basic idea about Bohm mechanics (tried writing the first equation myself and got quite close [its actually quite obvious a step])
    * https://www.youtube.com/watch?v=ZCLMb6HYyjs&list=PLOy5dvWSc5d5mGF4hr0D3j4G0AZFTkVvs&index=3 | Learnt more about Bell's perspective (seems more refined than Aharonov's)

5.2 Tuesday, June 9

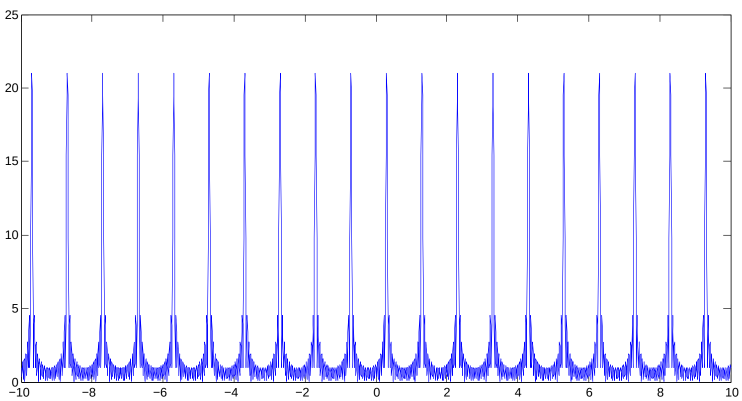
- Health/Misc
  - Emails | Ensuring the GRE decision is correct :(
  - Gym
- Spent most of the day trying to systematically derive the eigenkets of modular momentum and modular position.
  - I had some dimensionalities wrong, which I didn't care for initially, but they were fatal
  - I couldn't find the co-effecients I'd set out to find; eventually looked at the paper and they had derived these eigenkets
  - Of the two eigenkets they'd given, one of them didn't work for me. I was able to fix it, but don't know why the original won't work.
- Some ideas:
  - Is it possible to construct say for a q, p system, a set of operators r_i with $i \in \{1, 2, \ldots n\}$ where they commute and $n > 2$?

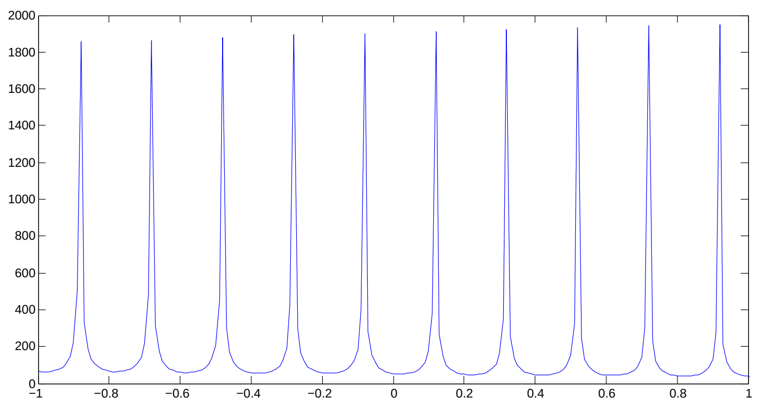
– It seems that asking for more (or general) information disturbs the system less. So in an interference experiment, can I ask which all slits the particle went through? Instead of asking precisely which slit it went through? For instance in the usual interferometer.

- Seminars
  - https://www.youtube.com/watch?v=jKGuGptafvo&index=4&list=PLOy5dvWSc5d5mGF4hr0D3j4G0AZFTkVvs| This is insane. It asks where the photon was, after it has been detected. The answer is spooky.
  - https://www.youtube.com/watch?v=zQ8Bm33o0uI&index=6&list=PLOy5dvWSc5d5mGF4hr0D3j4G0AZFTkVvs | This is about twisters. Is mathematically too sophisticated to understand in a short time.
  - https://www.youtube.com/watch?v=w8x8tyOaxQk&list=PLOy5dvWSc5d5mGF4hr0D3j4G0AZFTkVvs&index=7 | This I just started

5.3 Wednesday, June 10

- Eigenket
  - Derived the result using a slightly different approach. Instead of requiring both eigenkets to be equal, I constructed the ket to be an eigenket of modular momentum and then demanded it is an eigenket for modular position, to fix the coefficients. This did it. The result is $|\bar{x},\bar{p}\rangle = \sum_n e^{in\bar{x}\left(\frac{2\pi}{L}\right)}\left|\bar{p}+\frac{nh}{L}\right\rangle$
  - Analogously one can start with $|\bar{x}+nL\rangle$ and obtain an expression.
  - To prove they are the same, I took the fourier transform of these as $\int e^{i\bar{p}x}dx$ in the position space and upto constant scaling factors, the result was $e^{\frac{i\bar{p}\bar{x}}{\hbar}}$ which proves nothing I just realized. I must do this for an arbitrary p and not for $\bar{p}$.
  - I also wrote a small matlab code [projectSiegen/simplerPlots]
    * Need to fix the x axis after fft: http://stackoverflow.com/questions/13397393/unit-of-fftdft-x-axis, http://stackoverflow.c do-i-obtain-the-frequencies-of-each-value-in-a-fft/4371627#4371627
    * The output/plot is here





where first we have the position representation of $|\bar{x},\bar{p}\rangle$ and in the second plot, its fft (scaling in the second plot is wrong). One can see how the distribution is peaked in both the x and the p representation, thereby manifestly allowing for simultaneous sharp values of x_{mod} and p_{mod}. For this plot, I used $\bar{x} = 0.7$, $\bar{p} = 10$, $L = 1.0$, $h = 2\pi$, $n_{max} = 10$.

