MS Thesis | Summary

Atul Singh Arora

20th May 2016

Contents

Introduction

The purpose of this document is to be evidence of and support the steps taken in the direction of innovating for the master's thesis.

Part I
Perfunctory Systematics of Research | Semester 9

1 Reg, Topic Decision, Multi-Mode Paper | August 3-8, 2015

Motivation: Construct a Bell like test for the case where the system is not two level, but d-level.

Setup: Consider a 4 mode light. One direction is $\mathbf{k}$ and the other is $\mathbf{k}'$. The annihilation operators are $a_1 = a_k^{\parallel}$ and $a_2 = a_k^{\perp}$ and similarly a_3 and a_4 are defined.
Next, we also allow some photon number conserving transformations. One possibility would be $a \to Ua$, where

Recall: Single mode light: Take a plane wave like solution for a cavity and quantize it to get quantum optics. In this you'll get a and $a^\dagger$ corresponding to the k and direction of E (or B) of this classical solution.
N mode light: You allow arbitrary plane waves. You'll get $a_k, a_k^\dagger$ but for a given direction, you can have two polarizations (which are enough to generate all polarizations)

2 Ideas, Bohmian Mechanics started | August 10-15

- [GRE] Subject GRE, vocab GRE and quant GRE were done in full swing
- [Research/ms] Made a presentation about Arvind sir's paper, discussed about uncertainty in speed of light with Bhati being the chief guest, started reading Bohmian Mechanics from Holland, thought about Bohmian Mechanics and how to make it relativistic; (figured that there must be non locality in built as it is, because even in QFT this nonlocality doesn't disappear. However, in
- [protocol f]

3 arXiv and Hamilton Jacobi for Bohmian | August 17-22

3.1 Monday | Aug 17

- [official] Summer project form
  - had to get the certificate from Ali/Otfried
  - had to get Arvind sir's signatures
- [research/summer] Started proof reading the paper
  - Had issues with the discussion section; definition $X = E_+ - E_-$ having only two values as outcomes :(

3.2 Tuesday | Aug 18

- [research/ms] Met with Arvind sir
  - Kichoo finalized his project. He's not doing Bohmian Mechanics
  - I finalized my project (as I had earlier) to Bohmian Mechanics and Contextuality
- [research/summer] Read Haridichi's paper about measuring a bounded observable using a two level system
  - Implicitly it had used results from POVMs; so had to read about POVMs from Nielsen's book
  - Couldn't still fully understand what Ali was doing, I figured that $A_i^\dagger A_i$ type of operators if they satisfy $\sum A_i^\dagger A_i = \mathbb{I}$, then one can talk about probability of getting the i^{th} input as $\left\langle A_i^\dagger A_i \right\rangle$. Ali was using $E_\pm$ and the fact that $E_+ + E_- = \mathbb{I}$ as his scheme. However, for projectors, since $P^2 = P$ and if $P^\dagger = P$, then the condition stated earlier essentially becomes $\sum P_i = \mathbb{I}$ and that makes sense. However, they aren't projectors. So I wasn't sure what was happening.
- [gre] Vocab and Quant

3.3 Wednesday | Aug 19

- [research/ms] nill :'(
- [research/summer] Put the paper on arXiv
  - Ali updated the minor modifications slightly (I still wasn't comfortable with $A^{two} = E_+ - E_-$ having only two possible values) | wasn't very happy intially but figured it today that this statement is not quite needed and had sent the update to Ali, who while made minor changes, wasn't too happy :(
  - Putting it on arxiv required some debugging etc. | moral of the story is to use PDF as images instead and not using hyperref defiend explicitily
  - Ali was happy by the end of it
- [research/official] Presentation of work at the QCQI group meet
  - Went reasonably well
- [GRE] words + little bit of quant

3.4 Thursday | Aug 20

- [official] Registration/course add/drop etc. taken care of
- [research/] The $E_\pm$ issue was understood in more detail after talking to Arvind sir
- [research/ms] Read some more things from Holland
  - Looked at the section on propogation fo the S function
  - started reading the section on Classical Statistical Mechanics
    * He talks about how in CStat Mech, we use the function $f(x, p, t)$ to describe a probability, whereas here we use the less general density $\rho(x, t)$ where the p has been specified.
    * Derives the continuity equation (by demanding the the particles be conserved)
    * He talks about some special cases; viz. specific solutions making some assumptions about the dynamics
- [GRE] words/vocab, reading section from Manhattan.
- [protocol f] Earthlings

3.5 Friday | Aug 21

- [official] KVPY report printed, printed the form again, submitted it to the dean's office
  - Went to the bank for NET
- [research/ms] Holland
  - Finally figured why $\frac{d}{dt}d\Omega = (\nabla.v)d\Omega$; just looked at Aris and it was right there. The idea was to use the jacobian to describe the change in volume and then everything follows.
  - Tried to look up papers related to Bohmian Mechanics and Contextuality (couldn't find too many papers related to this)
    * In Quantum Physics without Quantum Philosophy, 3.8.3 talks about contextuality (looks very wordy)
    * In Bohmian Mechanics and Quantum Theory, page 67 is a chapter on contextuality
  - Can I think of a way of constructing a theory that's local, but not real? Can reality then be emergent? Does it mean that the moment I say I don't assume reality, then it must mean that my theory must have some sort of measurement and then it is essentially a combination of worst of both worlds?
  - Found the following interesting overview of Bohmian Mechanics: http://philsci-archive.pitt.edu/3026/1/bohm.pdf
- [GRE] words/vocab, quant (finished the geometry section)

3.6 Saturday | Aug 22

- [official] Some silly German Research Opportunity thing
- [research/ms] Why must contextuality only be talked about in the context of spins? What about phase space contextuality? Reading Holland; following is a summary.

1. To understand the continuity equation $\rho + \nabla.(\rho v) = 0$, examples of v are taken
   (a) $v = v(x)$, a solution is obtained as
   $$\rho(x,t) = \frac{1}{v(x)} v\left[x\left(t - \int \frac{dx}{v}\right)\right]\rho_0\left[x\left(t - \int \frac{dx}{v}\right)\right]$$
   in which further assuming that $\rho(x)$ results in $\rho = A/\left|v(x)\right|$
      i. $v = v(t)$ then we get
      $$\rho(x,t) = \rho_0\left(x - \int vdt\right)$$
      which means that ρ is constant along particle trajectories
   (b) connection with Liuoville's equation
      i. $f(x,p,t)$ is defined instead of $\rho(x,t)$. Pure and mixed states are defined accordingly as $f(x,p,t) = \rho(x,t)\delta(p - \nabla S(x,t))$ being pure and the remaining as mixed.
      ii. $\frac{df}{dt} = \partial_t f + \frac{1}{m}\sum p_i \partial_{x_i} f - \sum \partial_{x_i} V \partial_{p_i} f = 0$ is the Liuoville's equation (which holds since we can show that the volume doesn't change under Hamiltonian evolution and particles inside the volume stay inside; $f(p', q', t+\delta t) = f(p,q,t)$ is essentially the statement $\frac{df}{dt} = 0$) which is linear in f.
      iii. One may project out the moment space. They define equivalent of ρ as $P(x) = \int f d^3p$, mean momentum as $\overline{p_i(x)} = \frac{\int p_i f d^3p}{P(x)}$ and $\overline{p_i p_j}(x) = \frac{\int p_i p_j f d^3p}{P(x)}$. The louvielle equation can then be expressed in terms of these spatial variables. Integrating it we get
      $$\partial_t P + \frac{1}{m}\sum \partial_{x_i}(P\overline{p_i}) = 0.$$
      To get the momentum transport equation, after multiplying the louvielle equation with p_i and integrating, we get
      $$\partial_t(P\overline{p_i}) + \frac{1}{m}\sum \partial_{x_j}(P\overline{p_i p_j}) + P\partial_{x_i}V = 0$$
      (apparently integrated by parts and assumed $f \to 0$ as $p_i \to \infty$)
      While f is constant along a phase space trajectory, the spatial density P (equivalent of ρ) is not. It's apparent from the derivation of the continuity equation; either we start with a fixed volume or a fixed number of particles, not both.

If you substitute $f = \rho\delta(p - \nabla S)$ as stated earlier, you'd get $P = \rho$, $\overline{p_i} = \partial_{x_i} S$, $\overline{p_i p_j} = \partial_{x_i} S \partial_{x_i} S$ as expected. The substitution also yields what's called a field theoretic version of Newton's Laws given by

$$\partial_t \rho + \frac{1}{m}\nabla.(\rho\nabla S) = 0$$

and

$$\left[\partial_t + \frac{1}{m}\sum \partial_{x_i} S \partial_{x_j}\right] \partial_{x_i} S = 0$$

iv. Remarks:

A. It's not obvious that if we start with a state that has well defined momentum (delta distribution) but the positions are given by $\rho(x)$, then they will continue to be well defined in momentum. This happens only exceptionally. In general, a pure state maybe sent to a mixed state. We'll see examples of these. [todo: ensure examples make sense]

B. Can we decompose any mixed ensamble into a linear combination of pure ones? The answer's no. [proof?] Say there are many solutions of the Hamilton-Jacobi equation, given by S_i. Thus, we can construct a linear combination as $f(x, p, t) = \sum P_i \rho_i(x, t)\delta(p - \nabla S_i(x, t))$ where P_i (degenerate notation) refers to the distribution of momenta at a given point. $\sum P_i = 1$ is assumed for normalization. Claim is that this is not in general possible to decompose a state into this form. An explicit example is that of reflecting through a potential barrier (in CM) [todo: ensure the example works]

C. While this is not particularly useful in CM (the pure and mixed states), the formalism helps in comparison with QM.

(c) Pure and Mixed States

i. Illustration: We see that $f_0(x, p) = \delta(x - x_0)\delta(p - p_0)$ remains sharp (it can be checked by inserting it in the louviel equation) to yield $f(x, p) = \delta(x - x(t, x_0, p_0))\delta(p - p(t, x_0, p_0))$ [this is expected, since you're in essence saying there's only one particle]

ii. Illustration 2: We want to see what happens to a Gaussian like state, does it spread? We start with $\rho_0 = \frac{e^{-x^2/2\sigma^2}}{\sqrt{2\pi\sigma^2}}$ and $S_0 = px$ with σ and p constant. This form of S_0 has already been solved for and tells us $\rho = \frac{e^{-(x-vt)^2/2\sigma^2}}{\sqrt{2\pi\sigma^2}}$. There's no spreading classically! We'll see for the same initial conditions, what happens quantum mechanically.

iii. Illustration 3: What initial conditions yield a spreading Guassian? We start with the same ρ_0 but use $S_0 = \frac{m(x-x_0)^2}{2t}$, in which case the solution we saw the result is

3.7 Sunday | Aug 23

- Summarizing the work I did on saturday.

4 Reached the chapter on Bohmian Mechanics | Aug 24-29

4.1 Monday | Aug 24

- [research/ms] Worked on making notes about Bohmian Mechanics
- [GRE] words, vocab; quant
- [misc]
  - KVPY document received; had to attend a PhD defence; fixed the NET application issue (talked to Bagla sir etc.); got black and white print outs; scanned various documents
  - Found something called *SparkleShare* which is dropbox like with git under the hood. Works great. I can switch off the automated git uploading whenever I like and switch to manual git. For the usual things, I can use it like a dropbox folder :)

4.2 Tuesday | Aug 25

- [research/ms] Worked on making notes about Bohmian Mechanics from Holland
- [misc] Gave the NET document for resubmission.
- [GRE] vocab (questions), quant

4.3 Wednesday | Aug 26

- [research/ms] Started the Bohmian Mechanics part from Holland!! QCQI group meeting; I think it was Vikrams. He talked about Quantum Simulation; about tunneling.
- [misc] Registered for TOEFL
- [GRE] minor work

4.4 Thursday | Aug 27

- [research/ms] Reading Bohmian Mechanics part from Holland
- [misc] Scanning, printing etc., packing
- [GRE] word list creation on Inkscape

4.5 Friday | Aug 28 [off, travel]

- [GRE] word lists

4.6 Saturday | Aug 29 [off]

5 Midsem, Presentation, Bohmian Mechanics (made sense of insolent multi-valued integration..) | Aug 31 - Sep 6

5.1 Monday | Aug 31 [off, travel]

- [course] Philosophy Reading the book

5.2 Tuesday | Sep 1

- [course] Philosophy: Found *@voice* an application that can read out things from the phone; used gscan2pdf for converting the notes sagar had sent to OCRed PDF (which isn't very good to look at) which @voice played out :D Also made proper notes for most of the 1st chapter of the book by Benn.

5.3 Wednesday | Sep 2 [exam]

- [course] Philosophy: studied and took the exam
- [research/ms] Started making notes on the Bohmian Mechanics part, (chapter 3) from Holland; got a place in CAF to sit and study (an office if you will)
- [misc] gave away the sweets

5.4 Thursday | Sep 3

- [research/ms] Presentation: Kishor talked about LOCCs, majorization, the iff condition between them, entanglement distillation and entanglement of creation and finally how they're related to key distribution (he barely started);
  - Bohmian mechanics working on figuring that $\oint dS = nh$ and tried constructing the cases when this could happen
- [misc] Tug of war practice + Abhishek sir nomination + re arranging the room in CAF + landscape thing
- [gre] verbal GRE work done

5.5 Friday | Sep 4

- [research/ms] Presenation; mine: Talked about how to get to the Hamilton Jacobi equations and took some time. Working out the differential equations is hard. Completed till about page 4 of my notes. It went well
  - Bohmian mechanics: figured the relation with the curl theorem of $\oint dS$ and atleast it now makes a little more sense. Need to work out the details still though; the point is that I now know which curl related theorem could be used, about which I wasn't certain until now.
- [misc] Table chair thing + landscape thing + multiple protocols
- [gre] quant GRE

5.6 Saturday | Sep 5

- [research/ms] Concluded that I'll just ask sir now for some advice. What I'm doing doesn't seem to help;

  So we start with $\oint dS = \oint \nabla S.dx = \oint p.dx = \int \nabla \times p.da$. If $\oint dS = nh$, then we must have $\nabla \times p = \sum_a \Gamma_a \int_{\gamma_a} \delta(x - x_a) dx_a$ where γ_a is the nodal line. If we assume $\oint dS = nh$ holds, then can we construct some example of the same? Let's first see how $\oint dS = nh$ can be derived. If the only condition is that ψ is single valued, then we know that at any point, S' and S both yield the same ψ, where $S' = S + nh$. If one considers a loop, then say we start from a point S_a. Then after completing some distance, the change in S is given by ΔS. So the value of S starting from $S_a$will be $S_a + \Delta S$. Now if we come back to the point a, then from uniqueness of ψ, we only demand $S_a + \Delta S = S_a + nh$. If S itself was unique, then we'd say $S_a + \Delta S = S_a$. Now at this point itself I seem to have trouble. I have tacitly assumed that S is single valued when I'm evaluating the 'change is S' along the curve.

  Talked to manu for a while and made some progress, then figured it was non-sense and made some more progress. Finally, Manu found a document that helped clarify a few things. The issue was still that they had used a vector field and not a potential. And it wasn't clear to me what potential must I use in that case.

- [misc] words GRE (prashansa came to CAF) + teacher's day (abhishek sir was awarded) + dinner

5.7 Sunday | Sep 6

- [research/ms] Finally found a potential that works (looked at acheson, griffiths and an extra document that manu had found.)

  The potential is $V = k\theta$. Note how this is itself, as a function of position is multi valued and yet we never have any issues integrating this (as we'll see shortly). While V is multivalued, $\nabla V = \frac{k}{r}\hat{\theta}$ is happily single valued :) And not just that, check this; $\oint_\gamma \nabla V.dx = 2\pi$ (simply because γ is chosen to be a circle and then $dx = rd\theta\hat{\theta}$). Since in the domain of interest, everything is well defined, I can write $\oint_\gamma \nabla V.dx = \oint_\gamma dV = 2\pi$. And one can show independently (I know only a simple minded proof with discritizing the function) that $\oint dV = 0$ whenever V is single valued (or a function). So what does this example show? Various rather peculiar things. (I) that $\oint dV$ maybe non zero for a reasonable physical situation by virtue of multivaluedness of V. Yes, V is multivalued and yet we can integrate the said expression without ambiguity. (II) that there happens to be a singularity within the loop, over which the integral is non-zero. (III) The curl, $\nabla \times \nabla V \neq 0$ at the center and $= 0$ else.
  Now we've made plausible various things which would've seemed arbitrary otherwise.

- [misc] trying to get a template in which to write the thesis

6 Temerity of GRE prep, Sycophancy or Ascendency of Bohmian Mechanics | Sep 7 - 12

6.1 Monday | Sep 7

- [research/ms] Worked out an explicit example of $\nabla \times p = \sum_a \Gamma_a \int_{L^{(a)}} \delta(\mathbf{x} - \mathbf{x}^{(a)}) d\mathbf{x}^{(a)}$ by assuming that the $d\mathbf{x}^{(a)}$ term is infact a vector and $d\mathbf{a}$ is essentially $\mathbf{n}dxdy$ to see that everything fits well eventually. Then I moved to the next section and everything seemed alright. Basically the discussion of the 'quantum potential term', viz. the term which if removed would reduce the expression to a classical hamilton jacobi equation, was slightly confusing. This term is given by $Q := -(\hbar^2/2m)\nabla^2 R/R$, and yet the claim is that this essentially depends on S. The same can be said of V which depends on $x(t)$ which in turn can be determined only once S is specified (NB: S contains information about both the Hamiltonian and the momenta of the system). So in this sense, both V and Q depend on S. However, the distinction seems to be made on the following ground: if $V = V(x)$ then, ∇S is sufficient. However, for Q it seems higher derivatives will also be necessary. Why this makes any difference, I am yet to learn.

- [GRE] did 2 tests in the morning (both verbal, am still miserable at them :( )

6.2 Tuesday | Sep 8

- [research/ms] Revised work I did yesterday.

- [research/summer] started the process of submission to PRA. Figured how to change the name from Dr. to without Dr. and then am waiting for Ali; he has to tell me if I should use the arXiv link or not. Made reasonable progress at putting things up online. The main trouble was that I wasn't able to add references. Figured there was a command '\nocite{*}' that had to be inserted to fix things. Did that. In addition, I had to change the split images into a single image; else the system kept yielding errors. After that, I had to follow some minor steps but then I got a response from Otfried. He had completed proof reading the pre-print and had made several comments which had to be accommodated.

- [GRE] did 2 tests, 1 math, 1 verbal (need to find more tests now) | did vocab also :)

6.3 Wednesday | Sep 9

- [research/ms] Bohmian Mechanics; finished reading the section on uniqueness of the wavefunction and started reading further, about commutation relations and so on. Some summarizing etc. will be typed out here later. Also, prepared for the presentation with Arvind sir;
- [research/summer] glanced through the changes suggested by Otfried.
- [GRE] morning slot: didn't wake up | vocab only

6.4 Thursday | Sep 10

- [research/ms] :(Didn't do much but thought of the following. Why can't I think of a bohmian like picture, after a particle has been created for instance, according to QFT. Perhaps I should start playing with the scalar field theory and try to see how I can get bohmian trajectories into them somehow. I suppose the eventual goal would be to think of an alternate interpretation to the field interpretition. I feel that there's some better way of handling these things. What does Hamilton Jacobi translate to? What happens to the Dirac equation? In QFT we treat the dirac equation as a field and create particles off of it. What will we do here?
- [research/summer] working on fixing the paper based on Otfried's comments. Minor fixes were quick. Fixing some references took time. The issue was that one has to use {} for evaluating commands within bibtex's .bib file. Didn't know that. Also fixed some other minor things. Located the PACS numbers related to the paper, started working on a cover letter and found appropriate emails for referees.
- [GRE] morning slot: read a little about the writing comprehension etc. but was too tired, slept after breakfast | did vocab words :)

6.5 Friday | Sep 11

- [research/ms] Thought about the relationship between QM and QFT. Even tried to derive the $[q, p]$ commutation starting from $[\phi, \pi]$. Figured ofcourse that if there're no commutations in Bohmian. This makes the Field theory aspect harder. Also got Bohm's original papers and started reading them.
- [research/summer] Finalized the cover letter and finally, submitted it to PRA. My first submission :D
- [GRE] afternoon slot will be used
- [misc] Phys Majors had a meeting.

