

The Problem of Quantization

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The plan of my lectures is as follows:

(1) Today: The problem of quantization: quantization by branes as a partial solution

(2) Tomorrow: Gauge theory and geometric Langlands: review of older results

(3) Monday: Lecture at the special session in memory of M. S. Narasimhan and C. S. Seshadri

(4) Tuesday: Gauge theory and geometric Langlands: application to the “analytic” approach by Etinghof, Frenkel and Kazhdan (new

Textbooks tell us that quantization means to replace functions f on classical phase space by operators \widehat{f} , in such a way that Poisson brackets are mapped to commutators:

$$[\widehat{f}, \widehat{g}] = -i\hbar \widehat{\{f, g\}}.$$

What textbooks don't usually emphasize is that this doesn't work, or better, it only works approximately, to first order in \hbar , or for a preferred class of functions on the classical phase space.

Accordingly, there actually is no completely natural operation of quantizing a classical phase space – none that is known, and I believe, none that exists. Quantization always requires some additional structure. As Ludwig Faddeev used to say, quantization is “an art not a science.”

The basic reason that there is a problem is that the symmetry groups of classical mechanics and quantum mechanics are different. In classical mechanics with a phase space M , the group of symmetries is (roughly) the group \mathcal{G} of canonical transformations of M (the group of symplectomorphisms). In quantum mechanics, the corresponding group is the group \mathcal{U} of unitary transformations of a Hilbert space \mathcal{H} . If M has infinite volume, \mathcal{H} is infinite-dimensional. In that case \mathcal{U} doesn't know anything about M , while \mathcal{G} does. So they are different. Since a classical system and its quantization have different symmetry groups, there cannot be an entirely natural passage from classical to quantum mechanics. In quantum field theory, when quantizing a system changes the symmetry group, this is usually called an anomaly. So I am telling you that at the kinematic level, there is an anomaly in the passage from classical mechanics to quantum mechanics. (Groenevald 1946, Van Hove 1950, ...)

Let us discuss the situation for the case that our phase space is $M = \mathbb{R}^2$ on which we pick linear coordinates x, p with Poisson brackets $\{p, x\} = 1$. Before I go on I want to stress that the choice of what we call linear coordinates on \mathbb{R}^2 is far from canonical, even if we say that they are supposed to obey $\{p, x\} = 1$. Any area-preserving transformation of the plane, that is, any transformation that preserves the symplectic form $\omega = dpdx$ (which in two dimensions is just an area form) would map p and x to different coordinate functions on the plane that we could equally well call “linear functions.” Any Hamiltonian function $H(p, x)$ generates, by Poisson brackets, an area-preserving transformation of the plane. So when we pick a pair of coordinates p, x on the plane that obey $\{p, x\} = 1$, we have made a choice out of a vast range of possibilities.

Once we have picked what we regard as p, x , we can make sure that Poisson brackets map to commutators for those particular functions by defining x to be a multiplication operator and setting

$$p = -i\hbar \frac{d}{dx}.$$

According to a famous theorem of Stone and von Neumann, this is essentially unique (any pair of self-adjoint operators p, x obeying $[p, x] = -i\hbar$ is unitarily equivalent to what we've just defined).

Having made this choice, we can now ask for what other functions on \mathbb{R}^2 is it true that Poisson brackets map to commutators. To simplify the discussion a bit, let us just consider polynomial functions of p, x . The question is: can we order the factors in polynomial functions of p, x so that Poisson brackets will map to commutators?

I am going to change notation slightly and write x_1, x_2 instead of p, x . Also I will introduce the Weyl ordering of a monomial by averaging over all possible orderings

$$\langle x_{i_1} x_{i_2} \cdots x_{i_k} \rangle = \frac{1}{k!} (x_{i_1} x_{i_2} \cdots x_{i_k} + \cdots).$$

I am *not* assuming to begin with that Weyl ordering is the best ordering; we will consider all possible orderings. But we will see that in a sense, Weyl ordering makes the match between classical and quantum mechanics as close as possible.

