projectSiegen | Summary

29th May 2015

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. Apprently 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

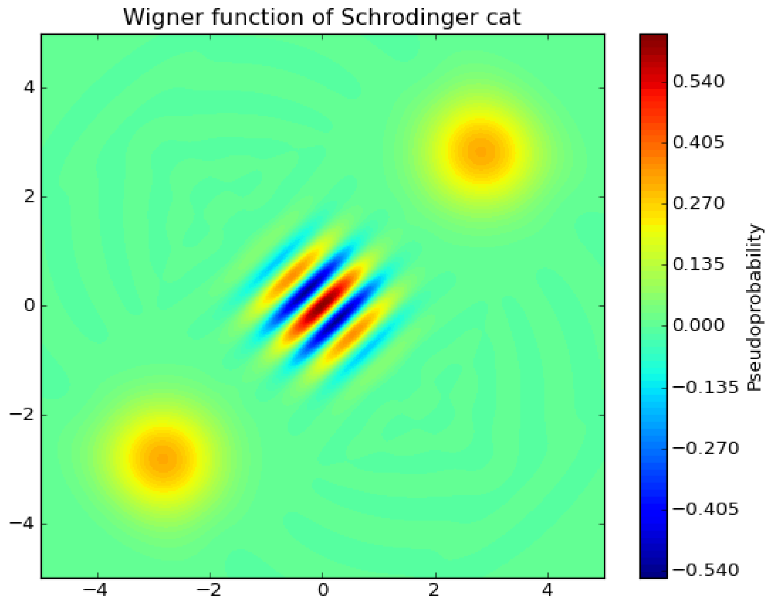
- I attempted getting an expression for the wigner function for the state $|\alpha\rangle$ and what I finally obtained, didn't seem to be real.

  - 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 $p(x|E,\rho) = \mathrm{tr}(\hat{E}_x \rho)$
  - 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)

3 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.

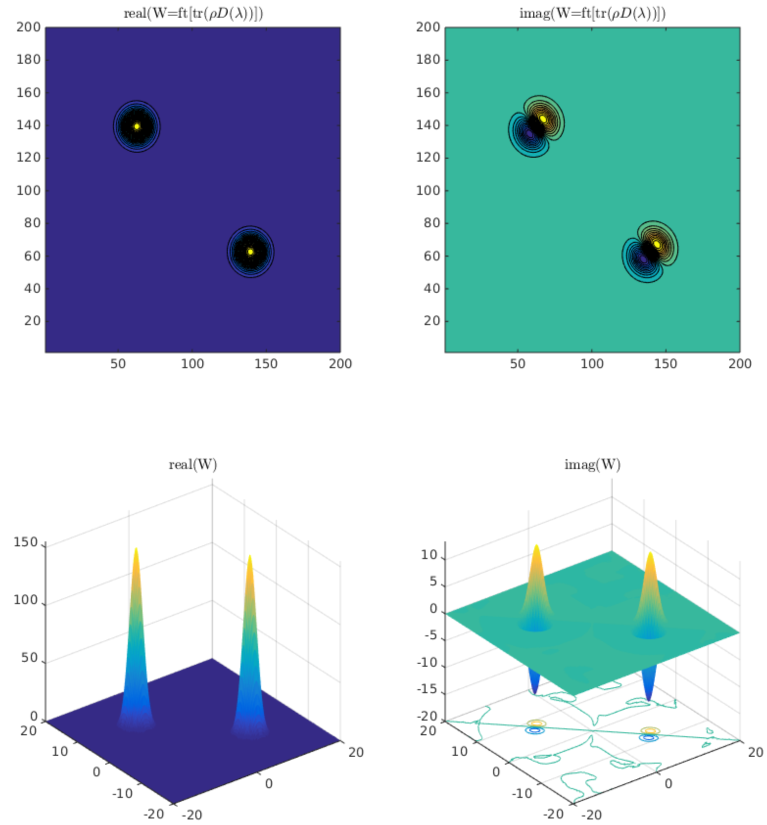
4 May 25-29 [Week 3]