6.6 Saturday | Sep 12 [weekend]

- [GRE] GRE vocab + test
- [misc] There was a discusison on entropy/information by Raja Ram Mohan which was intriguing, although most of what he said was familiar.

6.7 Sunday | Sep 13 [weekend]

- [GRE] prep
- [misc] Onam, tug of war

Hegemony of GRE | Sep 13 - Sep 24

- [GRE] prep all the time :(

Besieging exams and beyond | Sep 25 - Sep 30

- Took the TOEFL (Sep 26) and the GRE(Sep 28)
- Relaxed for about half a day + Personal efficiency improvement | standing straight improvements; wifi linux broadcast attempts (made improvements, but not successful); other improvements

7 Reprisal of Physics | Oct 1 - 3

7.1 Thursday | Oct 1

- [misc] Room efficiency tasks | washed clothes, cleaned the room, found the old phone (with the sim), resuming tasks (calendars, emails (KVPY, deanacad etc.), PhD application issues etc.
- [research/ms] just resumed reading
- [GRE S] classical mechanics questions (Klepner and Kolenkow)

7.2 Friday | Oct 2

- [GRE S] classical mechanics primarily (Klepner and Kolenkow) + had the first help course
- [research/ms] resumed reading Bohm's paper
- [Misc.]

7.3 Saturday | Oct 3

7.4 Sunday | Oct 4

8 Indignant time contraction; Immuring Physics, Applications and Bohm | Oct 5-10

8.1 Monday | Oct 5

8.2 Tuesday | Oct 6

8.3 Wednesday | Oct 7

- [GRE S]
- [research/ms] Was trying to translate bohmian mechanics to the discrete case. Realized there'll be an issue with the grad operator but then in the case of spins, we never write the kinetic energy term! Then some discussion with Jaskaran got me realize the following rather interesting conflicting statements, at least for spins. (I) QM is non local; Essentially if you assume locality, you can show determinism must exist, using EPR type states. Now one can use Bell's inequality to show QM is non local. (II) QM is non-deterministic can be shown using contextuality arguments and coloring theorems.; Comment A: Regarding the conclusion of (I), it is weakened by the fact that one can show you in QM, you can't communicate faster than speed of light. So in this sense the non locality is, well is it there at all?[1] Comment B: Disregard (I) for the moment. Usually in starting a proof for Bell's theorem, we assume that both locality and determinism hold (whether one can be derived from another is another matter). Thus a violation entails atleast one of the assumptions is wrong. We don't know which. From (II) it seems therefore that atleast determinism is false.[2] Comment C: If only (I) were true, then constructing a theory such as Bohmian mechanics may seem strange because then non-locality is explicit, but in some sense more sensible for we don't need 'observers' to make sense of what we're saying. If (II) is true even in continuous variables, then it would seem meaningless to even imagine constructing a theory such as Bohmian Mechanics.[3] Conclusion: One needs to come up with either a contextuality test for continuous variables (Ali's work) or figure how to setup Bohmian Mechanics for discrete variables.

8.4 Thursday | Oct 8

- [GRE S]
- [research/ms] got sick of trying to figure the spin thing and decided to linearly read the papers by Bohm first and then thinking about what to do next.

---

[1] Infact, one can show that the bell's inequality can be violated by 2 if the only assumption is no communication. QM does $\sqrt{2}$. So whatever this non-locality, it is certainly more restrictive than simply enforcing 'relativity'.

[2] I had been told that in contextuality, certain other assumptions have to be made (of which I'm not certain).

[3] There maybe defenses such as the observables are not really the same as the object's position/momentum etc. but more on that later

8.5 Friday | Oct 9

- [GRE S] Drude model etc.
- [research/ms] Bohm's paper; thinking about why it is that we can't talk about gravity essentially like electric fields. And then the obvious question that arises is that can there be a magnetic analogue of electric fields? | Jaskaran gave a small talk on Contextuality. He discussed various things including the KSBS inequality etc. Also briefly discussed the overview of the field and the progress. He concluded with discussing the nature paper about the Magic qbit etc.
- [Misc.]

8.6 Saturday | Oct 10

- [GRE S] griffiths, electrodynamics first chapter, classical mechanics revision
- [research/ms] Bohmian Mechanics (resumed reading the paper)
- [Misc.]

8.7 Sunday | Oct 11

- [Misc.] Philosophy exam prep
- [research/ms] Casually thought of linking Ekert and EPR protocols, primarily kichoo's idea + suggested a modification to jaskaran and kichoo's protocol

9 Emaciated resurrection of physics and inclemency of all else | Oct 12 - 16

9.1 Monday | Oct 12

- [Misc.] Philosophy exam prep, talk on Quantum Theory and SpaceTime (a person from IISER P had come and there was this student of his who in his fifth year (had in his fourth infact) published over 3 papers on the said subject! Insane), various things for the PhD application to leeds done

9.2 Tuesday | Oct 13

- [Misc.] Philosophy exam; iGuess: I figured how Stern Gerlach is analyzed in Bohmian Mechanics, also I read about how contextuality etc. it handled in bohmian mechanics. This was from Durr's book.
- [research/ms] Thought about how one could use the quantized harmonic oscillator analysis (Bohmian Mechanics) on Quantum Optics and see if it makes enough sense. Obviously this would not be about photons, but then what would be can probably be explored. I found later that he (Bohm) has already considered some such cases. This is particularly relevant if I want to apply the contextuality analysis (one that's extended to continious variables) to bohmian mechanics to see what's going wrong.

9.3 Wednesday | Oct 14 [unwell :(]

- [group meet] Bhati's talk was rather interesting, about collapsing of wavefunction etc.

9.4 Thursday | Oct 15

- [research/ms] Reading/thinking about Bohmian Mechanics;
- [sGRE] usual revision

9.5 Friday | Oct 16

- [research/ms] Could barely do anything :(
- [Misc.] Had to go get a draft done for TIFR; found the toothbrush (for Arjit); tried to get Headphones (for being able to work in the office); finally even ordered them

10 Subject GRE misconstrued as a Hiatus? | Midsem break (Oct 16 - Oct 25)

- Subject GRE prepared for well :) went home and back after the exam;

11 Demurred Obstacles | Oct 26 - Nov 1

11.1 Monday | Oct 26

- [Misc.] Couldn't get enough rest, started slow; fixing things at the office (resuming efficient work); organizing things, updating calendars etc.; there was an earthquake today; updated various things
- [research/ms] resumed reading Bohm's paper | Finalized some things:
  - (a) Bohmian Mechanics: (i) Discuss the basic formalism (ii) Discuss how measurements are done (the hardest part to explain) (iii) Explain how a position measurement may not even yield the 'true' position (iv) Talk about spins; the stern gerlach in terms of Bohmian mechanics;
  - (b) Contextuality: describe it;
  - (c) Ideas worth exploring: (i) Extension to EM fields, not with photon trajectories as the essential target but for (ii) The relation between Bohmian and Contextuality; (iii) How there maybe ontological models s.t. measurement disturbs the values of the hidden variable; this is essentially how they explain spins, or even position/momentum uncertainty etc.; the basic idea then is that how is Bell's test acceptable? Because the contextuality tests are also built like so and they get away with it by saying that contextuality is about spins, and that is not really ontological. However, now they even have continuous variable contextuality! These need to be phrased clearly.
- [research/ms] Started looking for a poster template and started filling it slightly. Also tried getting lyx to work using textext but that didn't work. They haven't updated to make it functional in the new version.

11.2 Tuesday | Oct 27

- [research/ms] Meeting with Arvind sir | described the basic idea of the poster (emailed it to him). In the next meeting with sir, we concluded that we (read Jaskaran and Kishor) write down all versions of the algorithm. Apparently my small modification itself can become the main protocol, once security can be assessed well. That part still has to be explored.
- [research/summer] Tried to look at the issues pointed out by the referee and tried addressing those I could. Wrote to Ali. He said he'd be able to handle most. I updated the technical issues (about figures etc.)
- [misc] Had the philosophy lecture; Also thought about why it is that we don't have superposition of charges, like we have superposition of magnetic moment; I mean we have states like $|\psi\rangle = \frac{|\uparrow\rangle + |\downarrow\rangle}{\sqrt{2}}$ but we don't have a neutron being in a superposition of + charge state and - charge state. I guess then this is the starting of particle physics in some sense. But this is still a curious thing, the charge for B field can be in a superposition but the charge for E field can't be.
- [health] running initiated + gym

11.3 Wednesday | Oct 28 (Unwell)

- [research/ms] Arun delivered the talk. He was insisting on the epistemic view of science. He said two things which are relevant for my work. First was the idea of a reduced density operator and its connection with Bohmian Mechanics. Detlef's book has discussed this. The next idea which is more directly relevant at the moment is that of the GHZ test. He was happily asserting that there the notion of reality must be given up. It was then obvious for me to use this as the starting point for my project. The continuous version of the GHZ test already exists and it would be fun to see how it works/fails with bohmian mechanics.

11.4 Thursday | Oct 29

- [research/summer] Updated some things that Ali had sent. There were various aspects that had to be looked at. Infact, Ali had even made a small mistake in the references (had put the wrong one). Made some minor language changes and sent it to him.
- [research/ms] Found an interesting paper that discusses how bohmian mechanics handles Bell, GHZ and more. Infact, finally I figured precisely what it is that I'll work on. Instead of looking at contextuality per say, I would instead look at the GHZ test generalized to position and momentum. The paper I found could be useful in setting up the system. In essence then I must look at the following: (1) How is measurement generalized in Bohmian Mechanics [detlef's article should suffice] (2) GHZ test in continuous variables (3) GHZ test in Bohmian mechanics
- [remarks/arvind]: Contextuality and GHZ, Nielsen doesn't prove the ancilla and POVM statement
- [misc] Got an email from the University of Leeds asking for my TOEFL scores and stating that Dr. Beige is interested in supervising my project.

11.5 Friday | Oct 30

- [research/ms] started figuring how to write the first simulation.
- [research/summer] Ali had responded. Made appropriate changes. Made some language changes. Sent it to Ali for a final glance before resubmission.
- [misc] Shopping (food) + nutrition optimization started; jelly lost her keys and found it!

11.6 Saturday | Oct 31 (unwell)

- [research/summer] Did a final review of the changes and resubmitted things to PRA.
- [research/ms] Formulated the thesis problem more clearly (in an email I wrote to Ali). Writing the small goals for the project to work on (constructed the appended topics part).
- [FYI] for lyx, to cite things, first Insert -> List .. -> BiBTeX .. and add your bibtex file. Thereafter, just use Insert -> Citation and you're done. Also, to restart numbering after a section, add \@addtoreset{section}{part} to the preamble. And finally, to add a book for citation, use this website isbt-to-bibtex.

11.7 Sunday | Nov 1

- [PhD] Applications; found this loop quantum gravity guy in France; wrote to him (he responded, as I'll find later). Have to write a letter of motivation/proposal etc. and that's about it.
- [research/ms] Found something called the relative interpretation of QM. COuldn't fully understand/appreciate it. Also learnt some GK about loop quantum gravity.

12 Voyage of Veracity, BM QM | Nov 2-9

12.1 Monday | Nov 2 (20 hours up time!)

- [research/ms] Working on the numerics aspect: Found various documents on simulating the schrodinger equation [1, 2]. They range from using RK4 to using more interesting hybrid QFT approaches. Setup the whole thing in fortran from my old chaosTerm project. Got the basics up and running. I can initialize to a Guassian now.
- [misc.] extreme shopping, project nutrition seems to have been erected successfully; also figured how to keep my schedule fixed

12.2 Tuesday | Nov 3

- [research/ms] Idea: Why is it that position/momentum can't be used to harness, essentially arbitrary quantumness? Why do we rely on formalism similar to spins for extending to the CV setting?; Next, working on the numerical simulations. It's going good. About to simulate the schrodinger equation in a very simple case. Then we'll see the bohmian trajectory for a single particle!; In practice however, I am running into issues with attempting to simulate the schrodinger equation. The point is that $\dot{\psi}$ is needed at arbitrary q and that's not possible without interpolating $\nabla^2\psi(q)$.
  Since I couldn't find anyone (surprisingly) to help me with this, I simply interpolate, using Ref [3]. However, I just realized (after having almost written the whole code) that the spline interpolation is for reals, not for complex. I think it can be readily extended, but then I'd have to mess around with spline function! :(
- [misc.] talked to dad after a long time, started making philosophy notes on the computer now

12.3 Wednesday | Nov 4

- [research/ms] Working on the interpolation; accommodating interpolation of complex by treating them as 2 reals. So basically in fortran, here's one nice way of handling things. [todo: complete this seciton]
- [research/summer] Got a response from the referees. Wrote a response (this time Ali asked me to do it straight!) and sent it to him. He suggested another change. Added that and sent it. He then wanted me to confirm what I had written. I did and reverted to him. He also read and confirmed that what I'd done was correct. He infact read it within a half hour and was even able to explain certain things I hadn't read, that were discussed in the paper. All in all, made the required changes and sent them before sleeping.

12.4 Thursday | Nov 5

- [research/summer] The paper got accepted today!
- [research/ms]

12.5 Friday | Nov 6

- [revision] Spent the day reading about Corioli's force, it was rather interesting. I was wondering if there's a quantum corioli's effect. And it turns out that there's one paper that discusses this.

12.6 Saturday | Nov 7

- [revision] Was reading about corioli's force etc. to prepare myself for presenting in front of the class, also did some coordinate systems (spherical, polar etc.) and in the actual session could only complete coordinate systems. Also made a nice feedback + attendance sheet and from the feedback it looked like they're happy with what they learnt.

12.7 Sunday | Nov 8

- [research/ms] finished putting all the pieces together and debugging all the errors to get the program to run for the first time, only to realise that RK4 can't work in principle. After evaluating $m1$, I realized that I can't do $q + m1 \times dt$ simply because $m1$ is not guarenteed to be real. I tried fixing this by taking absolute values and it still wouldn't give the right results. Spent some time in the night in trying to convert it to euler to see if something improves. No help. Eventually found a small *mistake* in the second difference formula I was using.

13 Perusing through the Tumult of the Palimpsest Code | The hegemony of constructing the exegesis of my work | Nov 9-15

13.1 Monday | Nov 9

- [research/ms] Working on trying to figure why the code's not working. Now I'm in a situation where I 'print' an object (of custom type) before sending to a function and then inside that function. They produce different results. WTH! Anyway, trying to figure this. In one attempt found a way to add procedures to types themselves (like a class). Apparently, the proof of concept code works in isolation but is'nt working with my code. Trying to figure that. Man Fortran's killing me today. So figured that. Here's the deal: when using procedures within a type, do the following:

```
type contVar
        integer :: someVariables
        contains
                procedure, pass :: contVarAllocate
end type contVar

contains
subroutine contVarAllocate(this,val)
        class(contVar) :: this
        integer :: val
        this%someVariables=val
end subroutine contVarAllocate
```

  The most important point in all of this is that you've to use the keywords *class* when you're using the datatype inside a class. The drawback is that doesn't fix the issue still :(
  Next I tried debugging using dgb. Spent quite some time, only to find that indeed before and after sending the values are different. :(
  Eventually tried writing a code with an allocatable array separately and that apparently does work alright. Alright, *figured* it. The issue was that the return type of my function was real and the value I intended to return was complex. So for some reason, that issue was rising.
  Even after resolving it, it just won't work. Neither with Euler nor with RK4. It just won't evolve properly. The norm goes crazy. Now I have no clue why it isn't working.

13.2 Tuesday | Nov 10

- [research/ms] Still can't figure what's really wrong. Checked the $\nabla^2\psi$ evaluated using the discrete difference formula and compared it to differentiating the interpolated polynomial. The latter looked better, but the difference isn't all that marked. I suppose I'd be forced to use FFT after all. I suppose the basic difficulty is that in this $\dot{\psi}$ involves a double derivative of ψ itself, whereas in all previous cases that I considered, $\dot{q}$ will usually be a function of q^m etc. Anyway, changing the approach at this stage doesn't feel feasable. I doubt anyway that this will work.

- Talked to sudeshna ma'am. While talking to her, realized that we have to keep the $\Delta t/\Delta x^2 \leq 10^{-6}$ or so for euler and 10^{-4} or so for RK4. I had made another small mistake. I was returning only the real part of $\nabla^2\psi$ initially (in the differentiation of the interpolate polynomial method).

- Talked to Abhishek sir and realized that I wasn't really using RK4. I had made a mistake in understanding how to implement. After the discussion figured that, fixed it and it started working! The mistake was that I was using different xs for finding $k_1, k_2, \ldots$ whereas to evaluate k_2 for example, I needed $x + 0.5\,dt\,k_1$ but $k_1$is complex in general. So I already had hints on things going wrong. But I realised after that $\psi + 0.5\,dt\,k_1$ is what I had to use because in some sense, I'm treating ψ as a functional. So, given an x, I have to find $\dot{\psi}$ at different ψs not different xs. So that fixed things. It was sad though because the interpolation code was completely useless!

13.3 Wednesday, Thursday | Nov 11, 12

- [research/ms] Numerics:
  - Got almost all basic stages to work. Was able to get the harmonic oscillator (as was done yesterday). Improved on the parameters. Then attempted to get one particle trajectory. And it worked!
  - As it turns out, interpolation was not useless but rather important. This is because I needed to find $\dot{q}$ at different qs and $\dot{q}$ is a function of $\psi(q)$. So in effect I needed both ψ and $\nabla\psi$ at arbitrary points. This worked well because I had already the interpolation code.
  - Next was to generalize this to handle an ensamble of particles. This was done by extending the interpolation code to handle arrays naturally and then the beauty of fortran kicked in and everything eventually started working smooth. Ofcourse I had made minor mistakes here and there, for example trying to add precision(real) to ψ so that I don't get a division by zero in $\dot{q}$ but that doesn't work like expected.
  - Anyway, after all of this, I realized that I need one last thing. I must start with particles distributed according to $|\psi|^2$and then evolve them. This did work finally. For this I wrote an algorithm, which was similar to the one I figured as an answer to Bagla sir's question in the computational physics course. That was a standard method of using the cumulative distribution and then inverting it. This new method was basically something that can be, unlike the former, generalized to arbitrary dimensions (at least intuitively, haven't proven this).
    * The algorithm is simply this. Start with finding the maximum of the probability density function, call it $P_{\max}$. Then construct an interval from 0 to $P_{\max}$ and successively choose a point in this interval, and move in constant steps; at some iteration, call this point h. Next, construct random numbers uniformly in the relevant range (over which the PDF is defined; viz. the relevant domain if you're fussy). Let's call it for instance x. Then check if $P(x) > h$. If it is, then you accept this point (save it in some array that'll represent the set of points which follow the said distribution). Else you reject the point. Repeat this a fixed number of times for each h. The claim is the set of points you get will satisfy the same PDF. Why it works is intuitively obvious. Start with considering $h = 0$. All points are accepted. Then as h increases, fewer and fewer points are accepted; viz. those which are more likely according to the PDF, get more representation in the final set of points. This is readily generalizable.
    * There is however a major draw back. How many particles you'll have in the final array is stochastic in this algorithm. However, this can be easily fixed. Consider the further generalization. Find h uniform randomly and then find x uniformly randomly as stated. If x is accepted, good else repeat till you have enough particles. Because of the stochastic nature of x, increasing h sequentially, or selecting it randomly, both are equivalent. The latter however allows for an obvious way of fixing the number of particles.

13.4 Friday, Saturday | Nov 13, 14 [intensity: 30 hours up out of 32 hours elapsed]

- [research/ms] Working on finalizing the poster (here's the final 1). Did various things. Read the paper that had analysed the GHZ and Bell case with Bohmian Mechanics, then looked at contextuality (the pierse mermin version) and finally looked at the phase space GHZ test. I liked the final output. Getting it printed was a chore by itself. However I was told later that there's too much text in it.