For polynomials of degree ≤ 2 , there is no anomaly. As long as one uses Weyl-ordering (but not with a different choice), the passage from classical to quantum mechanics maps Poisson brackets to commutators,

$$[\widehat{f}, \widehat{g}] = -i\hbar \widehat{\{f, g\}}.$$

This is nontrivial because if f, g are quadratic functions that we quantize to get \widehat{f}, \widehat{g} , then it looks like there could be a term of order \hbar^2 in $[\widehat{f}, \widehat{g}]$, which would spoil the match between classical and quantum mechanics. If and only if we use Weyl ordering, there is no such term.

In particular, homogeneous quadratic functions of x_1, x_2 generate the Lie algebra of $SL(2, \mathbb{R})$ by Poisson brackets, and also (since there is no anomaly) by commutators. Now consider homogeneous polynomials of degree n for some $n > 2$. They transform classically in an irreducible representation of $SL(2, \mathbb{R})$ of degree n . If and only if we use Weyl ordering, there is no anomaly in the commutator of a polynomial of degree ≤ 2 with a polynomial of any degree. This is essentially guaranteed by the $SL(2, \mathbb{R})$ group theory.

However, we will run into the anomaly if we consider the commutator of two Weyl-ordered polynomials of degree > 2 . It is enough to consider an example. Let $z = (x_1 - ix_2)/\sqrt{2}$, $\bar{z} = (x_1 + ix_2)/\sqrt{2}$, so $\{z, \bar{z}\} = i$ and $[z, \bar{z}] = \hbar$. We have $\{z^3, \bar{z}^3\} = 9iz^2\bar{z}^2$, so absence of an anomaly would require $[z^3, \bar{z}^3] = 9\hbar\langle z, z, \bar{z}, \bar{z} \rangle$. But instead

$$[z^3, \bar{z}^3] = 9\hbar\langle z, z, \bar{z}, \bar{z} \rangle + \frac{3}{2}\hbar^3,$$

showing the anomaly. (For polynomials of higher degree, the anomaly is not just a c -number.)

So one can quantize \mathbb{R}^2 by picking coordinates x, p with $\{p, x\} = 1$ and then one can quantize to get $[p, x] = -i\hbar$. Someone else could quantize the same space using coordinates x', p' that also satisfy $\{p', x'\} = 1$ and quantize them to get $[p', x'] = -i\hbar$. But these quantizations are different. Generically, in the first it is not true that $[p', x'] = -i\hbar$ and in the second it is not true that $[p, x] = -i\hbar$.

However, before going on, I should point out that there is a good reason that these matters are not better known. In physics, it is rare that one wishes to quantize an abstractly presented classical phase space. Almost always there is some additional structure that gives one some guidance about what quantization is appropriate. For example, in a scalar field theory, there is a preferred set of linear functions on phase space – the linear functions of the field variables. There is also a preferred function that should be bounded below – the energy. In particle mechanics, the spatial position and momentum are distinguished variables, because of the symmetries of space and spatial locality.

I want to discuss in a little detail a formal try to solve the problem of quantization that doesn't work. Keeping this story in mind will be useful toward the end of the lecture. A formal approach to the problem of quantization is as follows. Let \mathcal{L} be a unitary line bundle with connection T/\hbar and curvature ω/\hbar , where ω is the symplectic form. Such an \mathcal{L} is called a prequantum line bundle and the choice of such an \mathcal{L} is the first step in "geometric quantization," which we will discuss in a little while. Let r, r' be points in M , and for a path γ from r to r' , consider the action

$$I = \frac{1}{\hbar} \int_{\gamma} T.$$

Alternatively, let $I = [0, 1]$ and view γ as the image of a map $x : \gamma \rightarrow M$. Then the action is

$$I = \frac{1}{\hbar} \int_I x^*(T).$$

If we could attach a quantum theory to this action in a natural way, we would say we have quantized M . But we cannot.