  - Still to be done:
    * Equivalence
    * Action of the modular variable on arbitrary kets, equivalence with the guessed result

- Seminar
  - https://www.youtube.com/watch?v=w8x8tyOaxQk | I just cleared a misconception: In Bell's theorem, the only assumption apparently is *locality* and determinism or pre-defined values of the observables follows as a consequence. The only assumption is locality! He showed that even Nielson has this wrong.

- Discussion with Ali
  - Pushing the qubit analogy with the continuous case will probably not help all that much | you have no idea what you're trying to find or answer
  - Defined a seemingly doable and interesting problem as that of constructing a bell test for continuous variables, using the ability to add phases
  - He also told me about how in his paper, he'd linked the displacement operator in certain dimensions to the pauli matrices. That was certainly interesting.

- Misc
  - Gym + purchases

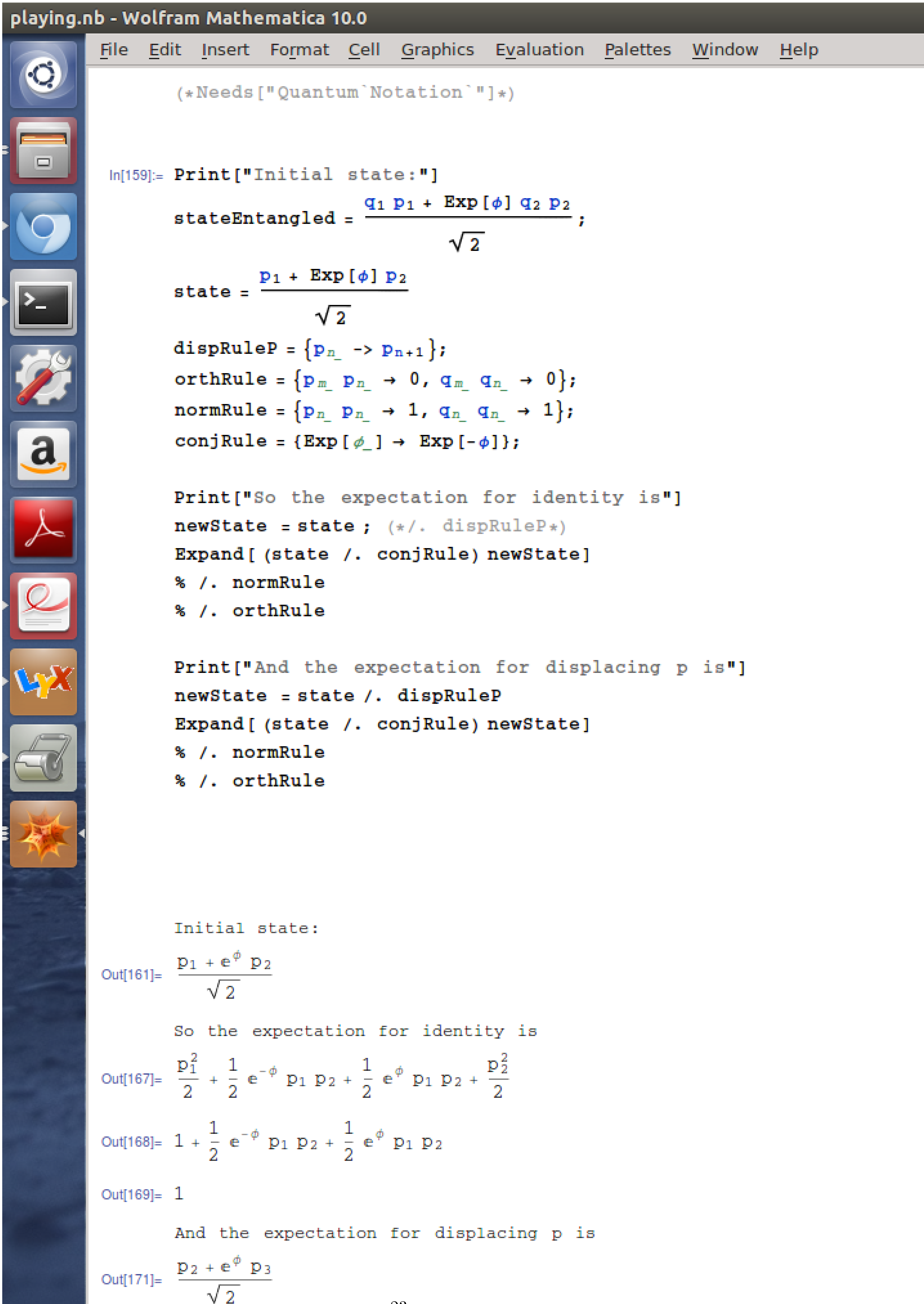
5.4 Thursday, June 11

- Misc/health
  - No gym, overslept (forgot to put the phone off of silent)

- Seminar
  - https://www.youtube.com/watch?v=Xm93utUP7YE&list=PLOy5dvWSc5d5mGF4hr0D3j4G0AZFTkVvs&index=8 | hilarious, and has gotten me to start liking Bohmian mechanics. Together with yesterday's seminar (that suggested that the consequence of Bell's prediction is that locality is violated), it seems like the way to go. While on the other hand, it is almost like Newton saying how can gravity act at a distance and Einstein coming up with a mechanism. Perhaps the times are similar.