4.1 Monday, May 25

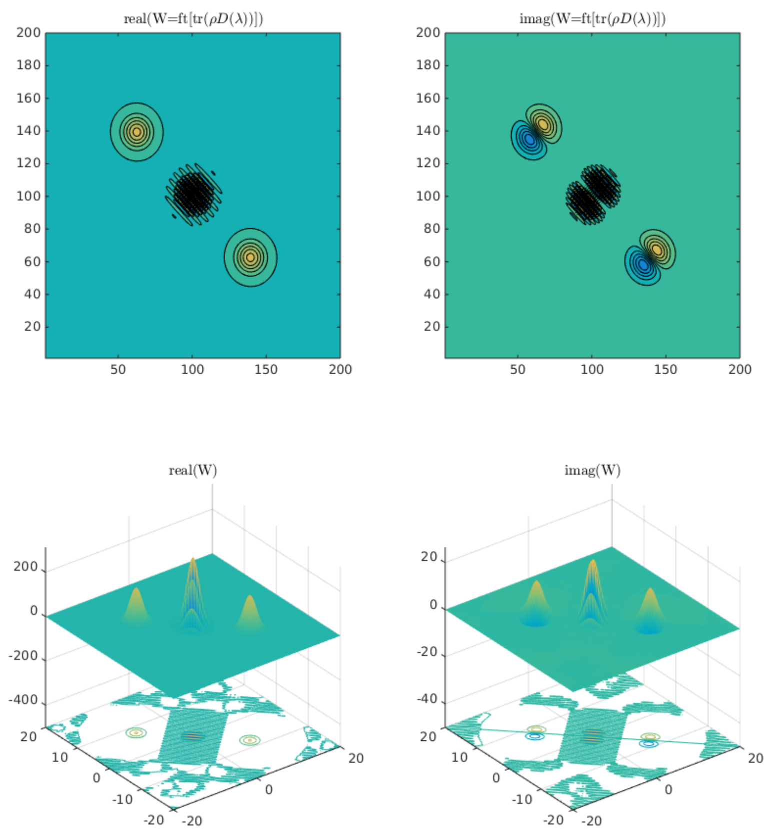
- Essentially spent time reading the NJP paper by Aharnov et al
    - It starts by building on the idea of what changes when we do a which way measurement (WWM), in the context of a double slit experiment
        - Talks about debates over momentum changes before and after, claims some results about 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.

4.2 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)