- [teaching] Took a class on Coriolis force. It was fun. Couldn't do too many examples though.

- [misc] Food improvement failed again today.

Bohmian Mechanics and Contextuality in (q,p)

A. S. Arora

Supervisor: Prof. Arvind; QCQI Group, IISER Mohali, India

Introduction

Abstract

There are at least two known theories that describe the same physical world; Quantum Mechanics and Bohmian Mechanics (also uses the Schrödinger Equation). While the latter is not popularly known, it provides exceptional clarity about certain aspects of reality. So far, no tests are known that can distinguish these two. One fundamental difference between them is that that the latter is deterministic (in the sense that (q,p) are well defined). The purpose of this thesis is to get these theories head on; we aim to construct a theoretical situation where this type of determinism is refuted by Quantum Mechanics (using generalization of the GHZ test, contextuality etc.) and analyze it using Bohmian Mechanics. This is a step towards understanding the relation between contextuality and non-locality.

Content

I will first discuss Bohmian Mechanics and then go on to discuss the known standard tests of determinism (GHZ and contextuality). Thereafter I will describe how the GHZ test is explained from the Bohmian perspective and also discuss a generalization of the GHZ test to phase space (q,p). Finally, I'll show some simulation results which will be generalized in the future to perform the generalized GHZ test using BM.

Bohmian Mechanics [5, 4, 2]

Background

The Quantum Mechanics (QM) that is taught, is usually the one which uses the 'Copenhagen interpretation'. This interpretation asserts that the most complete possible specification of an individual system, is in terms of ψ which yields only probabilistic results. While it can be shown to be consistent, it is worth exploring the reasons for believing this assertion. David Bohm[a] in an attempt to investigate the truth behind this, constructed a theory with 'hidden variables' (positions and momenta (q,p) of particles) that in principle completely specified the system but in practice get averaged over. He was able to show that his theory yields the same results as QM in all the physical situations he considered. Such a theory is worth studying because the following are at stake.

(1) Clarity: First, the widely held notion that at the atomic level, we must give up any conceivable precise description of nature, is plain false because there exists a heretic counter example, Bohmian Mechanics (BM). Second, deriving Classical Mechanics from QM (in it's usual interpretation) isn't possible due to the arbitrary distinction between the classical and quantum worlds. Within BM, classical mechanics can be recovered clearly.

(2) Accuracy of conclusions: The Bell test showed that there can't be hidden variable theories consistent with predictions of QM. Yet BM (Bohm's hidden variable theory) is consistent with QM; it allows the violation of Bell's inequality. The point is that we must be extremely careful about the conclusions we draw from our equations/experiments. The Bell test excludes *local* hidden variable theories, and BM is explicitly *non-local*.

There are a host of interesting questions which can be raised. For instance, one could ask why position and momentum aren't simultaneously determinable if in principle they're well defined? In the double slit then, the particle goes through one of the slits? Can one observe these trajectories? If particles have trajectories, what happens to identical particles? What happens to spins? Does the explicit non-locality entail we can communicate faster than light? Can one distinguish between BM and the usual QM experimentally? All these questions, except the last, have been solved or at least addressed.

Formalism

According to Bohm's original formulation of BM, a particle is associated with (1) a position and momentum (q,p), precisely and continuously defined & (2) a wave (ψ). For their description, the following are assumed:

- The ψ-field satisfies the Schrödinger equation.
- The particle momentum is restricted to $mv = p = \nabla S = \hbar \operatorname{Im}(\nabla\psi/\psi)$, where $\psi = Re^{iS/\hbar}$ and Im is the imaginary part.
- In practice, we don't control/predict precise locations of the particle; instead we have a statistical ensemble with probability densities $\rho(q) = |\psi(q)|^2$.

Comments:

(1) Note that the observers play no fundamental role in the formalism. If $\hbar = 0$ then we recover the classical Hamilton-Jacobi equation. Unlike QM, BM has a clear classical limit.

(2) These are readily generalized for N particles. Non locality in that case becomes explicit; $p_i = \nabla_i S(q_1, q_2, \dots, q_N)$ viz. momentum of the i^{th} particle depends on the instantaneous positions of all particles.

(3) Extension to spins: In BM, the particle only has (q,p). The spin is associated only with the wave-function. For a spinor, say $\Psi \equiv (\psi_+, \psi_-)^T$, the generalization is that $mv = \hbar \operatorname{Im}((\Psi, \nabla\Psi)/(\Psi,\Psi))$ where $(.,.)$ represents inner product in the spin space $\mathbb{C}^2$.

[a] Historically, de Broglie had formulated a similar theory and then gave it up. Later Bohm independently re-discovered it

Determinism

The GHZ test [7]

Objective: To show that realism is incompatible with QM.

Assume: Three particles are allowed to interact and three observers are given one particle each. The interaction is such that the following holds. There are two properties of these particles one can measure, X or Y. The outcome of the measurement is either 1 or -1.

Construction: Interestingly, for a specific initial state of these particles, if they measure $A = X \otimes Y \otimes Y$ then the outcome is guaranteed by QM to be +1. This also holds for $B = Y \otimes X \otimes Y$ and $C = Y \otimes Y \otimes X$. However, if $D = X \otimes X \otimes X$ is measured, then the result is -1.[a] Explicitly, this can be achieved with 3 spin half particles for example, with $|\psi\rangle = (|000\rangle - |111\rangle)/\sqrt{2}$ (where $\sigma_z|0/1\rangle = \pm|0/1\rangle$) and X, Y, Z as Pauli spin operators.

Hypothesis: Assume that the world is deterministic, viz. the properties had predefined values. Then if we evaluate ABC, then we know by construction that it must be $= 1$. However, it is also true that $ABC = D$ (because $Y^2 = 1$). By construction we also know that $D = -1$. Thus we arrive at $+1 = -1$.

Conclusion: This entails that our hypothesis must be wrong. More precisely, this implies that we can't have non-contextual determinism where the qualification "non-contextual" is subtle but necessary.

Contextuality [7, 8]

Two observables A and B are mutually compatible if the result of measuring A doesn't depend on whether B is measured (before, after, simultaneously or not measured at all). If we restrict ourselves to hidden variable models that assert that A and B have predefined values, irrespective of which compatible observable is measured, then such a theory would be termed "non-contextual" and deterministic. Kochen-Specker proved that such theories, viz. non-contextual deterministic theories are inconsistent with QM. Mermin and Peres showed this for a four-level system. Consider the following operators.

$$\begin{array}{ccc} A_{11} = \sigma_z \otimes \mathbb{I} & A_{12} = \mathbb{I} \otimes \sigma_z & A_{13} = \sigma_z \otimes \sigma_z \\ A_{21} = \mathbb{I} \otimes \sigma_x & A_{22} = \sigma_x \otimes \mathbb{I} & A_{23} = \sigma_x \otimes \sigma_x \\ A_{31} = \sigma_z \otimes \sigma_x & A_{32} = \sigma_x \otimes \sigma_z & A_{33} = \sigma_y \otimes \sigma_y \end{array}$$

Note that operators along a given row commute. This also holds for a given column and thus these are compatible. Also note that the measurement product along any row (R_K) or column (C_k) is 1, except for column three; $C_3 = -1$. Thus, QM predicts $\prod_{k=1,2,3} R_k C_k = -1$, in contrast to non-contextual models. Since no experiment yields ideal results, an inequality must be constructed. It has been shown that all non-contextual theories must satisfy $\langle \chi_{KS} \rangle = \langle R_1 \rangle + \langle R_2 \rangle + \langle R_3 \rangle + \langle C_1 \rangle + \langle C_2 \rangle - \langle C_3 \rangle \leq 4$. QM yields $\langle \chi_{KS} \rangle = 6$.

Remarks:

(1) Note that Mermin's test is state independent, unlike the GHZ test.

(2) While there's a subtle connection between the Bell test and Contextuality, the latter is more suited for testing determinism (non-contextual) because the locality assumption is not required.

[a] The tensor has been omitted henceforth. The details have been

GHZ, Spins, Phase Space and BM

In this section we'll first sketch the analysis of the GHZ situation using BM. That BM is able to explain this is not very surprising since spins are not assigned predefined values in BM, unlike (q,p). A generalization of the GHZ test to phase space is thus desired. Such a generalization has been discussed in the literature and is briefly summarized.

Spin GHZ | BM Analysis [3]

Assuming that Stern Gerlach type apparatus are used to measure the spins of the different particles, the initial state of the system maybe described as $|\Psi(r_1, r_2, r_3, t=0)\rangle = (\psi_{+++}|000\rangle - \psi_{---}|111\rangle)/\sqrt{2}$, where r_i represents the position vector in the frame of the i^{th} observer. If the particles are assumed to be Gaussian initially and propagating (with speed v_0 for instance) along the axes of their respective SG apparatus, then one can further define $\psi_{\pm\pm\pm}$. It can be shown that[a] the time evolution of $\psi_{\pm\pm\pm}$ can be written as products of 3 single particle solutions of SG setup, which was analyzed by Bohm himself. Once $|\Psi(r_i, t)\rangle$ is known, one can evaluate the equation of motion for the three particles using the formalism of BM. If the SG apparatus are setup to measure say XYY, then it is found that (in the direction relevant to measurement), four attractors basins form: $(+++)$, $(+--)$, $(--+)$ and $(-+-)$. The product is always $+1$ as was predicted by QM. However, when the SG apparatus are setup to measure XXX, the trajectories are found to obey equations which posses four attractive basins: $(---)$, $(-++)$, $(+-+)$ and $(++-)$. In this case, we get -1 as the product, again in agreement with QM.

Remarks:

Non locality enters from the fact that the attractor basins depend on the settings of *all* SG apparatus. Contextuality from this perspective is essentially the statement that the results of an experiment, depend on the experiment being performed. [4]

[a] details have been skipped for brevity and clarity

Phase space GHZ [6]

Consider unitary operators X, Y and the following re-definitions; $A = X^\dagger Y Y^\dagger$, $B = Y^\dagger X^\dagger Y$, $C = Y Y^\dagger X^\dagger$ and $D = XXX$. If the following anti-commutations hold, then we'll arrive at a GHZ like situation; $\{X, Y\} = 0$ and $\{X, Y^\dagger\}$. Given this, it follows that (1) A, B, C, D all commute and (2) $ABCD = -\mathbb{I}$. Thus any simultaneous eigenstate of A, B, C, D will result in the GHZ situation. Explicitly, in phase space, for some length scale L, $X \equiv \exp(i\sqrt{\pi}x/L)$ and $Y \equiv \exp(i\sqrt{\pi}pL)$ ($\hbar$ is chosen to be 1 in this section) satisfies the aforesaid conditions. To construct the simultaneous eigenstates, observe that for

$$|\uparrow\rangle_{x_0,p_0} \equiv \frac{1}{\sqrt{2}}\Big(\sum_{k=-\infty}^{\infty} e^{i\pi 2k p_0} |x = x_0 + 2k\rangle + i \sum_{k=-\infty}^{\infty} e^{i\pi(2k+1)p_0} |x = x_0 + 2k + 1\rangle \Big),$$

$$|\downarrow\rangle_{x_0,p_0} \equiv \frac{1}{\sqrt{2}}\Big(\sum_{k=-\infty}^{\infty} e^{i\pi 2k p_0} |x = x_0 + 2k\rangle - i \sum_{k=-\infty}^{\infty} e^{i\pi(2k+1)p_0} |x = x_0 + 2k + 1\rangle \Big),$$

we yield[a] $X|\uparrow\rangle = |\downarrow\rangle$, $Y|\uparrow\rangle = i|\downarrow\rangle$ and $Z|\uparrow\rangle = |\uparrow\rangle$ and similarly for $|\downarrow\rangle$. From this, the generalization of the GHZ state is found to be $|\Psi\rangle = \frac{1}{\sqrt{2}}(|\uparrow\uparrow\uparrow\rangle - |\downarrow\downarrow\downarrow\rangle)$.

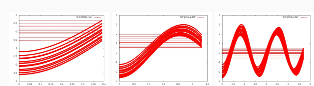
With the states and the operators defined in phase space, the GHZ test has been extended.

[a] here $x_0, p_0 \in [0,1[$ and are numbers. Strictly one must write in place of x, $\sqrt{\pi}Lx$ and for p, $\sqrt{p}/L$.

Achievements and Outlook

Results

In addition to learning BM, surveying the literature and narrowing the problem, the first few stages of writing a BM simulator have been achieved. This is of special interest since analytic solutions to Bohmian trajectories are rarely simple. Trajectories for free evolution, squeezed state evolution under harmonic potential (shown in the figure) and a one dimensional analogue of the double slit experiment have been simulated and found to be qualitatively satisfactory. The simulator was written in Fortran and uses RK-4 along with spline interpolation for time evolution.



Immediate Goals

Numerically, extension to many particles and ability to handle spins are the essential next steps. These are required to validate the spin GHZ test as described. Theoretically, improvement of the phase space GHZ test is desired so that normalizable states can yield the paradox. The analogue of the SG apparatus for measuring p is essential for a Bohmian analysis.

Future Scope

A more ambitious goal would be to explore phase space contextuality [1] using BM to understand its relation with non locality more directly. A puzzling question is that while formally in QM, spins and (q,p) are handled rather similarly, why can't we extend BM in a manner such that spins are as 'deterministic' as (q,p)? It is worth attempting to find such a formulation or to show that it doesn't exist. This is of great interest for this answer must depend on the fundamental difference between spins and (q,p) as properties. The thesis problem is a step in this direction.

Acknowledgement and References

Acknowledgement

I thank my project guide Prof. Arvind for his timely guidance and encouragement, despite not having faith in BM. I am grateful to Dr. A. Chaudhuri and Dr. S. Sinha for their assistance with the simulation. I also acknowledge K. Bharti, R. Bhati and J. Singh (QCQI group members) for various discussions. The QCQI group talks (especially those by S. Gambhir and A. Sehrawat) have been conducive in narrowing the thesis problem. KVPY and IISER Mohali are acknowledged for funding and research education.

References

[1] Ali Asadian, Costantino Budroni, Frank E. S. Steinhoff, Peter Rabl, and Otfried Gühne. Contextuality in phase space. *Phys. Rev. Lett.*, 114:250403, Jun 2015.

[2] David Bohm. A suggested interpretation of the quantum theory in terms of "hidden" variables. i. *Phys. Rev.*, 85:166–179, Jan 1952.

[3] Thomas Durt and Yves Pierseaux. Bohm's interpretation and maximally entangled states. *Phys. Rev. A*, 66:052109, Nov 2002.

[4] Detlef Dürr, Sheldon Goldstein, and Nino Zanghì. *Quantum Physics Without Quantum Philosophy*. Springer Berlin Heidelberg, 2012.

[5] Peter R. Holland. *The Quantum Theory of Motion*. Cambridge University Press, 1993. Cambridge Books Online.

[6] Serge Massar and Stefano Pironio. Greenberger-horne-zeilinger paradox for continuous variables. *Phys. Rev. A*, 64:062108, Nov 2001.

[7] N. David Mermin. Simple unified form for the major no-hidden-variables theorems. *Phys. Rev. Lett.*, 65:3373–3376, Dec 1990.

[8] E. Specker Simon Kochen. The problem of hidden variables in quantum mechanics. *Indiana Univ. Math. J.*, 17:59–87, 1968.

Figure 1: The Poster

13.5 Sunday | Nov 15 [unwell]

- [misc] I was too exhausted by the time Sunday hit. It was worth it though, finishing all the work in time. Emailed the poster to Arvind sir.

14 The judgement day | amending dilapidated machines and minds

14.1 Monday | Nov 16 [judgement day]

- [research/ms] Presented the poster to various people. Abhishek sir was impressed and Sudeshna ma'am also seemed impressed. Explained it also to yoyo and another person who's name I don't remember. Was able to express the idea and excitement. Aulakh sir felt it was philosophy (ofcourse he didn't spend enough time understanding what's being said). KP Singh felt that since BM doesn't make any 'new' predictions, it must be useless as a theory. He didn't call it philosophy but I doubt he thinks it is of any use.
- [misc] Food improvement finally worked! Packed for leaving.

14.2 Tuesday, Wednesday | Nov 17, 18 [travel + home]

- [misc] Fixed Anika bhena's [had to open it, re-attach the keyboard, and unplug it], Attika bhena's [win 10 issues (partly fixed), applications] and mum's computer [formatted to win 8.1, explained how to transfer information]. Also got the warranty issue taken care of and returned the defective product.
- [research/summer] Got an email from PRA, requesting a response to the proof of the PRA submission.

14.3 Thursday | Nov 19 [partly travelled + working]

- [misc] reached IISER @ 10:30 or so. ate something, bathed etc. and reached CAF by 12:30; went for lunch at 1:30 and after lunch, slept. Woke up at 6, went grocery shopping and then gym + food related.
- [research/summer] Looked at the PRA proof. These guys are rather meticulous. Wrote to Ali about the PRA thing. He's in a conference he said.

14.4 Friday | Nov 20

- [PhD] Responded to Almut, University of Leeds for the offer.
- [research/summer] Responding to the PRA proof. Wrote a rough response and sent it to Ali. This happened a few times. The final version (roughly) was sent to Ali at night.
- [misc.] Got the cycle!

14.5 Saturday | Nov 21

- [research/summer] The response to the proof was submitted finally.
- [misc.] Birthday celebration! A lot of things that happened, happened really well! Had a great time.
- [course/gr] Started with Ashok Sen's GR lectures; lecture 1

14.6 Sunday | Nov 22

- [misc.] post party, sleep caught up
- [course/gr] Started with Ashok Sen's GR lectures; lecture 1

15 Aftermath, Eliciting pillory, Gleaning GR | Nov 23 - 29

15.1 Monday | Nov 23

- [misc.] sGRE scores received. Not applying to the US anymore.
- [course/gr] lecture 2 (iGuess) | also scanned and uploaded the notes
- [PhD] looking up universities where I can apply given the GRE scores; wrote to sudipta (iGuess)
- [research/ms] thought of a way of constructing more optimized operators.

15.2 Tuesday | Nov 24

- [misc.] talked to Bag about US applications and QG in general.
- [course/gr] lecture 3 (iGuess)
- [course/qg] lecture 1 (PIRSA, by Rovelli)

15.3 Wednesday | Nov 25

- [course/gr] lecture 4
- [course/qg] lecture 2 (partly)
- [PhD/exam] TIFR questions (section A almost done)

15.4 Thursday | Nov 26

- [research/summer] Received the second proof of the paper. Have to figure it out and fix it.
- [course/gr] lecture 5
- [course/phil] assembling information and question 2 done
- [PhD/exam] Working on TIFR questions, section B. Started reading griffiths (the transmission lines chapter).
- [PhD] wrote some emails (3 people in the list that Sudipta had given)

15.5 Friday | Nov 27

- [research/ms] Optimized observables for the finite wavefunctions can be given in terms the usual displacement $e^{i\hat{p}L/\hbar}$ followed by a displacement of some periodic clump in the opposite direction $e^{-if(\hat{x}_{\text{mod}NL})\hat{p}NL/\hbar}$ (check this) where f is used to define the "clump" and N is the number of 'slits' for the given eigenstate. So the full operator would perhaps be $e^{-if(\hat{x}_{\text{mod}NL})\hat{p}NL/\hbar}e^{i\hat{p}L/\hbar}$. The difficulty then is to see how one can create an operator of the form $X^\dagger X = 1$. This still needs to be worked out.
- [course/gr] lecture 6
- [research/summer] The proof I think is still quoting the wrong references. Other things seem to be alright.
- [PhD/exam] Worked on 2 TIFR questions; got stuck at them; had to talk to Bag.
- [course/phil] Q3 done, working on Q4.