- Continuous variable bell test
  - So basically in the bell test, there're Alice's measurements, which are along z, x and then there're Bob's measurements which are along z', x'. The state is $01 - 10$ normalized, and then the result is that the inequality is violated.
  - I wanted to convert this into a setting where only one particular observable is measured, say z (I'm obviously talking about spins, so σ_z). The question then was if I could find suitable unitaries such that the effectively, when I calculate the expectations, I get the same answer. The answer I found is yes (rather trivially if thought of geomtrically).
  - Next I asked if I could make those unitaries act like essentially only adjusting the phases of the state. I figured that can also be done. Instead of measuring z and using $e^{i\sigma_y\theta/2}$ (with various θ yielding essentially various measurement settings) I could measure along say y and apply $e^{i\sigma_z\theta/2}$. This way then, the working of the unitary is infact just the phase shift I can happily do with my continuous variable.
  - Why this was useless? Well, it is deceptively close. It is not actually that simple, because now the simple phase shifts happen if the state is written in the z basis, however I am measuring along the y basis.

- Mathematica
  - Learnt how to use it. [watched 2 video tutorials, ~1 hour each | the tool is really good]
  - Wrote the code to find these stupid expectations quickly (it is not pasting :( )



playing.nb - Wolfram Mathematica 10.0
File Edit Insert Format Cell Graphics Evaluation Palettes Window Help
(*Needs["Quantum`Notation`"]*)
In[159]:= Print["Initial state:"]
stateEntangled = $\frac{q_1\, p_1 + \text{Exp}[\phi]\, q_2\, p_2}{\sqrt{2}}$;
state = $\frac{p_1 + \text{Exp}[\phi]\, p_2}{\sqrt{2}}$
dispRuleP = {$p_{n\_}$ -> p_{n+1}};
orthRule = {$p_{m\_}\, p_{n\_} \to 0$, $q_{m\_}\, q_{n\_} \to 0$};
normRule = {$p_{n\_}\, p_{n\_} \to 1$, $q_{n\_}\, q_{n\_} \to 1$};
conjRule = {Exp[ϕ_] → Exp[-ϕ]};
Print["So the expectation for identity is"]
newState = state ; (*/. dispRuleP*)
Expand[(state /. conjRule) newState]
% /. normRule
% /. orthRule
Print["And the expectation for displacing p is"]
newState = state /. dispRuleP
Expand[(state /. conjRule) newState]
% /. normRule
% /. orthRule
Initial state:
Out[161]= $\frac{p_1 + e^{\phi}\, p_2}{\sqrt{2}}$
So the expectation for identity is
Out[167]= $\frac{p_1^2}{2} + \frac{1}{2} e^{-\phi}\, p_1\, p_2 + \frac{1}{2} e^{\phi}\, p_1\, p_2 + \frac{p_2^2}{2}$
Out[168]= $1 + \frac{1}{2} e^{-\phi}\, p_1\, p_2 + \frac{1}{2} e^{\phi}\, p_1\, p_2$
Out[169]= 1
And the expectation for displacing p is
Out[171]= $\frac{p_2 + e^{\phi}\, p_3}{\sqrt{2}}$

5.5 Friday, June 12

- Mathematica Alternatives [was up till 5 or so, figuring this]
  - Tried to get mathics to work first and then looked at other options
  - Then tried installing sage etc.
  - Tried converting the mathematica code to Sage [took the longest, and worked the worst | there's no simple way I could find for indexing symbolic variables]
    - And for some reason, it would always show $1/\sqrt{2}$ as $\sqrt{2}/2$ which is plain annoying | this also I couldn't figure how to fix
  - Finally, tried to convert the Mathematica code to Mathics and although the code compiles, the result is not correct

```
Print["intial state:"]
state=(Subscript[p,1] + Exp[k] Subscript[p,2]) / Sqrt[2]
dispRuleP = {Subscript[p,n_] -> Subscript[p,n+1]}
orthRule = {Subscript[p,m] Subscript[p,n] -> 0,Subscript[q,m] Subscript[q,n] -> 0}
normRule={Subscript[p,1] Subscript[p,1] -> 1,Subscript[q,n] Subscript[q,n] -> 1}
conjRule = {Exp[k_] -> Exp[-k]}
Print["So the final expectation for identity is"]
newState = state Expand[ (state /. conjRule) newState]
% /. normRule
% /. orthRule
Print["And the expectation for displacing p is"]
```

  - This had helped | http://ask.sagemath.org/question/8181/implicitly-defining-a-sequence-of-variables/
- At the office, made progress with the Mathematica code I had written
  - Basically used that code I wrote yesterday to calculate the CHSH inequality
  - Initially I had made some mistake in evaluating the operator corresponding to $\cos(\alpha) \otimes \cos(\alpha)$ that lead to a happy violation with $n > 3$ or so. This was fixed.
  -

Bell type test with a double slit setup

The setup

- Consider the double slit setup as shown [TODO: add an image]
- In a typical Bell test, we have Alice and Bob who measure along [x,x’] or [z,z’]. However, this can be equivalently stated as: Alice and Bob apply a unitary, given as $U(\theta) = e^{i\sigma_y \theta}$ with setting [θ_1, θ_2] and [ϕ_1, ϕ_2] respectively, while both measure along x.
  NB: A measurement along x for instance, means σ_x is measured.
- In this setup, Alice and Bob, both measure the modular momentum. They can as before, use the different settings. These apply a relative phase.
- The target is to see if in this setup, we can violate the corresponding inequality:
  $E(x[z - z'] + x'[z + z']) \leq 2$, given that $-1 \leq z, z', x, x' \leq 1$
  - So we start with inputting the setting, one from Alice, one from Bob. Call it γ_A, γ_B. Given these, I find the resultant state, which is essentially just $(q_1 p_1 + \mathrm{Exp}[\phi]\, q_2 p_2) / \sqrt{2}$ with ϕ=$\gamma_A + \gamma_B$.
  - To measure x_{mod} I simply measure $e^{i\,\hat{x}\,2\pi/L}$, whose operation is simply to shift the lumps.