Let $P_{r,r'}$ be the space of paths from r to r' . We'd like to calculate the path integral

$$Z(r, r') = \int_{P_{r,r'}} Dx \exp(iI).$$

But there is so much symmetry that this cannot possibly lead to a sensible invariant answer. The only possible answer invariant under the symmetries would be $\delta(r, r')$, which isn't at all close to what we'd want. For example, in the case of a compact phase space which is supposed to have a finite-dimensional Hilbert space, we'd want an answer in the form of a finite sum

$$\sum_{\alpha=1}^W \psi_{\alpha}(r') \bar{\psi}_{\alpha}(r).$$

That is not $\delta(r, r')$. More broadly, bearing in mind that " r " encompasses both "position" x and "momentum" p , an answer like $\delta(r, r')$ would imply that the states can be sharp in both x and p .

Technically, if we try to compute the path integral

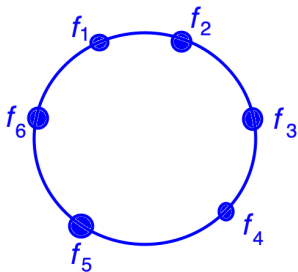
$$Z(r, r;) = \int_{P_{r,r'}} Dx \exp(iI) = \int_{P_{r,r'}} Dx \exp\left(\frac{i}{\hbar} \int_0^1 dt p_i \frac{dq^i}{dt}\right)$$

(where I've made a possible choice of T) what goes wrong is that boundary conditions that specify both p and q are overdetermined – not elliptic – and one has trouble defining the path integral even in perturbation theory. If instead one only specifies (for example) p or q at the initial and final endpoints of the interval, the path integral makes sense – but one has broken the symmetries of classical mechanics and one never gets them back. Using this kind of boundary condition would actually lead to geometric quantization, which we will discuss a little later.

I want to make two further remarks about this: First remark: A partial remedy is to consider the path integral on a circle rather than on the interval $[0, 1]$. Then one does not need boundary conditions and the path integral is better-defined. (It is not clear to me if it can be defined in a way that fully preserves the symmetries of classical mechanics.) One can no longer construct a Hilbert space, because one does not have “initial and final states” which are supposed to be inserted at initial and final endpoints of the interval on which we do the path integral. But the path integral of this theory on a circle suffices for “deformation quantization,” in which one constructs a quantum-deformed algebra of functions (sometimes with a trace) but no Hilbert space that it acts on.

One picks functions f_1, \dots, f_n on M and cyclically ordered points $p_1, \dots, p_n \in S^1$ and one integrates over the space of loops $x : S^1 \rightarrow M$ to define

$$\langle f_1 f_2 \cdots f_n \rangle = \int Dx \exp(iI) f_1(x(t_1)) f_2(x(t_2)) f_3(x(t_3)) \cdots f_n(x(t_n)).$$



The correlation functions $\langle f_1 f_2 \cdots f_n \rangle$ have the information needed to define the quantum deformed algebra: if

$$\langle f_1 f_2 f_3 \cdots f_n \rangle = \langle g f_3 \cdots f_n \rangle$$

(where g depends on f_1 and f_2 but not on f_3, \dots, f_n), one defines

$$f_1 \star f_2 = g.$$

This \star product is associative and has a trace, but it is not quantization since it does not have an appropriate Hilbert space representation. There is a rigorous version of these statements about the path integral on a circle by Grady, Q. Li, and S. Li, arXiv:1507.01812, following a well-known (non-path integral) approach to deformation quantization by Fedosov (1994). There is also a generalization of deformation quantization for manifolds that are Poisson but not symplectic by Kontsevich (1997) and Cattaneo and Felder (1999). I've explained all this because although we are really ultimately interested in quantization, deformation quantization will also appear as part of the story.

Now I want to say how we will actually make use of the ill-behaved theory

$$I = \frac{1}{\hbar} \int_{\gamma} T.$$

Suppose that one has a well-defined microscopic theory that somehow produces a Hilbert space \mathcal{H} , and that can be expressed by some formal argument in terms of the action I plus some massive degrees of freedom that are formally irrelevant. This means that the microscopic theory is giving us a way to embed the ill-defined problem of quantizing the action I in a larger, well-defined problem. In this situation, it is reasonable to declare \mathcal{H} to be a quantization of M . That is what we will actually do with brane quantization.