15.6 Saturday | Nov 28

- [research/ms] Real Bell Test in (q,p) and not one that's based on the spin case. Imagine there was no spin. How would we have arrived at an inequality for local realism? I don't want to look at a subspace. Perhaps there's a more direct way which we're oblivion to at the moment.
- [course/phil] Q4 working on it.
- Discussion
  - Squeezed state & Different colours and monitor for intersection
  - Introduction
    - Field
    - Problem
  - Background
    - Review of literature
    - Summary of important techniques
  - Results
  - Conclusion
  - References
- Joseph Samuel, (? what about him)

15.7 Sunday | Nov 29

- [course/phil] philosophy revision before the exam (question 6)

16 Imperial Tumult

16.1 Monday | Nov 30

- [course/phil] HSS exam
- [misc] HDD lookup

16.2 Tuesday | Dec 1

- [PhD] Europe list construction (from Loops 2015 conference I guess)
- [research/summers] Proof work
- [research/ms] Typesetting the report

16.3 Wednesday | Dec 2

- [PhD] Sent an application to NEI (iThink) + related work
- [research/ms] Typesetting the report
- [misc] circumstantial ionization issues

16.4 Thursday | Dec 3

- [PhD] SOP writing (quantum gravity)
- [research/misc] Talk by Sudip K. Goyal (iThink) on quantum memory etc.
- [misc] cycle repair attempt (failed as I'll learn later)

16.5 Friday | Dec 4

- [PhD] SOP verification/review by Prashansa etc., application to FAU (twice?)
- [misc] Manu's presentation verification etc.; Sudipta Sir seems to be against LQG; circumstantial distinct
- [research/misc] Talk by Sudip K. Goyal, he left open a paradox, spent some time figured it;

16.6 Saturday | Dec 5

- [PhD] After extreme confusion about what to do a PhD in, applications were sent to Max Planck and Rovelli.
- [misc] today was light, objective was to fix the schedule to optimize timing and efficiency, updated the calendar for various days

16.7 Sunday | Dec 6

- [misc] Various maintenance tasks: inbox gmail issue, hdd ordering, git repository issue, updated the log for various days. tel Figured the plan of action. So I must choose between LQG and String Theory. Provisionally I have chosen LQG. I have already started applying to places in Europe keeping this in mind. Now for US also, I can do the same. The only issue is that LQG may not be the best idea. To that end, the strings conference may be the optimal place to figure things. I think thus that I should apply to 3 places that do loop (for US) and then if after talking to some of these people I get more input, perhaps I can apply to 1 or 2 more places. First thing is that I should try to get a TOEFL waiver for these places. I must parallely apply to two places a day in Europe. One in Germany and one in France.
  The next thing is to revise physics/prepare for TIFR etc. That I think I'll start from tomorrow. Should perhaps spend atleast one hour on typesetting the documentation.

17 Amalgam of PhD applications etc.

17.1 Monday | Dec 7

- [misc] food
- [PhD] worked on PhD applications, finalized US universities

17.2 Tuesday | Dec 8

- [PhD] Maryland application (reco sent) + JEST registration, started preparing for NET(?)

17.3 Wednesday | Dec 9

- [PhD] Preparing for NET (electronics [from HC Verma] + op amp + complex analysis)
- [research/seminar] Jaskaran's talk on contextuality | still not very clear about why QM is contextual.
- [research/summer] PRA arxiv update

18 Inundation of Strings, Loops and NET | Dec 10 - 20

19 An Efficacious Harbinger

19.1 Monday | Dec 21

- [misc/personal] Jelly's birthday, prep + execution

19.2 [travel to Delhi] Tuesday | Dec 22

- [misc] Some family time | some home tasks taken care of

19.3 Wednesday | Dec 23

- [misc] Updated the calendar (partly), the keep and organized otherwise various things, multiple home tasks taken care of
- [PhD] scores sent to Louisiana State University, examined the deadlines of other universities

19.4 Thursday & Friday | Dec 24, 25

- [misc] Updated organization
- [research/ms] Initiated thinking | Realized that I need an optimized operator of the form $Y' = U^\dagger Y U$ so that my commutations are preserved. This doesn't seem all that trivial. The other approach is to construct everything for the variable ϕ instead of the position x, where the periodicity is natural and can be harnessed to construct eigenstates quite naturally. To progress along this direction one needs to construct the momentum operator in the polar coordinates. Now we know that $\nabla = \hat{r}\partial/\partial r + \hat{\theta}(1/r)\partial/\partial\theta + \hat{\phi}(1/r\sin\theta)\partial/\partial\phi$ and thus one can imagine how to construct the 'radial component of the momentum operator'$\equiv \hat{p}_r$. However as it turns out, the more useful quantity (routinely encountered) is the radial part of ∇^2, where by part I mean the one that can be separated/depends only on r. Why that's an issue is because first of all, one must know what it means to talk about $\hat{p}_r$ as one can show that $\hat{p}_r = \text{symmetrized}\{r^{-1}(-i\hbar\nabla).r\}$ and that itself becomes slightly messy. Then one must know how this will be measured. I already am not very clear about the momentum is measured off of the screen in our earlier work, let alone in polar coordinates. This however doesn't mean it is a dead end. I'll try to explore the first direction first.
  - One quick calculation one can attempt is to try to swap the position of the following operators (attempt a commutation like behaviour):

$$\begin{aligned} e^{i\hat{x}_{\text{mod}}\hat{p}L/L'h}.e^{i\hat{p}L_2/h} &= \left(1 + \hat{x}_{mod}\hat{p}L/L'h + (\hat{x}_{mod}\hat{p}L/L'h)^2 + \dots\right).e^{i\hat{p}L_2/h} = \text{<after some calculation>} \\ &= e^{i\hat{p}L_2/h}e^{i(\hat{x}+L_2)_{\text{mod}}\hat{p}L/L'h} \end{aligned}$$

    where L_2 is the distance over which the position is modulo over.
  - Next one can try to ask a more relevant question, which is: Does $\exists$ a U s.t. $e^{i\hat{p}Lf(\hat{x}_{\text{mod}})/h} = \hat{U}^\dagger e^{i\hat{p}L/h}\hat{U}$? One approach to this is to find the 'matrix elements'. This doesn't yield an exact solution. Can see the calculations if you wish.
- [PhD] Europe, sent to three people in France (iGuess)

19.5 Saturday | Dec 26

- [misc] Delayed christmas celebration prep and execution
- [PhD] Europe attempted (partly, iGess: sent to FAU)

19.6 Sunday | Dec 27

- [misc] Unwell, headache
- [PhD] Europe, tried looking for people in Cambridge. Found and sent an email to one retired person (earlier had sent it to some other retired person)

20 Ameliorated by the perquisite

20.1 Monday, Tuesday | Dec 28, 29 [unwell]

- [research/ms] Worked on various aspects of the GHZ test. None of them seem to work fully. Summarized later.

20.2 Wednesday | Dec 30

- [misc] Couldn't get the ticket booked (was waitlisted). Booked new tickets (planned) and packed for Amritsar.

20.3 Thursday - Sunday | Dec 31 - Jan 3 [obligatory vacation | was refreshing though]

- [misc] Went to my cousin sister's. Had a great time. Returned on Sunday morning. Got the bike fixed. Fixed the office (CAF). Helped jelly with the visa things.
- [research/ms] On the way, worked out the minimalistic requirements. I'll find later that I'd made a small mistake and in addition, one can reduce the requirements to obtain a GHZ like +=- paradox.

Part II

Pontification, prolixity or probity | Semester 10

1 Realizing the quixotic

1.1 Monday | Jan 4 [registration + setup]

- [misc] Spent the day on registration (Prashansa helped a lot, even though she didn't have to). Took Prashansa to the counsellor. Saw the doctor. Setup the room (getting it optimized for work efficiency), done partially.
- [seminar] A Nobel Laureate delivered a lecture. Didn't find it all that interesting. He was talking about how we determined the atomic structure of various things. He tried to be accessible to all, but was neither intuitive nor precise.

1.2 Tuesday | Jan 5

- [revision] Gate questions (10 attempted)
- [misc] Schedules updated;
- [research/ms] Was able to make good progress today. So here's what I did. First of all, consider the same states $|\psi_0\rangle, |\psi_1\rangle$ for $N = 8$, as those considered in my first paper. Recall that for $\hat{Z}$ as defined there, viz. $\hat{Z} = Z(\hat{q} \mod 2L)$, or more precisely as $\hat{Z} = Z(\hat{q}) = \text{sgn}(\sin(\hat{q}\pi/L))$, we had $\hat{Z}|\psi_0\rangle = |\psi_0\rangle$ and $\hat{Z}|\psi_1\rangle = -|\psi_1\rangle$. In addition to this, I define $\hat{X} = e^{-i\hat{p}L/\hbar}$ (as opposed to defining it to be hermitian). Now, we know that$|\psi_\pm\rangle \equiv \frac{|\psi_0\rangle + |\psi_1\rangle}{\sqrt{2}}$ is not an eigenstate of $\hat{X}$. We infact used expectation values there and were able to show that for high enough N, everything works out alright. Keeping the operator simple, helps retaining the commutation relations to a reasonable degree and consequently makes it easier to arrive at a violation. Now however, if we construct a more optimized operator, we might just be able to get away with requiring precise commutation relations to hold. So we optimize the observable $\hat{X}$ to $\hat{X}' \equiv \hat{X}\hat{T}$, where $\hat{T} \equiv e^{i\hat{p}NLa(\hat{q})/2}$ where

$$a(q) = \begin{cases} 1 & 2L < q < 4L \\ 0 & \text{else} \end{cases}.$$

The idea is that you shift certain peaks to the right place, before applying the displacement operator $\hat{X}$. To illustrate this, consider explicitly $|\psi_0\rangle = (|\varphi_{-4}\rangle + |\varphi_{-2}\rangle + |\varphi_{-1}\rangle + |\varphi_{-3}\rangle)/\sqrt{4}$. The operation of $\hat{T}$ is $\hat{T}|\varphi_4\rangle = |\varphi_{-5}\rangle$, $\hat{T}|\varphi_3\rangle = |\varphi_{-6}\rangle$ and $\hat{T}|\varphi_n\rangle = |\varphi_n\rangle$ for $n \in \{-4,-3,-2,-1,1,2\}$. It is now evident that $\hat{X}' = \hat{X}\hat{T}|\psi_0\rangle = |\psi_1\rangle$. Note also that $\hat{X}'^{\dagger}|\psi_0\rangle = |\psi_1\rangle$. Similarly $\hat{X}'|\psi_1\rangle = |\psi_0\rangle$ and $\hat{X}'^{\dagger}$ does the same. So finally, now consider $|G\rangle \equiv (|\psi_0\psi_0\psi_0\rangle - |\psi_1\psi_1\psi_1\rangle)/\sqrt{2}$. With $\hat{A} \equiv \hat{X}' \otimes \hat{Y}' \otimes \hat{Y}'^{\dagger}$, where $\hat{Y}' \equiv i\hat{Z}\hat{X}'$, calculations yield $\hat{A}|G\rangle = |G\rangle$. With $\hat{B} \equiv \hat{Y}'^{\dagger} \otimes \hat{X}' \otimes \hat{Y}'$ and $\hat{C} \equiv \hat{Y}' \otimes \hat{Y}'^{\dagger} \otimes \hat{X}'$ also, by symmetry we get $\hat{B}|G\rangle = |G\rangle$ and $\hat{C}|G\rangle = |G\rangle$. Now $\hat{D} \equiv \hat{A}\hat{B}\hat{C} = \hat{X}' \otimes \hat{Y}'\hat{X}\hat{Y}'^{\dagger} \otimes \hat{X}'$ and $\hat{E} \equiv \hat{X}' \otimes \hat{X}' \otimes \hat{X}'$ yield the paradox. If values were predefined, the value of $\hat{D}$ and $\hat{E}$ would return the same answer. However, a simple calculation yields $\hat{D}|G\rangle = |G\rangle$ (this can be seen directly by applying $\hat{A}$, $\hat{B}$ and $\hat{C}$ sequentially on $|G\rangle$ [was figured the next day]), while $\hat{E}|G\rangle = -|G\rangle$.

  - [issues]
    * One needs to construct an experimental setup, viz. figure how to measure the Hermitian versions of $\hat{X}$ etc.
    * Perhaps the whole thing is better constructed using Hermitian observables, but then wouldn't the $Y^{\dagger}Y = 1$ trick fail, causing serious issues?
    * I am messing up the sign somewhere. Write this thing properly on TEX.

- [PhD]

1.3 Wednesday | Jan 6

- [revision] no time
- [research/ms]
  - Issues Resolved
    * Experimental Setup: Not even clear what measuring $\hat{X}'$ really means.
    * As it turns out, even if you construct Hermitian $\hat{X}'$ + h.c., even then its square works out alright, so long as the subspace is restricted to linear combinations of $|\psi_0\rangle$ and $|\psi_1\rangle$. Thus thinking of $\hat{X}'$ might still have some meaning. However it's not as obvious as the $\cos(pL/\hbar)$ and $\exp(ipL/\hbar)$ relation.
    * Fixed the inconsistency in the signs I had yesterday.
  - Writing the thesis:
    * Finished the subsection "introduction" of the "Hamilton Jacobi" subsection.
- [PhD] started filling the application for King's College and Oxford.
- [misc] Manu came; had pizza

1.4 Thursday | Jan 7

- [revision] Sleep catchup (post Manu arrival)
- [research/ms] Figured that even $\hat{H}$ is not all that trivial to measure. For instance, for a free particle, measuring $\hat{p}$ is enough. Now consider a particle in a harmonic oscillator potential and is in an eigenstate of $\hat{H}$. Now ofcourse, the energy of the particle can't be measured by measuring the position and momentum (q,p) unlike in classical mechanics where the very state of the system is defined by q,p and once that's known, every function of it is well defined. This is the very first, almost trivial $f(\hat{q},\hat{p})$ which can be investigated to understand how one can measure such quantities in general. Talked to Bagla sir and he couldn't help. During the Cosmology and Gravitation class, I figured doing the following may help. (1) Take the large N limit and see how with $\hat{X}'$ and $\hat{X}$ both, one gets the same result on the state $|G\rangle$. (2) Consider using expectation values for your result and measure that directly. Write a simulation with just this (for in this case, both constructing the test and making measurements is easy). (3) Define some equivalent sets $\hat{A}, |\psi\rangle$ and $\hat{A}', |\psi'\rangle$ s.t. the result of measuring $\hat{A}, |\psi\rangle$ is equivalent to measuring $\hat{A}', |\psi'\rangle$. Then I came back and got stuck at evaluating dI from I. Also talked to Abhishek sir and he didn't know.
- [PhD] Oxford application (mostly done), wrote to Joe Conlon and he responded also (neutrally).

1.5 Friday | Jan 8

- [PhD] Updated the CV & made minor changes to the Oxford research proposal. Sent it to them.
- [research/ms] Worked on writing the thesis. Noted some inconsistency in the book Holland.
- [revision] (none)

1.6 Saturday & Sunday | Jan 9 and 10

- [misc] Went with Jelly to North Country; she had to buy things for her Switzerland interview
- [PhD] US applications, a large chunk done (including searching)

2 Tempestuous Simulacrum: The docile is deleterious

2.1 Monday | Jan 11

- [revision] (pending)
- [research/ms] Talked to Prof. Arvind, made quick progress. He told me that formally, any "Hermitian" operator, $\hat{Q}$, can be measured by introducing an apparatus and a measurement apparatus with the following interaction Hamiltonian (for a short time) $\hat{H} = -a\hat{Q}\hat{p}_y$. However, I realized that the way I was thinking earlier, was infact a little different. I was asking if there exists some interaction so that a measurement of the particle's position yields the value of the observable $\hat{Q}$ at the earlier time? That I doubt is helpful but anyway. Further, I started and finished reading the measurement section of Bohm's second paper. Figured it was precisely the same as what sir was saying.
- [PhD]: Submitted the Illinois application & Brandeis U application filled and submitted (application fee was waived). US applications done.

2.2 Tuesday | Jan 12

- [revision] (pending):
- [research/ms] Spent quite some time on figuring how to extend the analysis done for $\hat{Q}$ with a discrete spectrum to that with a continuous spectrum, with $\hat{Q}\,|\psi_q\rangle = q\,|\psi_q\rangle$. Finally figured I was making a mistake, the state corresponding to $\phi(y+L)$ is $\int dy\, \phi(y+L)\,|y\rangle$, and not $\int dy\, \psi(y+L)\,|y+L\rangle$ (yes it sounds foolish now). I was able to show that if the "apparatus" is initially at position 0, with spread δy, then after measurement, say one obtained y'. The eigenvalue this will correspond to is $q = (1/ta)(y' \pm 2\delta y)$.
  So this essentially means that $\hat{Q}$ is only required to be Hermitian. Then, once we know the complete set of eigenfunctions, we are through. I therefore tried evaluating the eigenfunctions of $\hat{X}'$. To get some insight, I started with $\hat{X}$ and there itself it got hard. One can write it as $|p\rangle$ or as functions periodic in the position space, with periodicity L. The difficulty is making them equivalent. Showing the latter to be complete anyway won't work for I have constructed only two functions (with eigenvalues ± 1). By the time $\hat{X}'$ is used, even $|p\rangle$ ceases to be an eigenstate and I end up with only the position space constrained functions. So that's a big mess.
  Also realized that the measurement scheme is also essentially just a displacement operator.
- [PhD] Europe lookup started in full swing. Making the list.

2.3 Wednesday | Jan 13

- [research/ms] Talked to Arvind sir again (yesterday's meeting had gotten cancelled). Kichoo finally talked about randomness certification etc. and it looks rather interesting. I think he'll be able to do something new with it. I wanted to see if contextuality could be used instead. Anyway, coming back to my work, I explained all that I had done yesterday to sir. He said that we don't need the full spectrum. If my state can be expressed a linear combination of the eigenstates I've already found, then I'm good to go. I can skip the 'hard' part. He also seemed to have liked the optimization operator $\hat{T}$. Infact, I just realized I told him that I'll explain to him the details of the test (perhaps in the next meeting).
  Further, I just realized that one can infact *measure non-hermitian operators* also. The only requirement will be that (a) the eigenvalue for the some states is real and (b) the state being measured is at most a superposition of these states. This is exciting and I must talk to sir and get this verified. Ofcourse the question is then, what happens when you use the same measurement scheme (that of measuring non-hermitian operators) to measure a state that doesn't satisfy (b)? Looks like that it will fail to satisfy Schrödinger's equation (the time evolution will become non-unitary), viz. probability densities won't be conserved. So for now, I must translate this into a simulator problem. I was wondering however, if I should use the two degrees of freedom of the same particle or should I use two 1-d particles? We'll see. Infact, my preliminary feeling is that simulations aren't exactly needed. I may have to plot some functions, if at all.
- [course/cosmology] Obtained the text. Started reading them.
- [misc] Updated this document for the past few days. Jelly help. Nostalgia triggering pictures.
- [PhD] (failed)
- [revision] (pending)

2.4 Thursday | Jan 14

- [research/ms] Worked out some more eigenstates (infact arbitrary eigenvalued) of the displacement operator. Figured how to construct the relevant state by a straight forward generalization of the GHZ state and my previous work on the Bell Test. I am now confused about whether I should try doing the Bell test first, or if I should try doing the GHZ test. A little exploration reveals that the most interesting (that I can think of) method would be to use the Wigner functions for the operators. In that spirit, the Bell test is almost trivial (applying the unitaries is almost trivial). Thereafter, the wigner functions may be evaluated directly. Infact, this tells me that if there're really trajectories (as described by Bohmian mechanics), then what would be the difference between quantities evaluated as averaged for various bohmian trajectories vs. evaluated using the Wigner function formalism? Why must negative probabilities come into play?
  However, the point for me at the moment is that I must start writing the simulation. For that I need the broadest (in the sense of what the targets that can be achieved) and the simplest requirements.

- [course/cosmology] Figured the spider diagrams. Started reading the introduction chapter. It is hard (not always clear) but rather interesting; the fact that things are the way they are.

- [PhD] Reminder list sent

2.5 Friday | Jan 15

- [course/cosmology] 2 hour lecture. Before and after studied the introduction chapter. Now using highlighting using Okular extensively.

- [research/ms] Working on a presentation to present my work at Phi@I.