The Code

Approach one (only phases, doesn’t violate | as intuitively expected)

This has been matched with analytic results for m=2,3 and the rest seem to be correct also.

```
In[104]:= m  = 50;
ex[α_, β_] := (
    (*Print["Initial state:"];*)
    (*stateEntangled=(q1p1+ Exp[i(α+β)]q2 p2)/√2;*)
    stateEntangled = (Exp[i (α+β)] q1 p1 + Sum [qn pn, {n, 2, m }])/√m;
    (*state=(p1+ Exp[ϕ] p2)/√2;*)
    state = stateEntangled;
    (*Print[state];*)
    (* dispRuleP={pn_->pn+1};*)
```

- Discussion with Ali [made quite some progress relatively]
  - He suggested we could use instead of the $00+11$ type of state, $(0+1)(0+1)+(0-1)(0-1)$ type of state. Although he himself wasn't so sure, it actually got me closer to the picture I had with σ_z rotating the phase in the $0,1$ basis, while the state is expected to measured in the $+-$ basis.
  - The issue however is that we still don't know what is the analogue of σ matrices and how it must be constructed. The displacement operator itself is not the best option.
  - So basically we might be able to link all the 3 ideas, the AB effect (phase shift), the locality issue (Bell test) and finally the relation between pauli matrices and density operators
  - Also started to question the issue related to the meaning of modular position on the screen as opposed to moments after the particle leaves the double slit.
- Seminar [TODO: add link] It was the last in the series, about Bell. Very interesting ending.

5.6 Saturday & Sunday, June 13 & 14

- [*first non-trivial result*] Figured the mapping between the continuous variable and the discrete case
- Misc/Health
  - Gym on Saturday only.
  - Thought: Physics is about finding the most accurate assumptions.

6 June 15-21 | Week 6

6.1 Monday, June 15

- Misc/Seminars
  - On the Nature of Causality in Complex Systems, George F.R. Ellis [https://www.youtube.com/watch?v=nEhTkF3eG8Q]
  - About non-commutative geometry, stopped watching after some time [https://www.youtube.com/watch?v=GKu5NkVPI2w]
  - Bracket notation for lyx [http://tex.stackexchange.com/questions/48031/lyx-quick-way-to-insert-dirac-bra-ket]
  - Thought:
- Otfried's Lecture: About how if one assumes that no information is transmitted alone, the CHSH bound is violated by 4 and not just by $2\sqrt{2}$. There's more to QM, more restriction than is apparent.
- Ali
  - Discussed what I had worked out. It seems correct. I must now do the explicit calculations without assuming $N \to \infty$.
- Continuous variable Bell Test
  - Worked out the explicit details for the usual bell test, with the scheme, rotate the state first, then take the expectation. Found the 'correct' values of θ and ϕ.
  - Worked out the continuous variable case for $n \neq \infty$. Had made an assumption which I couldn't support in one of the proofs. Recalculated without that assumption and obtained the same result.
  - Started documenting the same
- Walking back
  - Figured that the phase shift operator must be a function of modular position ($x \text{mod} 2L$) and that σ_z must correspond to this somehow
- Health etc.
  - Gym

6.2 Tuesday, June 16

- Roope's Lecture
  - I couldn't follow much today. Need to read from the text
- Continuous Variable Bell Test
  - Ali told me about the more elegant representation he'd found for his contextuality paper with the set $D(\alpha_i)$ forming the pauli matrices. He showed that the commutation is obeyed if α_i satisfy a constraint. The task was to use this formalism and arrive at the violation.
  - To that end, I started looking for states that correspond to $|\pm\rangle$ etc. and realized after an entire day of thinking, that it is not straight forward, and perhaps, if one of my assumptions isn't false (that there're only two eigenkets of $D(\alpha)$ upto phase/scaling ofcourse) then these states don't span, well anything. You can't even construct eigenkets of $D(\alpha_2)$ from those of $D(\alpha_1)$.
  - I had spent the whole day thinking about things based on the wrong assumption that displacement operators only displace. They infact, also add a phase which prevents say $\sum_n |n\alpha + \beta\rangle$ from being an eigenstate of $D(\alpha)$ for $\beta \neq 0$.
  - My usual scheme of using $|0/1\rangle$ states with $2L$ separated lumps also doesn't work if the lumps are made of coherent states. Other than it not being an eigenstate of neither $D(\alpha_1)$ nor $D(\alpha_2)$, they are not even orthogonal. I had infact imagined that states of the form $\sum_{m,n} |n\alpha_1 + m\alpha_2\rangle$ are simultaneous eigenkets of $D(\alpha_1)$ and $D(\alpha_2)$ while these operators don't even commute. Now I am in a situation where I can't even construct eigenkets general enough to span another eigenket!
  - Other than this, I added the operator for the phases, viz. σ_z's analogue in the continuous variable case.
- Health
  - Headache :(