The problem of the non-uniqueness of quantization only has partial remedies. I will discuss two partial remedies:

- ▶ geometric quantization
- ▶ quantization by branes

(I have already mentioned deformation quantization, which is another partial remedy.)

In practice, quantization is usually carried out by picking a maximal set of Poisson-commuting functions x_1, \dots, x_n and treating them as multiplication operators, and treating the conjugate variables p_1, \dots, p_n as differentiation operators. The most systematic description of this procedure is “geometric quantization” (Souriau, 1966; Kostant, 1970; ...). The choice of a maximal set of Poisson-commuting functions is called a “polarization.”

The main types of polarization are a real polarization, locally a choice of p_i and x^j (coordinates and momenta), or a complex polarization, which is a complex structure on the phase space M with the property that holomorphic functions are Poisson-commuting (locally if p_i, x^j define a real polarization, then there is a complex polarization in which $z^j = (p_j - ix^j)/\sqrt{2}$ are the Poisson-commuting holomorphic functions). To get beyond the elementary case of \mathbb{R}^{2n} , the simplest case is that $M = T^*N$ is the cotangent bundle of some other manifold N , such that if x^i are local functions on N , there are linear functions p_i on the fibers of the cotangent bundle such that the symplectic form is $\omega = \sum_i dp_i dx^i$. In this case, geometric quantization will tell us that the Hilbert space \mathcal{H} that we should define is the space of L^2 half-densities on N . (One can similarly describe what geometric quantization says for other types of polarizations but I will omit this.)

Geometric quantization further tells us, for a given choice of polarization, what is a distinguished class of functions such that quantization, with the given polarization, does map Poisson brackets to commutators. The functions in question are simply the ones that, by Poisson brackets, generate canonical transformations that preserve the polarization. In the simplest case of $M = T^*N$, these are the functions that are at most linear in the momenta p_i . In other words, in this particular case, geometric quantization will just tell us that functions of the form

$$w(x^1, \dots, x^n) + \sum_{k=1}^n v^k(x^1, \dots, x^n) p_k$$

can be quantized in such a way that Poisson brackets are mapped to commutators. This statement is not hard to prove, but it is also not trivial and only works with the correct operator ordering. If f, g are two functions that are linear in the p 's, then the commutator $[f, g]$ potentially has a term of order \hbar^2 , which would spoil the match between Poisson brackets and commutators. The statement of geometric quantization in this particular case is just that operator ordering can be chosen to avoid any term of order \hbar^2 in $[f, g]$.

For functions quadratic or higher order in p , terms of order \hbar^2 and higher generically cannot be avoided by operator ordering and there is no precise match between commutators and Poisson brackets.

Geometric quantization extends this discussion to make similar statements for more general types of polarization, always picking out a preferred class of functions that behaves well. Most of what we usually call quantization can be expressed in terms of geometric quantization, but geometric quantization has two drawbacks, one of which I consider somewhat esoteric and one of which I consider really significant. The esoteric drawback of geometric quantization is that it is possible to have a phase space M (for instance a compact one) that admits no polarization. So if we rely on geometric quantization, we don't know what to do.

The issue that I really care about is this: More typically a classical phase space M does admit a polarization, and therefore, upon choosing a polarization, it can be quantized using geometric quantization. But if there is one polarization, then there is a family of polarizations depending on infinitely many parameters. Geometric quantization with different polarizations gives quantizations that generically are inequivalent. Here “equivalent” means that there is a unitary transformation $U : \mathcal{H} \rightarrow \mathcal{H}'$ from the Hilbert space \mathcal{H} defined with one polarization to the Hilbert space \mathcal{H}' defined with another polarization, in a compatible way (where there are a few slightly different possible criteria for “compatible,” but none of them are satisfied).

To some extent, this is not a limitation of geometric quantization, but an inevitable fact of life, because of the anomaly that I explained at the beginning. What to me is really a limitation of geometric quantization is the following. There are exceptional cases in which quantization with different polarizations *does* give equivalent Hilbert spaces. As I will illustrate with examples, when this happens, it typically is associated with important results. But geometric quantization does not shed much light on when/why this sometimes happens. This suggests that one might want an alternative approach to quantization that would explain such phenomena better.