- [PhD] Emails related to PhD etc. sent received

2.6 Saturday & Sunday | Jan 16, 17

- [PhD] Finalized the europe list; found LARS and applied for it.

- [misc] Gym located and food processing started; fixed the room (standard cleanliness levels)

- [research/ms] Thought about it at various times. What was troubling me was the following: If the GHZ test infact says that (q,p) can't be non-contextually deterministic, then how can BM, where (q,p) are infact non-contextually deterministic, be consistent with it? There are a few things that need to be understood.
  First, note that if a Bohmian particle has position q_0 and momentum p_0, then the measurement of an 'observable' which is a function of $\hat{q}$, will infact correspond to the same function, with $\hat{q}$ replaced by q_0. Similarly for a function of $\hat{p}$ only. But here comes the subtlety, if one measures an observable which is an arbitrary combination of $\hat{q}$ and $\hat{p}$, eg. $\hat{q}+\hat{p}$ or even $\hat{H}=\hat{p}^2+V(\hat{q})$, then upon measurement, the value will *not* be the same function with q_0 and p_0 in place of the operators. I must find out if values of all observables are determined completely by ψ and the initial positions of the particle. One quick example that can be thought of is that of a Harmonic oscillator; the particle can have essentially arbitrary q,p but will correspond to the same energy, if ψ is an energy eigenket to start with.
  Second, in the GHZ test, the usual structure is that one measures $A=X\otimes Y\otimes Y$, B and C (implicitly defined). The contradiction we get essentially hinges on the claim that if X,Y have well defined values, then ABC should essentially yield a situation identical to measurement of $X\otimes X\otimes X$. However that needn't be. Take for example the non-contextually deterministic theory, BM. Within BM, measuring A, B and C will yield values corresponding to the q,p values of the three particles (by corresponding we mean that $\hat{q}\leftrightarrow q,\hat{p}\leftrightarrow p$). However, a measurement of ABC, will not yield a value that directly corresponds to q,p, viz. it is not a product of values obtained when A, B and C are separately measured.
  Thus in essence, we have shown that it is incorrect to conclude from the GHZ argument that determinism (or precisely, a non-contextually deterministic description of nature) can't exist.
  There are ofcourse various aspects which still need exploration. (a) One possible approach could be to extend the analysis to full blown tests of contextuality, by first appropriately extending them to phase space, and then identifying how BM explains them. This could either be done in the context of quantum optics (for which BM isn't all that popularly known) or for say, the Mermin type argument (the Mermin square, for two spin half particles). (b) The other direction is to construct an appropriate hidden variable model, that is non-contextually deterministic, but still consistent with the usual GHZ test. The latter should give insight into whether or not the dynamics are essential for the analysis, viz. recall how a direct extension of the BM principle to discretized space gets into trouble (the ∇ operator has no analogue); but from the looks of it, a complete analysis of the GHZ phase space test would shed some light on it.
  [written on and developed these to this form, on Monday]

3 Timorous Tests, Ponderous Progress

3.1 Monday | Jan 18

- [research/ms] I have to now, convert the entire GHZ test into a scheme, where only pure functions of the p (or q) are measured. No mixed functions. This means that the scheme I worked out the other day, is essentially useless for the current purposes. Once that's done, then I must work out what measuring $Y^{\dagger}XY$ essentially yields, viz. given ψ and (q_0, p_0) what the measurement outcome should be, deterministically.
  One could also try to see how the Wigner function and BM's $(q_0, p_0) + \psi$ get related eventually.

3.2 Thursday | Jan 21

- [research/ms] Finally discussed things with Arvind sir. Explained to him what I had worked out. The outcome was that (a) I try to extend the Mermin's proof to phase space, (b) Try to simulate the measurement process itself for the GHZ test and (c) glance through the paper sir has provided.
  It isn't still very clear, how we're so confident that 'complicated relation' between p_0 of the particle and measurement of arbitrary functions of $\hat{q}, \hat{p}$ are so easily consistent with QM. Ofcourse, from the calculation, one will get the eigenvalue corresponding to the operator upon measurement, but given p_0 and q_0, precisely which value one obtains, is not a simple exercise to determine. This is where I must use my computational tools. Alternatively, sir suggested I look at this paper where a formulation of QM is presented, wherein q, p do infact yield the appropriate answers, viz. position and momenta are treated symmetrically.
  Also, it would be interesting to see how the classical limit can be obtained, viz. why classically, a measurement of $\hat{p}$ yields p. Infact in this sense, perhaps one can see precisely what contextuality would mean for classical mechanics.
  I was also able to extend the GHZ setup, to that of contextuality (peres mermin) rather trivially infact. Now the argument becomes even more clear. If one imagines that these variables have pre-defined values, then 'ofcourse' $C_3 = 1$ and not -1. The flaw again is that determinism or pre-defined values, don't entail that you simply multiply the result of observables. You must multiply the observables and then ask what is the observed value. This readily extends to even the $\langle \chi_{KS} \rangle$, which BM will happily violate. So the claim that $\langle \chi_{KS} \rangle \leq 4$ for any non-contextual deterministic theory, is wrong; BM is an exception. The reason has been delineated earlier.
  For my satisfaction though, I'd like to also see how BM is compatible with the no-coloring type of proofs of the KS theorem. In addition, I am unclear about how spins can be handled, as in whether spins can be thought of as having predefined value, in the sense just described. But wait, don't they? Since like (q, p) by itself doesn't yield upon measurement p (q maybe it does), similarly, spins also have predefined values, dependent on the (q, p) and thus compatible with determinism. I just found a paper titled "Spin and non-locality in quantum mechanics", where the spin vector has a pre-defined value. Damn it. Now I feel that the description of nature is not unique after all.
  Finding the classical limit, seems like the best way to proceed. What would that mean for the KS inequality? What about Bell's inequality? Also, did non-locality have anything specific to contribute to this argument? Was non-locality necessary? These questions still need to be worked out.

- [PhD] Submitted the application for Cambridge

3.3 Friday | Jan 22

- [research/ms]

3.4 Saturday & Sunday | Jan 23, 24

- [teaching/thermo] Taught these people thermodynamics

- [PhD] applied to Netherlands, Max Planck; wrote for Geneva and Bonn; was working on Notinghum

- [research/ms] Looked at various results that relate contextuality and non-locality. I realized that while Bell's assumptions about the pre-defined output of local hidden variable theories seemed alright, when one makes the same assumption about contextuality, then the measurement process is completely neglected. Thus, one must show that even classically, such an event can happen. That classically also, 'contextuality' happens, should be either shown or explained why it doesn't. And what better theory than BM to do this, which has a clear classical limit; it becomes newton's laws when $\hbar \to 0$.

4 Quandary: A Percipient Fomented, Squelching the reasonable

4.1 Monday | Jan 25

- [research/ms] Trying to work out the details of the classical limit of the measurement process. Quickly realized that it will not be trivial, the interaction Hamiltonian used has a derivative (∇) which might cause issues it seems.

4.2 Tuesday | Jan 26 [Holiday | Independence day]

- [research/ms] Was thinking that perhaps simulating the Bell's test will help clarify if one can have ... [later: always finish what you start writing]

4.3 Wednesday | Jan 27

- [research/ms] It looks like BM is cheating a little. In the sense that it claims that it has a clear classical limit, in which the Hamilton Jacobi framework is recovered. In this limit, it is said that one needn't worry about measuring the observables as being anything special. The observables will yield the right values. However, for quantum mechanical measurements, the interpretation (or strictly, it's author) suggests we use the effective Hamiltonian $H_I = a\hat{p}_1\hat{p}_2$ with $1,2$ labelling the system and the auxiliary particle that will measure the momentum, respectively. If one tries to find the classical limit of this measurement process, with the hope that the measurement of $\hat{p}_1$ will infact yield $p_1 = \nabla_1 S$, one finds a lot of things getting messed up. First of all, this interaction adds a term to the continuity equation:

$$\frac{\partial P}{\partial t} + \nabla_1\left(P\frac{\nabla_1 S}{m}\right) + \nabla_2\left(P\frac{\nabla_2 S}{m}\right) + \overbrace{2aR\left[\nabla_1 R\nabla_2 S + R\nabla_1\nabla_2 S + \nabla_2 R\nabla_1 S\right]}^{\text{Extra Term! } | \text{ How will } P \text{ satisfy the continuity equation?}} = 0$$

The interaction also adds a "quantum potential" in addition to the expected potential, which doesn't seem to disappear in the large mass limit.

$$\frac{\partial S}{\partial t} + \frac{(\nabla_1 S)^2}{2m} + \frac{(\nabla_2 S)^2}{2m} + \overbrace{a\nabla_1 S\nabla_2 S}^{\text{expected part}} - \overbrace{a\hbar^2\frac{\nabla_1\nabla_2 R}{R}}^{\text{quantum part}} - \underbrace{\frac{\hbar}{2mR}(\nabla_1^2 R + \nabla_2^2 R)}_{\text{usual quantum potential}} = 0$$

Thus, BM can't do both, have a clear classical limit and be unable to explain the measurement process's classical limit. Ofcourse all of this is happening primarily because in the usual derivation, the author regarded $V = V(q_1, q_2)$ and not that of p_1, p_2. However, this is very unsatisfactory of a theory that in the classical limit, things cease to make sense. One can yet, see that with $\hbar \to 0$, the quantum potential and the quantum part of the interaction, both disappear. However, the continuity equation is not recovered still. I may have made some calculation mistakes, which I can check later, but at the moment, I must be able to make sense of the notion of measurement in the classical setting, even if this particular method needs to be abandoned; there must however be cogent reasons for doing so.
Further, it is interesting to note that there're velocity dependent forces (non-dicipative) such as Corioli's force that should perhaps result in a momentum dependent potential. In this case, it would be interesting to see how BM trajectories behave. Infact, what about galelian invariance? How does that come in the picture in BM? Looks like that'll be fine; one only needs to change $x \to x - vt$ where v is just a constant. Let's anyway see.

4.4 Thursday | Jan 28

- [research/ms] Started reading a paper by Roy-Singh on symmetric treatment of position and momentum, in a causal qauntum theory. They made a more real bohm theory. Spent some time figuring the meaning of the following puzzle: (The $\partial\dot{x}/\partial x = 0$ or $\neq 0$) and figured the continuity equation more carefully and how its different from Louivelle's equation. The paper took some time to seep in, but was interesting.

4.5 Friday | Jan 29

- [research/ms] Today I had an interesting thought. While I was convinced that there must be something wrong with the following argument, the argument seemed to be sound. The argument would lead to the conclusion that one can communicate faster than light, if Bohmian Mechanics is right. So the idea of BM is that since we don't know the initial position of the particles precisely, we can't quite make predictions stronger than CM. However, consider this.
There are two particles, one with person A, and the other with person B. Now both measure the position of their particles. Thus the positions are precisely known. Next, they allow their particles to evolve under a Hamiltonian (interaction) such that they become entangled. Suppose at time t, the particles are entangled. Now from the de-Broglie theory, both can predict the locations of their particles at t. Now comes the interesting part. The protocol they follow, is as follows. Say A uses this setup, only to receive signals. If B wants to send a signal to A, all that he needs to do, is to disturb his particle at t. This would cause some change in, either the wavefunction or the position, or both, of the particle, compared to if B had not caused any disturbance. Now B measures her particle's position at $t + \epsilon$ and gets the value x_B. She knows what position her particle should've been at, if B didn't do anything, call this position $x_B^{(0)}$. If now $x_B \neq x_B^{(0)}$, then she knows B sent a signal. If $x_B = x_B^{(0)}$, then she knows nothing was sent. Since the equations are non-local, this interaction is instantaneous, and in principle faster than speed of light.
Ofcourse, one can fill in the details, which is what I almost started doing, but finding the fault in the argument is what

was pivotal. It turns out, the fault was rather straight forward, although a little subtle. The idea is that regardless of how precise the position measurement is, its uncertainty will spread with the wavefunction, thereby rendering any prediction useless. Thus, in this light, chaos restores locality. This is actually not too hard to see. Imagine you have an uncertainty δx, when you measured the position and got the value x. Now we can imagine a gaussian associated with this, as the wavefunction, after collapse. Since the de-Broglie Bohm theory ensures that the probability distribution $|\psi|^2$ is preserved by the dynamics of the particles, as ψ evolves, δx will increase (in case of free evolution), effectively destroying 'position information'. The idea can only be harnessed, if one can somehow decouple the uncertainty in x from the wavefunction. One possibility I considered was that of very strong ineractions, where the wavefunction changes dramatically, but ∇S doesn't, so that $\dot{q}$ is small, and thus δq doesn't change much. This idea, I don't suppose will work but can be tried.
Another thing one might want to work out, is how a delta function in position evolves, according to bohmian mechanics.

4.6 Saturday & Sunday | Jan 30, 31

- [PhD] Had a gate exam, wasn't as hard but as I wasn't prepared, not so great.
- [teaching/Thermo] Presented the first chapter in reasonable detail, tried to make things as clear. Made the attendance + feedback sheet.

5 Still myopic? An Epiphany and the apocryphal BM.

5.1 Monday | February 1[Unwell]

- [PhD] Applied to Nottingham and applied for the scholarship for Edinburgh
- [misc.] Shwetha got through MIT!!

5.2 Tuesday | February 2

- [research/ms] Today was rather interesting. I read through the paper by Roy and Singh, which talked about a symmetric treatment of position and momentum in a causal theory of QM. I even started reading the sequal where they show how this maybe extended to n particles. I haven't completely read it. I have the following questions though. First of all, since q, p are now precisely what they'll turn out to be if measured, then that makes a lot of the things I was working on rather simple. I can now go ahead and try to answer my question, viz. what happens to contextuality in the classical limit? I ask this for it looks rather binary to me, unlike the uncertainty principles. Next I can ask, what meaning does parity have, in this new causal mechanics. I feel it would be now great to attempt the GHZ test and the Peres Mermin test. Another thing that surprises me is that how can the probability $|\psi|^2$ be conserved by the particles, if they don't follow the de-broglie equation, which was just the probability current? I would like to simulate both and see how they differ. Ofcourse, various details of the theory need to be worked out, for the functions required, don't seem so straight forward for being numerically solved. I also think that using the optimized variables I had defined, to further extend the Bell test would be rather interesting. It is worth noting that the issue with the classical limit of measuring momentum disappears here, as the question doesn't arise, although I would like to see the same happen with BM.
On the larger scale, until the QM & BM stage, I was convinced that BM is perhaps a more detailed description, which when averaged, yields the right QM description. However, I am now confused again. With the RS (Roy Singh) description, I am unclear about which description is correct, or to be precise, I am unclear about the notion that nature has a unique description. Which seems to suggest that we have hit a stage where in the accuracy of our description of nature, can only be tested to a certain level (and its not about lack of technology), beyond which, atleast at the moment, it appears it is impossible to tell which is right. Looks like a rather important lesson for Quantum Gravity.
Another thing I thought of, was that of constructing a test which didn't have the shortcomings of the Bell & Contextuality test, wherein the possibility of change happening due to measurements, is not considered. This earlier was rather important because of two reasons. (1) In BM itself, the value of p depends on the measurement but more importantly (2) that measuring A and getting a value a, and similarly for B getting a value b doesn't entail that measuring AB would yield the value ab. The idea I was raising was that while Bell's test is robust under allowing 'measurements' to modify values, tests of Contextuality are not.
Ok so something is still wrong. I thought somebody had proven that one can't have all possible sets of commuting variables, have a probability distribution, due to 'contextuality'. But if what I understood is right, then according to BM, all outcomes are deterministic. So where's the catch? BM just sods it off by saying that the initial conditions include the initial position/momentum of the particle and therefore it is not included the type of theories considered by 'contextuality' like proofs. I then ask, that in the RS theory, clearly momentum doesn't require anymore knowledgeof the particle, than does a position measurement (both of them don't that is). Thus, it is natural to ask, what properties of the system, can be assumed to have a value, independent of measurement (I don't mean lack of determinism, I mean it doesn't depend on the initial conditions of the apparatus)? Can we construct a theory, wherein all observables have well defined values, regardless? I know it won't conflict with, atleast the Peres Mermin type arguments, simply because there we run into the

AB type of confusion. Antithetically though, the RS paper seemed to claim that such a description may not be possible. One more question that can be addressed is, how to construct an 'almost' $SU(N)$ algebra, out of p, q. We have already shown how to construct the $SU(2)$ structure (which was also already known). This is interesting, for then one can use contextuality in a single particle. This would then strongly challenge the view (proof?) that non-locality is a special case of contextuality. If I can simply construct a contextuality test in phase space, using a single particle, then I think I'd be done. But interestingly, I now recall, Ali had done something similar. He used, instead of displacement operators in one direction and the other direction specifically, a combination of displacements in the two directions and used the geometric phase to his advantage. So even though his operators were those used for quantum optics (for quadrature) and not quite position and momentum, the analogues to exist. Which means that at once, it can be shown that contextuality and non-locality are different.
Now, non-locality as we showed earlier, can be shown to disappear (atleast partly), using chaos. However, how does one show Bell like non-locality? Or if one can show why we must have a $2\sqrt{2}$ as maximum violation.

5.3 Wednesday | February 3

- [research/ms] I realized today that the contextuality test is constructed in a slightly more interesting way. They don't just say that $AB \to ab$ if $A \to a$ and $B \to b$. They say that if $[A, B] = 0$, then $f(A, B) \to f(a, b)$ if $A \to a$ and $B \to b$. So I basically wanted to go against this. Obviously, if one starts with a system in the state $|a, b\rangle$, then it will follow. However, it is possible to find an eigenstate of AB, which is neither an eigenstate of A nor an eigenstate of B. This calculation was done (fine, using online mathematica like stuff) on $A = \sigma_x \otimes \sigma_y$ and $B = \sigma_y \otimes \sigma_x$ and $AB = -\sigma_z \otimes \sigma_z$. Obviously, $|00\rangle$ is not an eigenstate of A (nor of B). Thus measuring $|00\rangle$ will always correspond to the same value, 1, however my theory can assign A and B to $\pm$. Thereby violating the condition that $AB \to ab$. Ofcourse these guys don't even consider issues related to disturbing the system after measurement, but that maybe partly justified as all A, B, AB commute. This was an interesting realization and result, as far as my understanding is concerned. TODO: write the details.

- [misc] Got shortlisted for the TIFR interview.

5.4 Thursday | February 4

- [research/ms] Missed a meeting with sir. Explained all that I had thought of, to Bhati.

5.5 Friday | February 5

- [research/ms] Finally after a lot of thinking, I figured the best thing to do, is to 'correct' the contextuality test. This should be the focus for a few weeks and then whatever it is that I am able to find, I report.

- [teaching/thermo] Studied the second chapter from Callen and looked at all the tutorial problems.

5.6 Saturday | February 6

- [research/ms] Started writing the ideas so far, into a draft, for self clarity and ease of communication.

- [teaching/thermo] Had a good session, as in people mostly rated it 5/5.