6.3 Wednesday, June 17

- Appropriate Eigenkets of the displacement like pauli operators
  - Thought along the lines of displacing states if there existed a number operator like object for the coherent state, namely $\hat{O}|\alpha\rangle = \alpha|\alpha\rangle$, then I could use this to construct something like $D(\lambda)e^{-i\mathrm{Im}[(\lambda\hat{O})]}|\bar{\alpha}\rangle \propto |\bar{\alpha}\rangle$ where $|\bar{\alpha}\rangle = \sum_n |n\alpha\rangle$
  - The other approach I had thought of was if there was some operator like $\hat{S}_\alpha = \sum_n |(n+1)\alpha\rangle\langle n\alpha|$ with α sufficiently large so that $\langle n\alpha|m\alpha\rangle \approx \delta_{nm}$. So what I intended to do was something like $(1 - \hat{S}_{\alpha+\lambda})D(\lambda)|\bar{\alpha}\rangle \propto |\bar{\alpha}\rangle$ which for sure won't work because you would need something like $\hat{S}_{n\alpha+\lambda}$ for each term in the summation which defeats the purpose of the operator itself.
  - I tried to construct pauli operators using $J_k = \epsilon_{ijk}p_i x_j$ but then that is anyway the commutation for angular momentum. Then I realized that I've also gone from 1 dimension to 3 dimension space.
- Guest Lecture on Bohmian Mechanics by Detlef Duerr from Munich on "What is and to which end does one study Bohmian Mechanics?"
- Couldn't make much progress with finding eigenstates of the displacement like pauli operators. Kind of proved that they don't exist.
- Ali gave me a paper that does a GHZ test using continuous variables and modular variables. This seemed interesting because they had obtained the pauli like operators with the only difference being that they hadn't considered $\cos(p\alpha)$; instead they had $e^{ip\alpha}$ like operators but rather elegantly used.
- Health:
  - Not feeling well, no gym

6.4 Thursday and Friday, June 18 and 19 [unwell :(]

6.5 Saturday and Sunday, June 20 and 21 [catch up]

- Saturday
  - Worked on completing the report (couldn't do the full thing because the proofs were in the university and even though I went, I couldn't enter; apparently you need some sort of a chip to enter)
  - Started re-reading the GHZ paper and understood where the $mod2$ was coming from.
  - Took the hint from the state they had used and tried to work it out independently. Didn't work
  - Peculiar point was that they had taken a state that looked similar to the simultaneous eigenket of $\bar{x}$, $\bar{p}$
  - Seminar/Eating time: Watched Kardar's lecture # 1 or 2
  - Protocols: Pending documentation etc.
  - Health: Not fully normal, no gym, about 6 hours throughput
- Sunday
  - Read the GHZ paper further, realized that they were using more fancy things than just modular operators. They were using some kind of binary operators. Idea was that eventually, they'll construct an operator $[\tilde{x}]_k$ which would essentially yield the kth bit of x when acted upon $|x\rangle$. They then exponentiate such operators and define X, Y and $Z = iXY$ (I actually never realized that you could easily define $\sigma_z \propto i\sigma_x\sigma_y$ because $[\sigma_x, \sigma_y] = 2i\sigma_z$ while $\{\sigma_x, \sigma_y\} = 0$. The point is that when we ask for x_{modL} say, then we extract infinite bits, but they're not really needed. For instance my $Z(x_{mod2L})$ operator essentially only needs to know if $x_{mod} < L$ or not. So my Z operator is infact somehow only using 1 bit of information. This is perhaps the right direction towards an elegant generalization.
  - I found that $X|\uparrow\rangle = |\downarrow\rangle$ works even when $X' \equiv \left(X + X^\dagger\right)/2$, viz. $X'|\uparrow\rangle = |\downarrow\rangle$. To check if Y and Z also work,
  - I need to figure how to convert (the old problem which I didn't solve then because it seemed like a digression) between position ket representation and momentum ket representation (I mean I know how, but I just don't get the right result), since I can't use the displacement action as I could with usual modular variables.
    OR
    I could just find how the binary operators act on $|p'\rangle$ instead of $|q'\rangle$
  - Seminar/Eating time: Watched Kardar's lecture # 2
  - Protocols: Pending emails etc.
  - Health: Not fully normal, no gym, about 6 hours throughput

7 June 22-28 | Week 7

7.1 Monday, June 22

- Protocol:
  - Organized documents (physical)
  - Updated documentation (was lagging)
- Completed the article on continuous variable bell test (the proofs were pending, proof read them partially!)
  - Emailed it to Ali
- The GHZ paper
  - Not able to make much progress | things I couldn't derive, I still can't derive
    - I have issues with taking log of product of exponentiated operators and the exponentiation of objects like $(a + b + c)mod2$
    - I am unable get the right answer for converting something from position basis to momentum basis. How sad is that :(
  - I was however able to figure out
    - How $Y|\uparrow\rangle \propto i|\downarrow\rangle$, but I just can't get the sign right. This is very important if I wish to show that $\frac{Y+Y^\dagger}{2}$ yields the same result as Y; this in turn is essential for the Bell test.