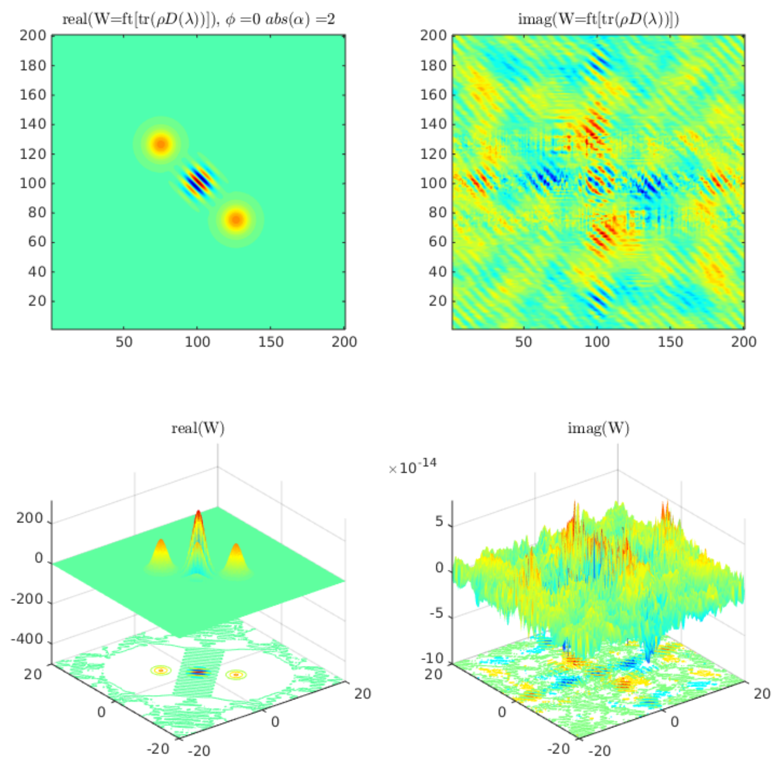
- The issue was essentially that I had messed up one step. I failed to put the extra phase when I evaluated $D(\lambda)|\alpha\rangle \neq |\lambda+\alpha\rangle$. There's a phase which I had missed.
- Here's the working code

```
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

4.3 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. $\nexists a_n\ s.t.\ f(x) = a_0 + a_1(x-x_0) + a_2(x-x_0)^2 + \ldots$ 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 $e^{\frac{i\hat{p}L}{\hbar}}\psi(x) = \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \ldots\right]\psi(x) = \frac{i}{\hbar}\sum\frac{\psi^n(x)L^n}{n!} = \psi(x+L)$, 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: $e^{\frac{i\hat{p}L}{\hbar}} = \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \ldots\right]$.
- 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, $e^{\frac{i\hat{p}L}{\hbar}} \neq \left[1 + \frac{i\hat{p}L}{\hbar} + \frac{1}{2!}\left(\frac{i\hat{p}L}{\hbar}\right)^2 + \ldots\right]$, 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)

4.4 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 $O(0) = \cos\left[1/\hbar p_x(0)L + 2k_0 x(0)\right]$, 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! :)

4.5 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.

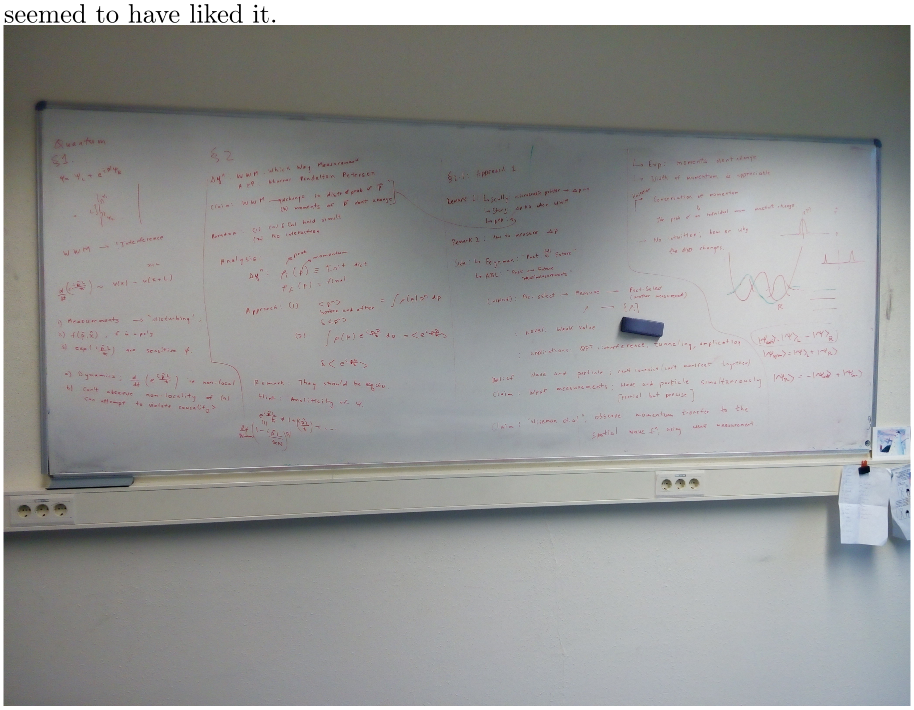
- AB effect for Photons: I had this idea yesterday and today I started looking for prior work on the field. Here's what I found.
  - I realized that I had read some discussion on AB effect in Ryder. This wasn't although directly related to what I was looking for, but was interesting nonethless. This 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
  - 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 L and electron delocalized with L.