5.7 Sunday | February 7

- [research/ms] One possibility is to construct a test (to test memory, which is 'required' for any contextual theory) where, alright. First imagine that we have commuting observables given by $[A_{ij}, A_{ik}] = 0$ and $[A_{ji}, A_{ki}] = 0$. So for a non-invasive system that remembers what I measured before, I measure these in the said order A_{11}, A_{12}, A_{13} and then take another identical copy, measure A_{33}, A_{23}, A_{13} and compare the value of A_{13}. If the value of A_{13} is different, then I know that my non-invasive system is contextual. Note that non-invasiveness is important, else we can't say if the system is contextual or if post measurement, the system obtains a new assignment. It will be more tricky to construct such tests for invasive systems.
Ofcourse one can generalize this to finding if assignment of A_{ij} is contextual by measuring it in the two different contexts, and to varying amounts.
To find values which don't have any context, we may choose to measure along parallel diagonals.
To get some handle on non-invasive systems, let's imagine that if say a measurement is *unfaithful*, (viz. if a repeated measurement of some observable A_{11}, on identically prepared system, yields different values, with certain probabilities) then we can imagine certain hidden variables, which are not in our control, are causing the variance. As an approximation, we assume that whenever these measurements yield the same result, the system has the same underlying hidden variables. It may happen that after measuring one *unfaithful* operator on two systems, we find the hidden variables are approximately the same, then upon subsequent measurement of the next *unfaithful* operator, one might discover that the value obtained for the two systems is different. In this case, one learns that the system didn't infact have the same hidden variables. If

one does obtain the same value, then again one continues to assume that the underlying hidden variables are the same. This construction maybe completely irrelevant though, since we would like to compare systems with identical hidden variables to start with, and then make *different* measurements.
To make some progress, I can imagine a black box, which takes in two states, and tells me if they are identical (including the hidden variables). I don't still learn what those hidden variables are, just that two systems are identical. (TODO: ensure this is correct). Ofcourse, in the final test, I don't want invokation to any such box, but to start, let's see if I can proceed assuming such a box exists.
It turns out, there're various smaller, more tractable issues that need resolution. (a) Consider a theory that assigns values to the peres mermin observables. Now upon each measurement, A_{33} is assigned a new value ($1 \to -1$ and $-1 \to 1$). | Now is (a) contextual? Is it multiplicative? [Our target is to construct by hand, a non-contextual hidden variable theory, that can violate the KCBS inequality.] (a) is clearly not multiplicative. If I measure $A_{11}A_{33}$ in a single shot, I will get an answer which will be different from measuring A_{11} and then measuring A_{33} and taking the product $a_{11}a_{33}$. Infact, my theory doesn't explicitly tell me the assignment for $A_{11}A_{33}$. [What happened to measuring compatible observables together? What role does that play? Will contextuality handle that aspect?]
One approach I can follow is, to take the most obvious, non-contextual multiplicative hidden variable assignment and then make a small change in that (like I just did). Then investigate which property has changed, among non-contextuality and multiplicativity. Note that in doing so, definitions of contextuality and multiplicativity will need to be refined. So start with some definitions, and as you refine, show why the alternatives don't make enough sense (or how they trivialize things). Proceed iteratively, to an assignment, that is non-contextual and yet violates the KCBS inequality. If you're able to retain multiplicativity, great. If not, that's also alright.
Also, one must add a boolean property, *efficacy*. This would be true, if the model is s.t. sequential measurement of commuting observables, behave as faithful observables, viz. their values don't change.
I infact realized, that one might think that multiplicativity and commutativity are the same thing. But surprisingly, this is not so. It looks like, in our analysis, we'll end up showing that a violation of the KCBS inequality implies that nature is either contextual, or non-multiplicative or both. From the perspective of BM, nature is non-contextual, but multiplicative. Perhaps in other HV models of QM, nature can be made multplicative but contextual. However, we can't have a HV model that is non-contextual and multiplicative, which consistently describes nature.
The outstanding challenge now is, first of all, write all of these things systematically with proper examples constructed (as suggested in the approach above). And second, try to construct a new test, which can distinguish between multiplicativity and contextuality.
I have a feeling that all of this has something to do with the maximal set of commuting operators. This property is rather strange.

6 Snide Analysis; Making contextuality flippant

6.1 Monday | February 8 [exam]

- [course/cosmology] Had Bag's exam today.

6.2 Tuesday | February 9

- [research/ms] Figured two interesting things today. First, one can start with assuming that theories are non-invasive, which means that they admit measurement schemes which are non-invasive; viz. measurements can be made without disturbing the system. With this, one can construct two sets of theories. Each has only one non-classical feature, one is contextual and the other non-multiplicative. With these, one can construct the non-imaginative, but standard (that's why non-imaginative) Peres Mermin situation. Describe the two variants of the KCBS test. Variant one will be violated by both, contextual and non-multiplicative theories. However, variant two will be violated only by the contextual theory.
The other interesting thing I figured, was that while a measurement of $\hat{A}+\hat{B}$ doesn't mean that the result will be $a+b$, (where a is the value obtained upon measurement of $\hat{A}$ separately, and b is that of $\hat{B}$), however, according to QM, $\left\langle \hat{A}+\hat{B}\right\rangle = \left\langle \hat{A}\right\rangle + \left\langle \hat{B}\right\rangle$. Thus, on an average, measurement of $\hat{A}+\hat{B}$ will be the same as measuring $\hat{A}$ and $\hat{B}$ separately, average separately, and add. Note how the experiments are entirely different. This may prove pivotal in two ways. First, this shows how QM distinguishes between addition and multiplication of observables, quite explicitly. Second, this will help in extending the aforesaid scenario, even when we give up non-invasiveness. Then we will be able to show if QM is truly contextual or not. Ofcourse, one also needs to see this in the light of the assumption that $[\hat{A},\hat{B}]=0$.
Further, I need to see if the *multiplicity* argument holds for addition also, viz. for $[\hat{A},\hat{B}]=0$, if $\hat{A}\to a$, $\hat{B}\to b$, then does require $\hat{A}+\hat{B}\to a+b$? This is certainly required on an average, viz. $\left\langle \hat{A}+\hat{B}\right\rangle = \left\langle \hat{A}\right\rangle + \left\langle \hat{B}\right\rangle$, unlike multiplication, $\left\langle \hat{A}\hat{B}\right\rangle \neq \left\langle \hat{A}\right\rangle\left\langle \hat{B}\right\rangle$. The commutativity argument doesn't figure here, since we're talking about compatible observables.

- [PhD/JEST] Tried to do some questions, revised some bit of the keppler problem. Perhaps remembering the solution will help ($r(\theta)$ for a given U and various initial conditions)

6.3 Wednesday | February 10

- [research/ms] Finished writing the main ideas in the draft. They look interesting. So the final argument goes as follows. A multiplicative theory will yield $\langle \chi_{\rm KCBS}\rangle = \langle \chi_2\rangle$. For simplistic 'deterministic' theories, $\langle \chi_{\rm KCBS}\rangle = \langle \chi_2\rangle \leq 4$. A theory that violates the KCBS but not χ_2, is certainly non-classical in some way, but must be non-multiplicative. As we have demonstrated (to do), for QM, $\langle \chi_{\rm KCBS}\rangle \neq \langle \chi_2\rangle$ in general, thus predictions of QM can't be explained by any theory that is multiplicative. More specifically, we have ruled out contextual theories, which are multiplicative, as candidates for describing the quantum world. These theories are usually believed to be the appropriate alternative candidates to simplistic 'deterministic' theories. *In fact, one can remove contextuality completely from the picture;* there exist formulations of quantum mechanics in terms of hidden variables, which are non-contextual and non-multiplicative. They are consistent with all predictions of QM, including violation of the KCBS inequality.
I need to work on the following.
(1) Not that the progress so far is particularly depressing, but it would be great if I could somehow construct a true test of contextuality. Also, I would imagine that one can construct a better test of multiplicativity. I think I should try to focus on this independent of contextuality. On specific states, it is easy to show. I must aim at constructing a state independent test. But as usual, let me start with state dependent ones and see what progress I can make.
(2) I should either show this analytically, or numerically, that $\langle \chi_{\rm KCBS}\rangle \neq \langle \chi_2\rangle$. Infact, if I can show this for even one state, it is enough to show that QM is not multiplicative.
- [PhD/JEST + Misc] Glanced through a few questions; nothing that serious; got an interview call from ICTS also.

6.4 Thursday | February 11

- [research/ms] Constructed an incorrect 'proof' of contextuality in 2 dimensions! Was convinced it is correct to start with and then found the flaw. Started working on a test of multiplicativity, independent of that of contextuality. Initially I felt I was just trying to come up with some operator identity that numbers won't satisfy. This again became an orthodox contextuality like test. I realized later that the test for multiplicativity is actually quite different in spirit. So I started with constructing a state dependent test. In general, one can write the simplest test as follows. Consider two observables A, B, s.t. $[A, B] = 0$. For a multiplicative theory, $m(AB) - m(A)m(B) = 0$. This also entails that $\langle m(AB)\rangle - \langle m(A)m(B)\rangle = 0$. If I can show that there exists some state for which $\langle m_1(A)m_2(B)\rangle - \langle m(AB)\rangle \neq 0$, then I'm through. Ofcourse, it is also possible that on an average, the equality holds, but there exist special events where it is violated. One can consider those, but a more robust and experiment friendly method would be to find some averaged expression.
Some possible directions for explicit examples include: Choose a, b s.t. $e^{i\hat{p}a}e^{i\hat{q}b} = e^{i\hat{q}b}e^{i\hat{p}a}$, basically so that for $\hat{A} = e^{i\hat{p}a}$, $\hat{B} = e^{i\hat{q}b}$, $[\hat{A}, \hat{B}] = 0$. Now, one needs to cook up the right state to see if a violation can be arrived at. Further, one can use the $|11\rangle$ state, with $A = \sigma_x \otimes \sigma_y$ and $B = \sigma_y \otimes \sigma_x$. This would be a 2 qubit example. Finally, for a two qubit case, perhaps using $P_1 = |0\rangle\langle 0|$, and $P_2 = |1\rangle\langle 1|$ on a superposition $(|0\rangle + |1\rangle)/\sqrt{2}$ would make most sense.
I also thought of how this maybe extended to addition. For instance, measurement of $O = A + B$, will always be s.t. $\langle O\rangle = \langle A\rangle + \langle B\rangle$, according to QM. According to a multiplicative theory, $m(O) = m_1(A) + m_2(B)$, where the order is not important. Upon averaging, we will get $\langle m(O)\rangle = \langle m(A) + m(B)\rangle = \langle m(A)\rangle + \langle m(B)\rangle$. Consequently, I will see no violation, on an average. A suggested modification is to define $C := AB$, so that $O = CB^{-1} + A^{-1}C$. Now perhaps a sequential can be done on the latter two and a violation arrived at. This however, seems to not be as neat as I'd like (even if it works that is).
[research/ms feedback] Kishor and Bhati both looked at the paper and couldn't quite find any major mistake (kichoo found a typo though).
- [PhD/misc] prepared for JEST in the morning, booked flight tickets for TIFR etc.

6.5 Friday | February 12

- [research/ms] New word for multiplicative. Absolutely compatible; observables maybe compatible, but can't be absolutely compatible; weakly compatible := ! absolutely compatible. Inert measure, Multimeasure (veto), progressively measurable, completely compatible, incompletely compatible, partially compatible. A theory in which all compatible observables are completely compatible, is itself called completely compatible. A theory in which even one compatible observable is not completely compatible, is called partially compatible. Any theory that is not completely compatible, is equivalent to being partially compatible.
Also thought about and realised why I wanted a different test of multiplicativity and why the χ_2 test was useful. The χ_2 test was useful, because it worked very well with the theories I was discussing (the two extremes). Now I want a test that is violated by QM. For that I want it to be as simple as possible.
- [PhD/misc] Booked the last flight ticket for coming home from ICTS.
- [misc/seminar] Bhati's talk on some sort of reduction model. Couldn't understand things in too much detail, but that's ok.
- [misc] Got things printed for tomorrow's session

6.6 Saturday and Sunday | February 13 & 14

- [teaching/Thermo] Went reasonably well. Still don't know how to solve that last question (that dropped the problem solving rating):
- [PhD/JEST] made some progress (almost finished the sample questions)

7 Incumbent on the wise, to be Inimical to speculation

7.1 Monday | February 15

- [research/ms] Updating the summary document. I am now trying to explicitly construct an explicit test for multiplicativity in 4 dimensions. Some useful commands include going to Wolfram Alpha and then

```
Eigenvectors[ {{0,0,0,-i},{0,0,-i,0},{0,i,0,0},{i,0,0,0}} ]
Eigenvectors[ {{0,0,0,-i},{0,0,i,0},{0,-i,0,0},{i,0,0,0}} ]
```

  What can be said about non-locality, it's relation to contextuality; what about Bell's test? Does that have anything to do with multiplicativity? What about all the beautiful properties such as monogamy associated with the Bell test and Contextuality? Where do these things stand in this new perspective? I have a feeling that something rather interesting is in store for us.
  After trying to construct an explicit test for multiplicativity, I realized that QM doesn't infact violate it. In essence, consider $A = \sigma_x \otimes \sigma_y$, $B = \sigma_y \otimes \sigma_x$. Now one can verify that $|[++]\rangle = (-i\,|00\rangle + |11\rangle)/\sqrt{2}$, $|[+-]\rangle = (-i\,|01\rangle + |10\rangle)/\sqrt{2}$, $|[-+]\rangle = (i\,|01\rangle + |10\rangle)/\sqrt{2}$ and $|[--]\rangle = (i\,|00\rangle + |11\rangle)/\sqrt{2}$ are eigenkets of A and B with eigenvalues $\pm, \pm$ as used in the nomenclature. Now for the state, $|\psi\rangle = |11\rangle$, we know that a measurement of $AB = \sigma_z \otimes \sigma_z$ would yield the value $+1$. Infact, $AB\,|11\rangle = |11\rangle$. However, since $|11\rangle$ is neither an eigenket of A, nor an eigenket of B, a measurement of either will collapse the state to some other state. A measurement of AB would leave the state invariant. This seemed to suggest that perhaps, this setup can be used to establish non-multiplicativity of QM. That isn't however the case. This is so because $|11\rangle = (|[++]\rangle + |[--]\rangle)/\sqrt{2}$. Now if AB is measured, we're guaranteed to get $+1$. However, if we measure A first, then we'd get either $+1$ or -1. Say we got $+1$. The state would be $|[++]\rangle$ and thus measuring B would always yield $+1$. Similarly, a measurement for -1 and similarly for measuring B first. Thus, even though the physical situations are completely different, $m(AB) = m(A)m(B)$. This completely undermines the argument I was trying to build against contextuality. Looks like QM is afterall multiplicative, although I haven't exactly proven this.
- [PhD/JEST] did some more questions (was thinking about this L^2 vs writing the KE using ∇^2. Should revise sakurai.)

7.2 Tuesday | February 16

- [research/ms] There're atleast two interesting directions to pursue at this stage. One is that I try to see what exactly happens for A, B, C s.t. $[A, B] = 0$ and $[A, C] = 0$ but $[B, C] \neq 0$. The other is that I try to construct the simulator for BM and see precisely how these values change upon being measured.
  The latter will take quite some time and be useful as a tool in general. I also realized that if one is allowed to update values, then one can violate the KCBS inequality, without caring for which compatible observable was measured before/with. So then in my old notation, non-multiplicativity will arise in the sense that $m_1(A)m_1(B) \neq m(AB)$ in general. However, $m_1(A)m_2(B) = m(AB)$. This I suspect will also hold for BM.
  From tomorrow, I will start working on the simulator. I want that table. Then I'll construct the theory. Should have 'experimental' results before attempting construction of a theory (don't have very solid principles guiding my theory now do I?).
  For the moment, consider the situation where we start with

$$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$$

  and we measure A_{33}. The system will yield the value $m_1(A_{33}) = 1$. Now the system checks if it is consistent with $C_3 = -1$ and $R_3 = 1$, corresponding ofcourse to A_{33}. It updates the values to be consistent, say to

$$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & -1 \\ 1 & 1 & 1 \end{bmatrix}.$$

  Now if I measure say A_{23}, I will get $m_2(A_{23}) = -1$. The system checks if $C_3 = -1$, which is true and if $R_2 = 1$, which isn't, and thus, updates the system state to

$$\begin{bmatrix} 1 & 1 & 1 \\ -1 & -1 & -1 \\ 1 & 1 & 1 \end{bmatrix}.$$

Finally, if A_{13} is measured, I get $m_3(A_{13}) = 1$. The requirements $C_3 = -1$ and $R_1 = 1$ are satisfied, so no changes are required. Ofcourse, $m_1(A_{33})m_2(A_{23})m_3(A_{13}) = -1$ as required. Note that in principle, $m_1(A_{33})m_1(A_{23})m_1(A_{13}) = 1 \neq -1$. Thus the theory is not 'counterfactually multiplicative' but is 'sequentially multiplicative'. *Since the former can't quite be 'tested', we must conclude that Bohmian Mechanics is atmost as speculative, as is contextuality.*
It is also true, that the memory model was not 'counterfactually multiplicative' but was 'sequentially multiplicative'. The point is that beyond this stage, since both models require the possibility of updating the internal state after a measurement is made, it is simpler to just update all commuting variables, rather than making the model 'contextual'. Contextuality in this light, is not a necessity, but merely a possibility.
Further, one can almost show that QM is multiplicative. Consider two operators $\hat{A} = \sigma_x \otimes \sigma_y$ and $\hat{B} = \sigma_y \otimes \sigma_x$. Let us assume that $\exists$ a $|\psi\rangle$ s.t. $AB|\psi\rangle = \lambda|\psi\rangle$. For simplicity, assume that $\lambda = 1$. We now show that the most general $|\psi\rangle$ can be written as a superposition of those simultaneous eigenkets of A and B, that satisfy $ab = \lambda = 1$. Let us start with $|\psi\rangle = \alpha|[00]\rangle + \beta|[01]\rangle + \gamma|[10]\rangle + \delta|[11]\rangle$, where $|[**]\rangle$ represent eigenkets of A and B; $A|[01]\rangle = |[01]\rangle$ and $B|[10]\rangle = 1|[10]\rangle$ for example. Now $\hat{A}|\psi\rangle = \alpha|[00]\rangle + \beta|[01]\rangle - \gamma|[10]\rangle - \delta|[11]\rangle$, and $\hat{B}\hat{A}|\psi\rangle = \alpha|[00]\rangle - \beta|[01]\rangle - \gamma|[10]\rangle + \delta|[11]\rangle$. Comparing this to $|\psi\rangle$, we get $\alpha = \alpha$, $\beta = -\beta$, ... Consequently, $\beta, \gamma = 0$. This proves that the most general $|\psi\rangle$ can be written as a superposition of kets s.t. $ab = 1$. Multiplicativity directly follows from this, as any measurement of A or B individually, will collapse the state into one of the simultaneous eigenkets (or their superposition, depending on the degenerecy).
For the most general proof though, some arguments are missing, which are being worked on.

- [PhD/JEST] Revised the black body radiation part from Kardar, looked at the rigid rotor and finally, worked out some simple problem about kinematics. Also looked at some more 1 mark questions; recalled the radioactive decay law.

7.3 Wednesday | February 17

- [research/ms] Wrote down the progress I made yesterday. This I had thought of in the past few days and had confused myself over it. If say A, B, C are s.t. they commute, and $ABC = 1$, like the PM setup, then in the measurement process, wouldn't $e^{i\hat{A}\hat{B}\hat{C}.\hat{p}_2} = e^{-i\hat{p}_2} = e^{i\hat{A}\hat{p}_2}e^{i\hat{B}\hat{p}_2}e^{i\hat{C}\hat{p}_2}$. Working now on figuring out how to write the simulator. I think it will be more clear if I implement the 'theoretical measurement' scheme, as opposed to working on the magnetic field gradient based approach.
  It looks like it maybe worth studying Page 158, Spins of Bohmian Mechanics by Detlef. Understood how in BM, one can't ascribe spin to a particle. The argument is simple but it entails that given identical initial conditions (including the hidden variable, viz position), the outcome of a measurement is not fixed. One must also feed the experimental setup. For example, if one reverses the magnetic field gradient, the spin of the particle would turn out to be $-1/2$ instead of $+1/2$ for the exact same initial condition. See the book for details.
  The interesting part for my work was that one can use simple arguments, like the ones used in the book, to measure σ_x and σ_y. Measuring σ_z however is a little complicated. Either one must turn the particle (which complicates the shape of the gaussian packet, viz. it must be analyzed carefully) or think of the particle beam splitting, along its very direction of propagation; neither are neat.

- [PhD/JEST] Some relativity questions, solved that black body radiation question, and did certain other questions

7.4 Thursday | February 18

- [research/ms] Took an interesting step forward today. Extended the GHZ test itself, to a full blown test of contextuality. The details, I'll type later, but see [2] for a glimpse. The advantage is that I need only measure σ_x and σ_y (no σ_z), so that simplifies things a lot. In addition, it looks like we (fine I) were (was) spot on about the kind of theories. We will now be able to show how BM is 'onotologically non-multiplicative' but non-contextual. However, it is 'sequentially multiplicative', as required by QM (which we proved a few days ago). So in essence, we would've clarified that while 'ontologial multiplicativity' is as much a philosophical speculation (since it can't be tested at present) as is contextuality. Confusing different measurement settings that yield different answers for the same initial conditions, doesn't explain 'contextuality'.