- Discussion with Ali
  - About the article I wrote
    - He seemed to convinced that everything is correct
    - was glad to see the similarity between the GHZ approach and our approach
    - Suggested I calculate the Wigner function etc. for the operators $U^\dagger XU$ and show they are not unbounded like the parity operator. Oh, I now understand what he meant. Instead of the density matrix, I just put my operator and evaluate its Wigner function. So my MATLAB code for finding wigner function wasn't really a waste afterall eigh :P
  - About his new proposal
    - So his idea was to use the first order coherence function and construct a Bell inequality using it.
    - It is interesting he said because first order coherence doesn't detect quantum effects. For that, one must use second order coherence. So if we can show the bell test using first order coherence, then we would've captured the effect somehow.
- Seminar/Eating time: Watched Kardar's lecture # 3

7.2 Tuesday, June 23

- Protocol:
  - Updated documentation
- The GHZ paper
  - Fixed the sign, $Y\left|\uparrow\right\rangle = i\left|\downarrow\right\rangle$ indeed.
  - The issue is that the states are
    - Expressed as a countable superposition of $|x\rangle$ (or equivalently$|p\rangle$) states, thus not very 'nice'
    - Even if we express it as a superposition of $|x\rangle$, there're infinite of them, therefore the state is not normalizable
  - However, nothing stops us from considering the Bell states with this formalism, because $X^\dagger$ and $Y^\dagger$ act the same way on $\left|\uparrow\right\rangle$ and $\left|\downarrow\right\rangle$ and therefore one can readily construct $\cos(\tilde{p})$ and $\cos(\tilde{x})$ and construct the inequality and show a violation by an identical analysis.
- I must now show that my $U^\dagger(\theta)XU(\theta)$ are not unbounded in their wigner representation (and they might be, who knows).
  - I have a feeling that it might just be. Justification:
    - The operator that sends $|q\rangle \to |-q\rangle$, Parity, is a delta function in the Wigner representation (we proved the other way round in the Topics in QM, CM course).
    - The $Z(x_{mod2L})$ operator also essentially does something similar, (for $0 < q < 2L$) $Z|q\rangle = \pm|q\rangle$, so for certain values, $|q\rangle \to -|q\rangle$.
    - Ok now it doesn't seem all the compelling and its worth checking.
  - It turns out that it is almost trivial to evaluate this also. The answer is $W(q,p) \sim e^{iZ(q-L/2)\phi}e^{ipL/\hbar}$ upto some signs. So it is well behaved and bounded.
  - Surprisingly though, if I don't use the anti-commutation, I get $W(q,p) \sim e^{iZ(q-L/2)\phi/2}e^{ipL/\hbar}e^{iZ(q+L/2)\phi/2}$ which is obviously not the same as the previous statement. I'll stick to this because to derive the commutation, I had to make some assumptions which I'm not so sure hold.
- Discussion with Ali
  - Actually it was while explaining the result to Ali that I found $W(q,p)$ exactly for $U^\dagger(\theta)XU(\theta)$. I must write this out neatly for the
  - Explained the difference between the different types of AB effects and the related photon AB effects
  - The next big question now is to understand how to extract the modular variable information off of the screen. This reminds me of the 3 pinhole paper we had read for Makundu's course.
  - Afterwards, he asked me to explicitly show that in my setting $[a_1, a_2] \neq 0$ (same for b_i). He basically wants me to explicitly show that the cause for the violation is the commutation explicitly, by showing $B^2 = 4\mathbb{I} + [A, A'] \otimes [B, B']$.
- Extracting Modular Variable information off of the screen: What are the various approaches one could take?

- So I could start with field quantization for the E, B fields. In that case, the E field is a function of q and the B field is a function of p. However, this is when they quantize in a box. When they quantize in general, then they have $q + ip = A$ and $q - ip = A^*$ and then it takes more reading to understand how they all relate to E, the observable.
  * I also just started thinking, agreed that the Hamiltonian is the same as that for a harmonic oscillator. That doesn't mean that the time evolution of q, p should be given by the Heisenberg equation of motion, or does it? Well if the Hamiltonian is correct, then given the right coordinates (q and p as we have), it should give the right evolution equations when plugged into the Hamilton's equations. So yes, in general also, one writes a Hamiltonian first which gives the right dynamics and then quantizes the conjugate variables. The usual trick. (I wonder at times if you could do anything other than the Lagrangian or the Hamiltonian, but not now anyway). Mandy had explained the rest quite well, about what picture is associated with the quantum states and where it's happening in the physical space.
- The other approach is that I take a wavefunction as I had, and then just evolve it according to a free Hamiltonian and from the interference obtained (from $|\langle x|\psi\rangle|^2$) I can explore how to extract p_{mod2L}.
  * I remember now the contradiction. The claim was that $\psi(t)^* p^k q^m \psi(t)$ doesn't depend on ϕ for any t, if $|\psi\rangle = |\psi_1\rangle + e^{i\phi}|\psi_2\rangle$ s.t. $|\psi_1\rangle$ and $|\psi_2\rangle$ don't overlap to start with, but $|\psi\rangle$ is freely evolved, viz. $H = p^2/2m$. The contradiction is based on intuition. It is known that under free evolution, the wavefunction diffuses. So the two lumps should eventually overlap. If they do overlap, I expect ...
- Allright, after all this mumbo jumbo and confusion, I just realized that I must not try to find the value of ϕ, θ and take them as given. The point is that I should just choose between ϕ_1, ϕ_2 (say) and then measure X somehow. This must yield a value between $-1 \le X \le 1$. Now I share my values with Alice and we plug them in the Bell's inequality and let the experiments say whether or not there's a violation. The question really now is how to find the measurement outcome of X. I got all confused. So for any observable, the axiom is that the outcome must be one of the eigenvalues, corresponding to the eigenket to which the state is collapsed. The issue is that my eigenkets are not really complete, or atleast not the 'subspace' (subset, fine) I am considering. $|\pm\rangle$ are not really eigenkets and that's why all the hassle. If they were, then the values of X would be ± 1. But here that's not the case; N must come into the picture somehow. Is it possible to have observables who's eigenkets aren't complete? That way I have difficulty figuring how to measure even theoretically. Maybe the notion of measurement can be generalized in some way here (I must get the Ns as probabilities somehow).
- On the way back, I figured that if I use unnormalized eigenkets of X such as $|\tilde{x}\rangle \equiv \sum_n |x = \tilde{x} + nL\rangle$, then $\int_0^L d\tilde{x}\, |\tilde{x}\rangle\langle\tilde{x}| = 1$ because $\int_0^L d\tilde{x}\, |\tilde{x}\rangle\langle\tilde{x}|\,|x\rangle = |x\rangle, \forall x \in \mathbb{R}$. However, only now I realized that such states would give $X|\tilde{x}\rangle = |\tilde{x}\rangle$, which means the measurement will always yield 1. What about the $|\tilde{x}'\rangle \equiv \sum_n (-1)^n |x = \tilde{x} + nL\rangle$ type of states? These are the ones that were supposed to yield $X|\tilde{x}'\rangle = -|\tilde{x}'\rangle$ resulting in a -1 outcome.
- I also tried proving the equivalence of the two representations of eigenstates of X but failed.
- After reaching home, I thought perhaps we can use some other system. Maybe we can swap $x \leftrightarrow p$ so that in position space, there's nothing fancy, but in the momentum space we have about 7 frequencies overlapped. Then perhaps we can simply measure X which will be defined in terms of the position operator now, from the screen, since I can simply measure $\hat{x}$ and record only its mod (or can I?).

- Seminar/Misc:
  - Kardar lecture # 3
  - Thought: When the motivation is not o, then exactly that becomes your strength, your value; people always want what money can't buy. This is what makes honour such a great word.

7.3 Wednesday, June 24

- Health:
  - 8 hours wasn't enough, need about 9 hours of sleep (without coffee that is), with coffee, I was able to pull off with 6 hours of sleep, 12 hours of work, 1 hour of gym, cooking and everything else perfected. Ofcourse, the system broke down towards the end for two days, that's a disaster, but that too was well planned (absorbed as vacations) and effectively lost only a few hours, assuming usual efficiency days.
  - No gym
- Paradox?: So the measurement axiom seems to suggest we express our state in terms of eigenstates of the operator corresponding to the measurement. One way to do that is to construct an identity operator using these eignkets and then simply multiply it with the state in question. The result would be a superposition of eigenkets as we wanted. Then the amplitudes will tell us with what probability we get which eigenket, thus which eigenvalue as the measurement outcome. Nothing new or subtle yet. But here's the issue. Consider the operator X as defined yesterday. Now if it is true, as has

been shown, that $\int_0^L d\tilde{x} |\tilde{x}\rangle \langle\tilde{x}| = 1$, and that $X|\tilde{x}\rangle = |\tilde{x}\rangle$, then it seems that the only outcome of measurement of X on any arbitrary state, must result in the value $+1$. However, we know $\exists$ states s.t. $X|\tilde{x}'\rangle = -|\tilde{x}'\rangle$ where $|\tilde{x}'\rangle$ has been explicitly defined earlier. It's measurement must yield a -1. So what's wrong?

- The obvious thing to try would be to see what we get when we do $1.|\tilde{x}'\rangle = \int_0^L d\tilde{y} |\tilde{y}\rangle \langle\tilde{y}| |\tilde{x}'\rangle$. If one uses the fact that $\langle\tilde{y}|\tilde{x}'\rangle = 0$ (well this itself is a little tricky, works well only if you define it as a partial sum first and then take the limits), then apparently $1.|\tilde{x}'\rangle = 0$ which is bizzare. And if one uses the expansion of $|\tilde{x}'\rangle$ then by the action of 1 on $|x\rangle$, we get $1.|\tilde{x}'\rangle = |\tilde{x}'\rangle$ as expected, but then (fine i'll have to do the calculation to find what is the issue in that case) the expansion we get is something like this $\sum_m (-1)^m |x = \tilde{x} + mL\rangle = |\tilde{x}'\rangle$. Here's the important step. $1.|\tilde{x}'\rangle = \sum_{n,m} \int d\tilde{y} \langle x = \tilde{y} + mL | x = \tilde{x} + nL\rangle |x = \tilde{y} + mL\rangle$. If I don't expand out $|\tilde{y}\rangle$, then the only operation I can perform to proceed is $\langle\tilde{y}|\tilde{x}'\rangle$ which is unambiguously zero for $\tilde{x} \neq \tilde{y}$. So then the only question that remains is $\langle\tilde{x}|\tilde{x}'\rangle$, which we said is zero only when treated as a partial sum.
  - ~~This maybe rationalized by imagining that $1.|\tilde{x}\rangle = \sum_m |\tilde{x}\rangle$ which is basically just a consequence of the fact that $|\tilde{x}\rangle$ is not normalized to even start with!~~
- Thought: It maybe more helpful to think of measuring the modular variable as extracting only the modular part, instead of thinking of it as displacements etc.
  - Basically, even $|p\rangle$s are eigenkets of X. $X|p\rangle = \cos(pL/\hbar)|p\rangle$. $|p\rangle$ are also complete and well behaved relatively anyway. So how must then I use this? [I must eventually figure out the equivalence between these but first I must get some reasonable answers]. Infact I am not even sure which set of eigenstates should one use for such purposes. In anycase, proceeding with this, let's see what we get. Looks like I need to know for sure exactly how to convert between position and momentum space.
- As it turns out, there's a paper titled 'Nonlocal Young Tests with EPR correlated particles' which states "The position measurements at the detection screens may thus be viewed as effective momentum measurements on $|\Psi_T'\rangle$". This was again after a discussion with Ali