- [PhD/JEST] did an interesting mechanics question, revised some stat mech for a question, some probability etc.

7.5 Friday | February 19

- [research/ms] Thought about how one can even do generalized measurements, and yield analytic results by assuming that the measuring particle is at a particular location. The idea is infact quite simple. The wavefunction of the measuring particle gets split, depending the state of the 'system'. After splitting of this wavefunction, in which constituent 'wavefunction' the measuring particle will sit, will depend, by the same token as the Stern Gerlach analysis, on the initial position of the measuring particle. Effectively then, if the measuring particle is assumed to be at the top most position, it would follow that the eigenstate (of the observable that we intend to measure) with the most negative eigenvalue, will be the outcome. This did however, get me thinking about how exactly the 'collapse' is explained in BM. (This was thought of while having lunch, bathing etc. Not exactly while in the office)

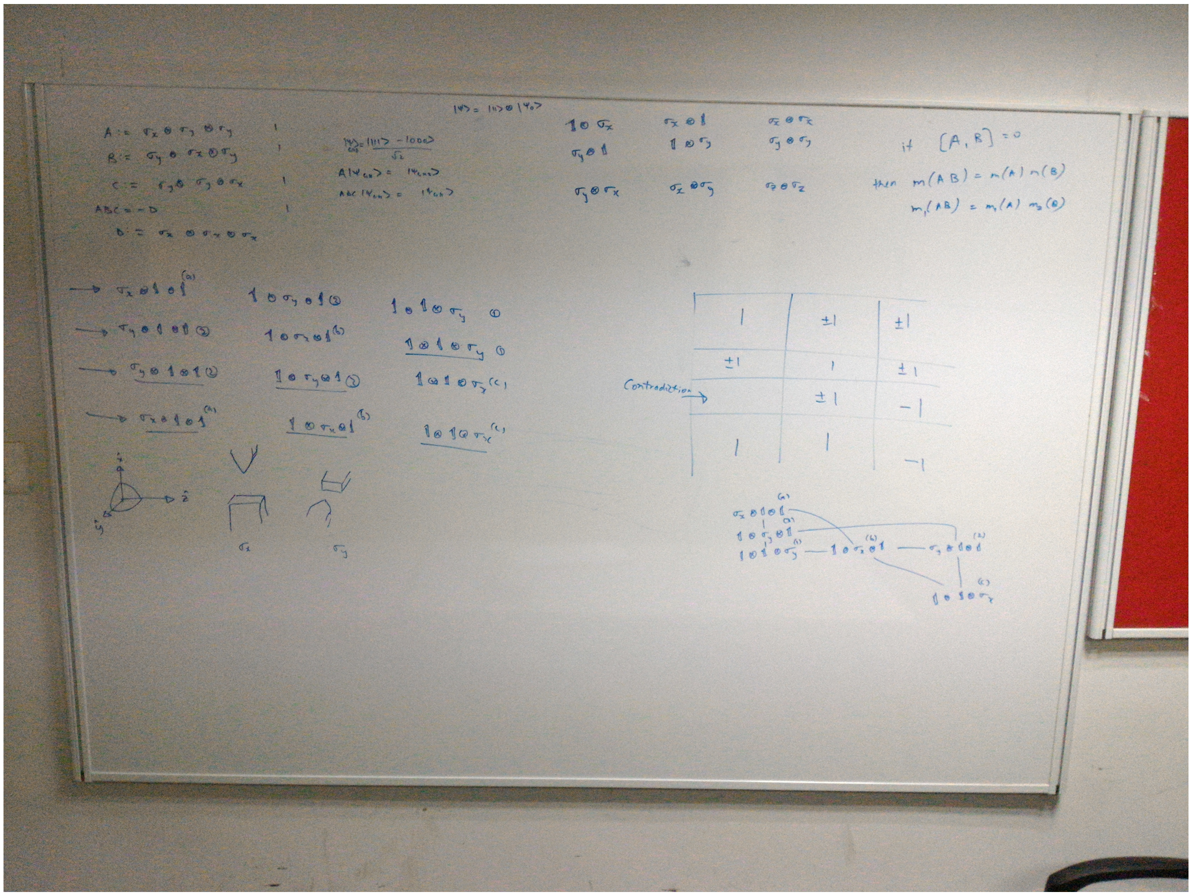


Figure 2: GHZ to contextuality

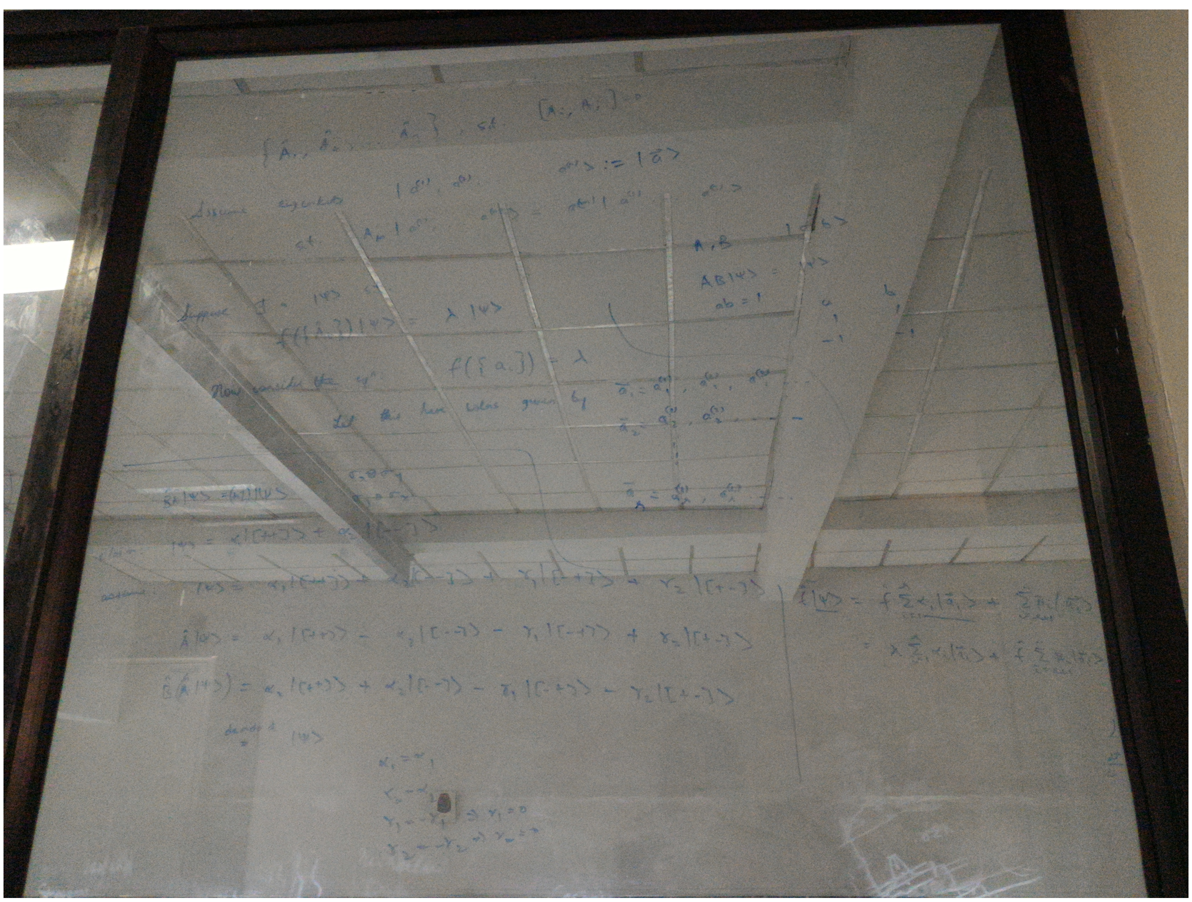


Figure 3: trying to prove QM is sequentially multiplicative

- [PhD/JEST] did the usual 5 questions

7.6 Saturday & Sunday | February 20 & 21

- [teaching/Thermo] Did a part of the 3rd chapter.
- [PhD/JEST] Had the primary focus on it. The exam happened. Didn't do too many questions; tried to be very accurate.
- [misc] Had fun at Decathlon (North country mall).

8 Prosaically Bromide or Appositely Novel?

8.1 Monday | February 22

- [research/ms] Updated records for the past couple of days. Took longer than expected. Just started work on calculating the assignments using BM, without simulations. It turns out that this is harder than I expected.
- [misc] Worked on organizing various things; figured how to do various things

8.2 Tuesday | February 23

- [research/ms] Worked out the BM analysis for the GHZ extension. Was running into issues with simultaneous measurements with the GHZ analysis. Sequential analysis was however working as expected, although I can't be too sure.
- [PhD/interview] Started reading Peskin, the first few pages (ones I never read during the course!) It was interesting to know that 'naturally' vectors are $x^\mu = (x^0, \vec{x})$ while 'naturally' derivatives go, $\partial_\mu = \partial/\partial x^\mu = (\partial/\partial x^0, \vec{\nabla})$.

8.3 Wednesday | February 24

- [research/ms] Realized that in the multiparticle case, the technique will be an issue. I am not entirely sure what's wrong with the argument, but a simple extension is clearly wrong. The velocities will depend on the position of other particles also, which I'm ignoring, if I use the single particle method.
- [course/cosmology] Working out various details from the lecture notes. Also realized that, what I had interestingly, naively written as $R = ar \to V = \dot{a}r + a\cancelto{v}{\dot{r}}$ was infact correct, if one looks at it from the 'total derivative' perspective, as given in Arris.

8.4 Thursday | February 25

- [research/ms] Met with Arvind sir. He suggested I try constructing an explicit model that resolves the issue. I realized that there's a lot that needs to be added to the draft. I will attempt doing that soon enough. It was concluded that we arrive at an explicit assignment using BM, s.t. the theory is non-multiplicative ('ontologically') but is sequentially multiplicative; in addition the theory is non-contextual and finally combine all previous ideas to show that contextuality is speculative and that any true demonstration of contextuality, will render BM false.
  After thinking about how to find answers without simulating, I finally realized that one can simply plot the full ψ in configuration space and then impose the idea that if $|\psi|^2$ describes the distribution of the particles, at some initial time t_0, then $|\psi|^2$ will do so, for all t. For instance, consider the .. [todo: write this]
- [course/cosmology] spent a lot of time, finishing the portions that had been covered last week. Also glanced through what was covered this semester.

8.5 Friday | February 26

- [research/ms] explained the BM analysis to prashansa. Realized how for even two dimensions, one is expected to get a circle (in between I confused it to be a square!)
- [course/cosmology] Preparation spilt to Friday also. Had the weekly test. Went well. The class was about random walks. That was also interesting.
- [PhD] did various tasks associated with it.

9 Hiatus | Interviews, irascible or euphoric?

9.1 February 28 - March 8 [PhD/interview]

- [research/ms] Figured how to prove sequential multiplicativity in QM.
  Let $m_i(\hat{O})$ refer to the outcome of measuring an operator $\hat{O}$, where i parametrizes the sequence of the measurement. Sequential multiplicativity is the statement that $m(\hat{P}) = m_1(\hat{P}_1)m_2(\hat{P}_2) = m_1(\hat{P}_2)m_2(\hat{P}_1)$. The proof is simple. Consider a set of compatible operators, which are also complete. For simplicity, let's say the set is given by $S = \{\hat{P}_1, \hat{P}_2\}$ (not anti-commutation, its a set). Now consider the operator $\hat{P} = \hat{P}_1 + \hat{P}_2$. Let $|\psi\rangle$ be s.t. $\hat{P}\,|\psi\rangle = p\,|\psi\rangle$ but $\nexists$ any λ s.t. $\hat{P}_i\,|\psi\rangle = \lambda_i\,|\psi\rangle$ (else the result we're trying to prove will be trivial; as $|\psi\rangle$ is a simultaneous eigenket of P_1, P_2 and P, with eigenvalues p_1, p_2 and p_1p_2). If you're uncomfortable with this 'claim' or its relevance, one can use $P_1 = \sigma_x \otimes \sigma_y$, $P_2 = \sigma_y \otimes \sigma_x$ so that $P = \sigma_z \otimes \sigma_z$. Now, the state $|00\rangle$ makes for a non-trivial $|\psi\rangle$. Coming to the proof, first we define $\hat{P}_i\,|p_1, p_2\rangle = p_i\,|p_1, p_2\rangle$note that it suffices for me to show that $|\psi\rangle$ must be a superposition of only those simultaneous eigenkets $|p_1, p_2\rangle$ s.t. $p_1p_2 = p$. This can be proved as follows. Consider

$$\begin{aligned} \hat{P}\,|\psi\rangle &= p\,|\psi\rangle \\ \implies \langle p_1, p_2|\,\hat{P}\,|\psi\rangle = p_1p_2\,\langle p_1, p_2|\psi\rangle &= p\,\langle p_1, p_2|\psi\rangle\,. \end{aligned}$$

  It follows immediately, that if $\langle p_1, p_2|\psi\rangle \neq 0$, then it entails that $|\psi\rangle$ contains $|p_1, p_2\rangle$ as an eigenket in its superposition, and $p_1p_2 = p$. However, if $\langle p_1, p_2|\psi\rangle = 0$, then we won't ever see p_1 or p_2 as outcomes, so it is irrelevant. That then completes the proof.
  This can be readily generalized to any number of operators, even if they're not complete (in which case the remaining operators maybe added to complete the set, but the eigenvalues corresponding to them, don't play any role in the proof, and thus making its extension trivial).

10 Surfeit of Irking issues, still not irresolute

10.1 Monday - Wednesday | March 14-16

- Worked primarily for Bag's Cosmology course. Learnt a lot. It was a lot of fun as well!

10.2 Thursday | March 17

- [research/ms] Today figured the final piece of the puzzle. I started with trying to arrive at precise assignments using the $e^{ia\hat{O}\otimes\hat{p}}$ type of hamiltonian. It was taking a lot of effort. Finally realized that I can just abstract out the Bohmain Mechanics by using a coin toss for making assignments in case the system is not in the eigenstate. [todo: add details, board clips etc.] Got a chance to talk to Arvind sir also, for a few minutes only though, but was able to convey the idea to him, atleast partially. Explained it to Kishor and he seemed to have been convinced (atleast distractedly) about the accuracy (but I could still be wrong).

- [misc] Project E started.

10.3 Friday | March 18

- [research/ms] Wrote the proof of sequential multiplicativity, finally and worked on updating various aspects of this report.

- [misc] Project E ended, qualified JEST ($\approx$400) and JRF-NET (89).

10.4 Saturday | March 19

- [research/ms] Started working on the draft seriously. Made various realizations about how to phrase multiplicativity etc. Explained the whole thing to bhati and it seemed to have made enough sense to him.

- [misc] Also qualified GATE ($\approx$400)

10.5 Sunday | March 20

- [research/ms] Completed writing the draft to a minimalistic level. Now I must discuss with arvind sir precisely how to proceed. Hopefully will see him tomorrow.
  Found some templates for the poster. Must discuss it with Kishor and Bhati.
  Was also trying to figure if one could downgrade positions instead of upgrading spins. The idea is intrinsically faulty, for then the measuring particle's position would also stop making sense.

11 An inkling of vehemence

11.1 Monday | March 21

- [research/ms] Finalized the poster template. Wrote down the list of things I have done for the Thesis.

11.2 Tuesday | March 22

- [research/ms] Discussed the developments so far with Prof. Arvind. He seemed perplexed about the contextual assignment of values. Did seem finally convinced. However, towards the end he made a remark that we shouldn't start with $\begin{matrix}1 & 1 & 1\\ 1 & 1 & 1\\ 1 & 1 & 1\end{matrix}$.
  This I should clarify with him, that this assignment is necessary for predicting values for the first time and hence not optional.
  The *first interesting* thing that sir had pointed out, in context of multiplicativity, was that one can measure a super operator $\hat{S}$ that can, in a single shot, measure all 'compatible' operators. His point was that then I know the values all these operators had, and thus the assignment must be consistent, even 'ontologically'. However, that isn't true. We can show this by the following simple example. Consider the state $|\psi\rangle = (|000\rangle + |011\rangle + |101\rangle + |110\rangle)/2$. Let the computational basis be written as $|i\rangle$, where i is the decimal of the binary equivalent. Let $\hat{S}$ be a super operator, given by $\hat{S} = i\,|i\rangle\langle i|$, which is just a diagonal matrix. Now a measurement of $|\psi\rangle$ will yield either 0, 3, 5 or 6. Now consider the operators $\hat{B}_1 = \mathbb{I}\otimes\mathbb{I}\otimes\hat{\sigma}_z$, $\hat{B}_2 = \mathbb{I}\otimes\hat{\sigma}_z\otimes\mathbb{I}$ and $\hat{B}_3 = \hat{\sigma}_z\otimes\mathbb{I}\otimes\mathbb{I}$. In this example, $\hat{C} = \hat{B}_1\hat{B}_2\hat{B}_3$. Now if I measure $\hat{C}$, I will always get a $+1$, and the state will never collapse. However, if I measure $\hat{B}_i$, I may get a ± 1 independently. A measurement of $\hat{S}$ will yield one of the values as stated, which would uniquely determine the values one gets for $\hat{B}_i$ *after* $\hat{S}$ has been measured, viz. if $\hat{B}_i$ are measured thereafter, they will agree with the values one would deduce from the outcome of measuring $\hat{S}$. We can't (atleast in no way conceivable presently) contradict (because QM is silent on that note) non-multiplicativity. I know this needs to be written properly, but the idea is roughly there.
  The *second interesting* thing that was raised, was whether the statistics of QM can be matched, for all states $|\psi\rangle$. I was under the impression that the said algorithm will work, but it won't. Consider $|\psi\rangle = \cos\theta\,|00\rangle + \sin\theta\,|11\rangle$. Obviously a measurement of $\sigma_z\otimes\sigma_z$ using the coin toss model will fail to get the right statistics.
  Other than this, Jaskaran didn't seem convinced of the accuracy of the model. He said that the assignments should be state independent, because contextuality is a statement about observables, and is state independent (he didn't want to bring in state dependence). That was one. He felt that the model is a sort of simulation of PM situation only and wasn't anything different or new. He suggested I look at 'Contextuality for preparations, tranformations and measurements' by Robert Spekkers.

11.3 Wednesday | March 23

- [research/ms] Figured how to fix the statistics in general, again motivated by Bohmian Mechanics. One could start with choosing a random number $c \in [0,1]$ (uniformly random). Then one finds $P_- = |\langle p_-|\psi\rangle|^2$ and then assigns -1 if $c < P_-$, else $+1$ is assigned.
  Further, Jaskaran claims to have found a mistake with my argument. Will listen to it, when he's available.