• Discussion with Ali

- I explained to him the issues, well the paradox stated above (there are mistakes in the results there, scaling). Obviously things looked spooky. I told him why I thought that a measurement on the screen shouldn't work ($[x, p_{mod}] \neq 0$) and was trying to construct the right set of eigenstates etc. He however he insisted I must extract this information off of the screen.
- He explained the idea of how he used coupling with spin to evaluate $\langle\sigma\otimes\sigma\rangle = \frac{1}{4}\langle D\otimes D\rangle$ as a way of measuring the correlated expectation value of the system (in his contextuality paper). Point being twofold (he didn't say it explicitly like this, but he has some crazy good intuition)
  - Recall that for $\psi = \psi_1 + e^{i\phi}\psi_2$, when you evaluate $\psi^*\psi$ it doesn't depend on ϕ if observed when ψ_1 and ψ_2 don't overlap. However the modular momentum does at that time. This ϕ dependence re-emerges when ψ_i overlap finally. The point is that the information you thought you can't get from the screen, you can. Somehow the modular momentum information is contained in the position measurement.
  - Next, note that for a product state like $|\psi\rangle = |\psi_1\rangle|\psi_1\rangle + e^{i\phi}|\psi_2\rangle|\psi_2\rangle$, you won't see interference on the screen of either observers. This is because you're left with a mixed state, after you trace out the other system. $\rho = |\psi_1\rangle\langle\psi_1| + |\psi_2\rangle\langle\psi_2|$. If you however calculate $P(x,y) = \psi(x,y)^*\psi(x,y)$, you'll see the ϕ dependence re-emerges, depending on again, the overlap condition. So basically, the modular variable expectation you evaluated, which depended on this ϕ, viz. $\left\langle X\otimes U^\dagger X U\right\rangle$ should be in some form present in the (x,y) correlation. This is what you have to extract.
  - Remark: It maybe mentioned that it is important to get the scaling factors right between this expectation and expectation of the modular variables, because that is crucial for the violation, if we intent to measure the modular variables indirectly.
- He again showed me the Non local young experiment with the EPR state. That actually has implemented recovery of modular variables in some way which is worth looking at.
- He also enquired if I evaluated the commutation: $[U_1^\dagger X U_1, U_2^\dagger X U_2]$, which I should and add to the report. Tomorrow.

• Seminar/Misc:

- Kardar lecture # 4
- Thought: I really want to construct a test that tests for dynamical non locality instead of the usual non-locality (co-relations)
- After this, I might just end up asking if Mandy will be willing to guide me. He is insanely hard working and it might just be worth it.

7.4 Thursday, June 25

- Health:
  - No gym as per new schedule (only Fridays, Saturdays, Sundays and Mondays)
  - Slightly tired despite about 8.5 hours of sleep | but feels optimal, even without coffee
- Group Meeting:
  - I was asked to speak today, did; couldn't quite reach the part which I had worked on. Just discussed
    - the motivation with the displacement operator etc.
      - ϕ dependence
      - dynamic non locality
    - the AB effect setup;
      - Electric AB in some detail
      - talked about how ϕ can be introduced
      - talked about the photon AB effect
  - next week barbecue details will be decided :P
- Nonlocal Young with EPR paper
  - reading and filling in the missing steps, it's getting more an more interesting because it seems to come dangerously close to the Bell test but doesn't mention it
  - it also seems like their EPR state can be used to somehow create the kind of states I wanted
  - I had apparently gotten too excited about how close the EPR paper is to my work, it isn't quite so. [thought about it at night]
- Physical Colloqium and Seminar
  - A master's student talked about some kind of trap that he'd created for his thesis.
  - The seminar was about unifying fundamental forces (the guy didn't include gravity)
- Eating/Seminar/Misc
  - Kardar Lecture # 4 completed

7.5 Friday, June 26

- Health/Protocol
  - Documented
  - Feeling good with 8 hours of sleep. Scheduled gym today :)
  - Added banana to mid breakfast/lunch meal | I can perhaps eat some more in this time
- Nonlocal Young with EPR paper
  - Read till section II and glanced through the rest. The relevant part has been covered. The following took the longest to derive, although it is quite trivial.
  - [*figured how to implement measurement*] So I finally figured out exactly how one can measure the modular momentum off of the screen. The essential idea is that you keep the screen far enough so that the free evolution can be approximated by a simple fourier transform. Consequence is that if the experimentally obtained probability $P_{\text{screen}}(x_1, x_2)\left|\psi_{\text{screen}}(x_1, x_2)\right|^2 \propto \left|\varphi_{\text{grating}}(p_1, p_2)\right|^2$ where $p_i \propto x_i$ and the constant depends on masses and the time for which it is evolved. In any case, with that information, one simply needs to plugin the value of the modular variable and we're done for the evening. Besides, one can just take the modular screen position and that itself should suffice.
    Even though this is not a new/novel result, the point is that it was needed.
  - With that big piece in place, it only remains to figure out how to construct the $|\pm\rangle$ entangled state from the EPR state. To that end, it was found that after the simplification, the state in the Young's paper and our paper are very distinct. Need to figure out if it is even possible to do this.

- Eating/Seminar
  - Kardar Lecture # 5 (probability + zeroth law)
- Misc/Computer
  - Latex/Mark down plugin for gmail