11.4 Thursday | March 24

- [research/ms] I found this http://journals.aps.org/pra/abstract/10.1103/PhysRevA.79.012102 today. Title of the paper: "Quantum contextuality in the Mermin-Peres square: A hidden-variable perspective" and here's a quote from the abstract: "...To prove that such a description is possible, an explicit and, in this sense, noncontextual hidden-variable model is constructed, which reproduces all quantum theoretic predictions for the Mermin-Peres square...". Now I can't find this on arxiv and therefore can't be certain how different it is from my work, but anyway. In addition there're discussions http://arxiv.org/pdf/1012.3052.pdf on how contextuality can't really be proven experimentally.
  Other than this, spent most of my time, on trying to write the poster. Some of the results include figure 4. The rough points of the draft have already been made.
  Finally, I also figured how one can 'complete' quantum mechanics using just a single hidden variable and make each operator have a predictable value. This sounds particularly interesting in the light of contextuality tests etc. In addition, I realized one can quickly show how values assigned to operators, may still depend on how they're measured; viz. the stern gerlach and direct value assigned to the operator can be shown to be different, effectively saying that one can't associate spin with a particle. However, that doesn't give rise to any inconsistency, since the theory is non-multiplicative. In addition to this, it also appears that one maybe able to turn the argument around and show that one can't associate even positions with particles. Now that would be seriously interesting and against BM. Perhaps then, it would remain how to construct an appropriate Hamiltonian, given an observable O, so that O can be measured using O', some other observable. One can also compare trajectories. I also seem to remember RS conjecturing the maximal completeness of their theory. Perhaps this theory is effectively doing that. But a little though reveals that in this sense, even BM is maximally complete. So

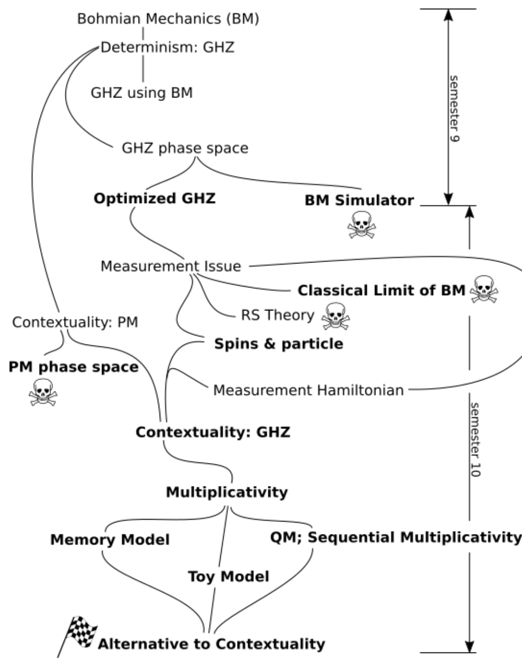


Figure 4: Research Flow

I'm guessing I remember the conjecture wrong. It also seems that one can show, within this simplistic framework, how non-locality and contextuality are related.
In hindsight, it appears to me that even RS were doing something mysteriously similar. My idea was simply to construct a cumulative probability distribution, by ordering the the outcomes based on eigenvalues (of whichever operator) and then using this single hidden variable to identify which event will happen. This is guaranteed to work because cumulatives are monotonic and increasing. It also has traces of this problem I thought about a long time ago in Bag's course, where I had to convert a uniform random distribution into an arbitrary random distribution.

11.5 Friday | March 25

- [research/ms] Various interesting ideas were discussed today. Let me start with the one that I had figured yesterday, or atleast appreciated properly enough then. We want to create a minimalistic hidden variable theory, in which only one hidden variable is assumed, and the outcomes of all experiments are predicted in the simplest conceivable way. Here it is.
  1. Initial: You're given $|\psi\rangle$; Choose a $c \in (0,1)$
  2. Assignment/Prediction: Given an operator $\hat{A}$, find its eigenkets $|a\rangle$ and note that $|\psi\rangle = \sum_a \langle a|\psi\rangle |a\rangle$. Define $c_a = \sum_{a'=a_{min}}^{a} \langle a'|\psi\rangle$. The value assigned to $\hat{A}$ is a s.t. a is the smallest eigenvalue, for which $c \leq c_a$. (for example, for $\sigma_x = A$, $\cos\theta\,|0\rangle + \sin\theta\,|1\rangle$, σ_x is assigned 0, if $c \leq \cos^2\theta$)
  3. After measuring, choose a new c and update $|\psi\rangle$ using the measurement postulate of QM, consistently with the value assigned in step 2.
  This has various interesting consequences (there's also an issue with the value of the spin operator and stern gerlach based value deduction etc.) It is especially interesting to note that this has to be non-multiplicative and peculiarities associated with it.
  The next interesting thing which was discussed, was observed and raised by Bhati. He proposed an interesting test to distinguish QM from BM. The essence of the idea was that one asks, what are the bounds in time, given for photons to arrive, at a given area (say the minima band) on the screen. He said that since according to BM, only a small set of trajectories (very close) should arrive at the screen, their time must be bounded; whereas according to the path integral picture, he claimed that this time is not bounded. This was eventually resolved by using the probability distribution $\psi(q_1, q_2, t)$ at the initial and final time to note that we will have an arc like set of dots (in the contour plot of ψ, for a later time) and perhaps the Gaussian in it make the time unbounded. So that was sorted. Interesting questions were raised about how to calculate the time corresponding to a path that the electron takes and about the time difference between the rays that interfere and their repercussion on single photon experiments.
  Finally, Jaskaran downloaded the paper for me. It turns out that the paper has a much more elaborate discussion and mine seems much more simple and straight forward. Also, he told me the issue with my construction; in a line, he said that it doesn't yield a joint probability distribution, which he demands mustn't change after measurement.

- [misc] Cleaned the room (including the balcony finally).

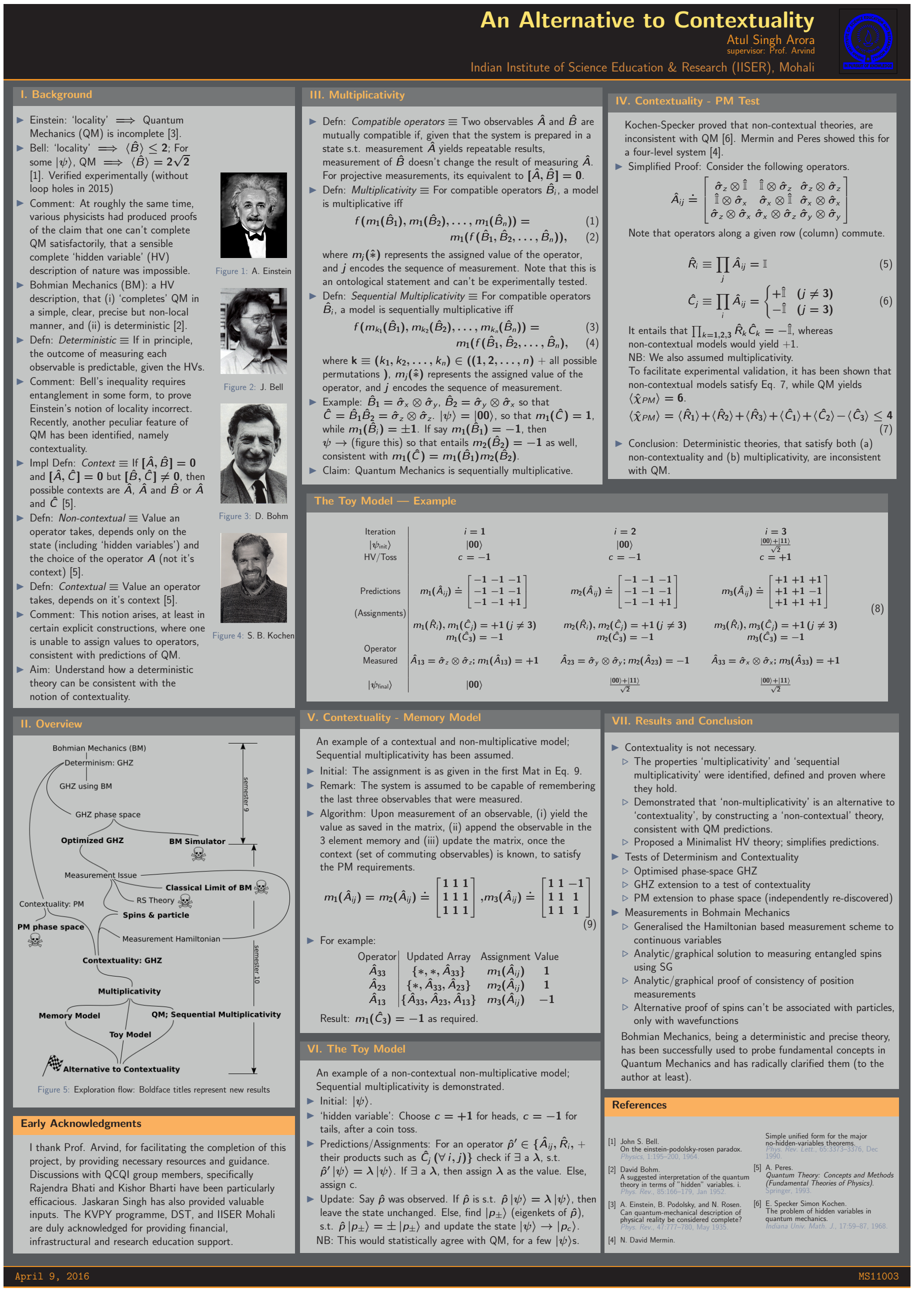


Figure 5: Poster Final Series

11.6 Saturday | March 26

- [research/ms] Recorded the discussion we had yesterday. Made more progress on the poster. Added most of the relevant content. Also learnt from Mermin's book, that he has infact 'postulated' something similar to multiplicativity and effectively said that if a theory is both multiplicative and non-contextual, then it can't be consistent with QM.
- [misc.] Updated the calendar, sorted my emails etc.

11.7 Sunday | March 27

- [research/ms] Completed the poster (see figure 5)! Started working on the thesis. Found a very good LyX template. Here https://github.com/kks32/PhDThesisLyX. Also realized that BM trajectories could effectively be evaluated using my c-variable theory! That means my BM simulator could be made extremely simple and efficient!
- [misc] Got yoyo some junk food to celebrate her offer!

12 Forlorn or Repletion

12.1 Monday | March 28

- [research/ms] Recall the conjecture that BM trajectories could effectively be evaluated using my c-variable theory. The proof of it should hinge on the fact that according to BM, the distribution of the particles preserves the $|\psi|^2$ distribution. Also, I realized one can show that there're effectively infinite hidden variable theories and there're very few constraints on how one must construct them.

- [course/Cosmology] Talked to bag, he gave me some hints on how to proceed. Let's see where I can go from here. My first attempt is to try to derive the result myself. Second would be to try reading Kaiser's paper. Let's see.

12.2 Tuesday | March 29

- [course/Cosmology] Term paper; working on figuring the jacobian issue. Interestingly, figured that one needn't have to evaluate the jacobian explicitly. Doing the radial part only also (in principle the other method should also but anyway) gives you correct answers. Talked to Bag about the next step, that requires fourier transforms. He explained to me the overall picture. The fourier transforms I tried, no luck. Went to see Bag again and he told me to take a single mode (without sum). That I came back and tried; did various wrong things, without much luck. Figured finally what was wrong and got an answer. The answer won't match though :(

12.3 Wednesday | March 30

- [course/Cosmology] Term paper; everything looked reasonable, couldn't figure what I was doing wrong. Finally figured it (upto a sign), while waiting for Bagla sir outside his office; I was making an assumption which wasn't required. That fixed things. Made some polar plots also. Was confused though, about $\hat{r}.\hat{k}$.

- [misc.] Kishor gave a QCQI talk. Couldn't understand the example. It was suggested he'll explain next time. I was also a little confused about the theorem about how if one assumes at best a pre-shared algorithm and no-signalling, one can show that all corelations will arise from local hidden variables.

12.4 Thursday | March 31

- [course/Cosmology] Updated the term paper, after discussing the doubts with Bagla sir; Also worked while watching the match, it was fun. Also completed the presentation (by stripping the extra words off of the term paper).

- [research/ms] Met with Arvind sir. Explained 1. Contextuality vs Multiplicativity (clear) + c-model, 2. Bohmian Trajectory using c-model and 3. Symmetrization of operators instead of states.

- [misc.] Arvind Sir told me to not do string theory in essence. Got choco stuff for Srijit and Shumo :)

12.5 Friday | April 1

- [course/Cosmology] Gave the presentation; Finished the term paper and submitted it also.

- [research/ms] Met with Abhishek sir, tried explaining the BM trajectory and c-model trajectory. Need to think more about it.

- [misc.] Jaskaran had a small doubt: The question was that after applying an projector P, the state should be $\text{Tr}_A(P^\dagger(\rho_S \otimes \rho_A)P)$ and not $\text{Tr}_A((\rho_S \otimes \rho_A)P)$ as is the case for finding the expectation from $\text{Tr}(\rho_S \otimes \rho_A P)$. The difficulty ofcourse was figured and resolved by deriving all the relevant quantities.

- [teaching/Thermodynamics] Studied chapter 5 from Callen's book. Looked at the notions of quasi static and reversible more carefully. Also looked at the maximum work theorem (saying that the max work that can be delivered, must come from a reversible path).

12.6 Saturday | April 2

- [teaching/Thermodynamics] Taught. Went well.

- [research/ms] I just realized that using my c-ingle variable theory, I can perhaps check how my own Bell's inequality (extended to q,p) gets violated! That ought to be fun!

12.7 Sunday | April 3

- [research/ms] Started working on the physics contents of the thesis, chapter 1 in progress.
- [misc] Had the farewell party today, didn't attend. Was working on the thesis. Went to Elante to be safe, had a mojito (without knowing it has alcohol in general, and still don't if it was virgin)

13 Importune but not irrevocable; respite fruitful

13.1 Monday | April 4

- [research/ms] Heavy: Working on writing the thesis, working on chapter 1.
- [misc.] Running, no gym | Fixed Prashansa's charger (soldered etc.)

13.2 Tuesday | April 5

- [research/ms] Heavy: Broke down the contents into chapters (made an index basically), completed chapter 1 and sent it to sir. Started work on chapter 2.
- [misc.] Shoulders.

13.3 Wednesday | April 6

- [research/ms] Heavy: Spent the entire day on chapter 2 (this is so far the longest chapter). (iGuess) Thought about taking the derivative of x for the c-ingle variable theory.
- [misc.] Kichoo continued the talk: was very interesting, finally was also able to figure how the inequalities are constructed.

13.4 Thursday | April 7

- [research/ms] Heavy: Chapter 3 almost completed (some simple parts missing)
- [misc.] Biscep (manu didn't come), went to ask for the GPS's marksheet, Deepika ma'am was on leave, the guy said he needs the picture

13.5 Friday | April 8

- [research/ms] Surprise! Figured finally how to prove that the c variable theory's trajectory, is the same as that of Bohm's! [todo: add picture] + Proof read the poster
- [misc.] Posted the warranty, went for GPS's marksheet, Deepika ma'am told the guy she had given it to him in a flash drive, the guy said come back at 5, and it should be done.

13.6 Saturday & Sunday | April 9 & 10

- [research/ms] Almost completed chapter 4 + had the poster presentation
- [teach/thermo] did some questions for teaching (had some interesting questions about the maximum attainable temperature and the temperature attained by just bringing two bodies in contact)

14 Audacious pillory | April 11 - 17

- [research/ms] Figured the relation between Bell's inequality and Multiplicativity! Added graphs and references to the thesis including writing the last chapter. Figured the continuous variables part properly and re-read the older chapters to remove mistakes.

15 Fortuitous Co-existence or Besmirching Semblance | April 18 - 24

- [research/ms] Added the theorem module (that is very simple to add in LyX. Just goto the settings/preferences and there's a section on modules. Just add theorems from there) in chapter 4. Worked on updating the template to match IISER requirements. Started working on a precise draft of the same and also on the presentation.
- [teach/thermo] Prepared for the lecture (but it didn't happen)

16 April 25 - 30

- [research/ms] Thesis submitted! Presentation completed and given. Also figured how one might implement an n-core digital computer using $\log_2 n$ qubits extra on a quantum computer.
- [teach/thermo] TA questions (last year's question paper) prepared. Held the help session.
- [course/cosmology] Studied for it (full blown revision etc.) and took the exam.

Part III
Epilogue

1 Resilience

1.1 Wednesday | May 18

- [protocols/friendship] Figuring how to make a game | talked with Cation
- [PhD] Max-planck lookup + Oxford

1.2 Thursday | May 19

- [revision] Electrodynamics (the theorems on curl, divergence etc.)
- [research/read] Cirac's work (simulating high energy physics using quantum computers)
- [research/ms] Thinking about how BM can be shown to have some un demonstrated funny things (like positions of two particles don't multiply).
- [protocols/ϕ@I Radiation] Held a session. Today was good.

Part IV
Appended Topics

1 The Thesis Problem - Non locality and Contextuality

1.1 Definition

The following has been taken from an email written to Ali Asadian.

[background]

As you know, in the GHZ test, one is able to show that determinism can't hold. However, this is done using spins. How this is handled in Bohmian Mechanics (BM) has been discussed already[5]. It is not in direct contradiction with BM because spins are not treated like (q,p). In BM, (q,p) are well defined, just that we can't observe them. However, spins in BM are only a property of the wavefunction and not postulated to have well defined 'values' like (q,p). Thus, while it is interesting to see how BM handles being deterministic and consistent with GHZ, it's not too surprising to see it work, since spins aren't assumed deterministic like (q,p).

[thesis problem]

Do you recall the GHZ paper that had been extended to continuous variables? The point was that this particular approach showed that there can't be determinism for (q,p). However, BM seems to be an example that does precisely this. How can this be? It is precisely this that I intend to explore in my thesis. If BM's predictions differ from QM, then we at once have a wonderful test to find which theory is correct. If the predictions match, as is more likely, we'll be closer to answering to atleast two important questions. (1) Which extra assumption goes into the GHZ like tests which is unaccounted and (2) How contextuality emerges from non-locality, especially in continuous variables.

[future scope]

Ofcourse, a more ambitious goal would be to look at your results [4] on contextuality in continuous variables and use BM to understand the relation of non locality with it more directly. Perhaps if I have enough time, I'll pursue that as well. The one question which I still haven't an answer to is the following: How is it that, while formally in QM, spins and (q,p) are handled very similarly, why can't we extend BM in a manner to include spins as 'deterministic' as are (q,p)? I would like to either find such a formulation or show that it doesn't exist. This is of great interest for this answer must depend on the fundamental difference between spins and (q,p) as properties. The thesis problem is a step in this direction.

1.2 Breakup

1. Numerical Analysis
   (a) Bohmian Basics
      i. Simulate a free particle. Start with say a guassian wavefunction and take the initial positions to be accordingly distributed.
      ii. Simulate a harmonic oscillator. Use the previous step, except now with a harmonic oscillator potential. The gaussian should oscillate nicely. Once that's confirmed, then check the trajectories.
      iii. Simulate a squeezed state.
      iv. Tunnelling perhaps?
      v. S,P orbitals, trajectories (interesting by themselves!)
   (b) Bohmian Advanced
      i. Simulate a two particle state.
      ii. Simulate stern gerlach
      iii. Simulate the GHZ experiment using Ref. [5]
2. Analytic Work
   (a) Bohmian Mechanics
      i. Quantum Theory of trajectories, Holland [todo: add reference] (depth-read chapter n, 50% depth-read chapter n+1)
      ii. Bohm's original paper [todo: add reference] (70% depth-read paper 1)
      iii. Measurements [6] (float-read)
      iv. Spins, Stern Gerlach [6] (float-read)
   (b) Bell, GHZ test etc. from the Bohmian perspective [5]
   (c) GHZ test in continuous variables [7] (careful-read, during summers)
   (d) Construct an experiment in BM to perform the GHZ test; check predictions

References

[1] https://jakevdp.github.io/blog/2012/09/05/quantum-python/.

[2] http://www.pa.msu.edu/~duxbury/courses/phy480/SchrodingerDynamics.pdf.

[3] http://ww2.odu.edu/~agodunov/computing/programs/book2/Ch01/spline.f90.

[4] Ali Asadian, Costantino Budroni, Frank E. S. Steinhoff, Peter Rabl, and Otfried Gühne, *Contextuality in phase space*, Phys. Rev. Lett. **114** (2015), 250403.

[5] Thomas Durt and Yves Pierseaux, *Bohm's interpretation and maximally entangled states*, Phys. Rev. A **66** (2002), 052109.

[6] Detlef Dürr, Sheldon Goldstein, and Nino Zanghì, *Quantum physics without quantum philosophy*, Springer Berlin Heidelberg, 2012.

[7] Serge Massar and Stefano Pironio, *Greenberger-horne-zeilinger paradox for continuous variables*, Phys. Rev. A **64** (2001), 062108.