Here are some examples in which different polarizations actually *are* equivalent. One of my main reasons is explaining these examples is to convince you that when different polarizations are equivalent, this is usually associated to significant results.

(1) The most basic example is \mathbb{R}^{2n} . To quantize it, we can pick a notion of what we mean by linear functions – an “affine linear structure.” Once we have decided on what we mean by linear functions, a further separation into p 's and q 's does not matter. The most famous special case of this is that the p 's and q 's can be exchanged by a Fourier transform. More generally, as I explained before, there is no anomaly in the quantization of functions on \mathbb{R}^{2n} that are at most quadratic. The functions that are at most quadratic generate arbitrary affine linear canonical transformations $\vec{x} \rightarrow A\vec{x} + \vec{b}$, where $\vec{x} = \begin{pmatrix} p_i \\ q^j \end{pmatrix}$. Since there is no anomaly in the canonical transformations that go from one set of linear coordinates to another, the choice of a splitting into p 's and q 's doesn't matter, once one has picked the affine linear structure in the first place. The affine linear structure certainly does matter.

(2) For a second example, consider Chern-Simons gauge theory in three spacetime dimensions, initially with a *compact* gauge group G . The action is

$$I = \frac{k}{4\pi} \int d^2x dt \text{Tr} \left(A \wedge dA + \frac{2}{3} A^3 \right).$$

This is an unusual example of a quantum field theory in which we really do meet the subtleties of quantization. We are hoping to get a topological field theory, because the action is defined without any choice of a spacetime metric. However, it turns out that it is not possible to pick a polarization without breaking the topological symmetry. Therefore, to show that the theory really is a topological field theory, we need a result showing that different polarizations are equivalent. It is this example that first got me interested in this sort of question 30 years ago.

Before going on to mention a few other examples that work, I want to mention an example that *isn't* understood: This is simply the same theory, but with a noncompact real semisimple gauge group such as $SL(n, \mathbb{R})$. After breaking the topological symmetry by a choice of polarization, one can quantize, but one doesn't know how to show that the different polarizations are equivalent. So in general, we don't really know how to show that $SL(n, \mathbb{R})$ Chern-Simons (for example) is a topological field theory. (This isn't just a problem of a missing proof. We don't know what the equivalence between the different quantizations is supposed to be.)

(This is a broad brush picture. Actually, fairly satisfactory – and indeed fascinating – answers are known in some important cases.)

(3) For a third example, we can consider representation theory of noncompact simple Lie groups. To be simple, we consider

$SL(2, \mathbb{R})$, the group of real matrices $\begin{pmatrix} a & b \\ c & d \end{pmatrix}$ of determinant 1.

$SL(2, \mathbb{R})$ acts on the circle – which we can represent as the real line plus a point at infinity – by

$$x \rightarrow \frac{ax + b}{cx + d}.$$

L^2 functions $f(x)$ do *not* provide a unitary representation of $SL(2, \mathbb{R})$ because there is no $SL(2, \mathbb{R})$ -invariant measure

$$\|f\|^2 \stackrel{?}{=} \int dx \bar{f}(x)f(x).$$

Instead, we should use half-densities

$$f(x)(dx)^{1/2}$$

and more generally $\frac{1}{2} + is$ densities for any real s :

$$f(x)(dx)^{1/2+is}.$$

These do provide unitary representations of $SL(2, \mathbb{R})$ with the inner product

$$\|f\|^2 = \int |f(x)|^2 dx.$$

It turns out that the representation provided by $\frac{1}{2} +$ is densities is isomorphic to the one provided by $\frac{1}{2} -$ is densities. This has an analog for semisimple Lie groups of higher rank and is important in representation theory. The Weyl group of SL_2 is \mathbb{Z}_2 , which acts here by $s \rightarrow -s$. For any semi-simple G , the Weyl group similarly provides equivalences between representations.

The equivalence of the two representations of $SL(2, \mathbb{R})$ is usually demonstrated by an elementary explicit formula. However it can be understood as a statement of equivalence between two different methods of quantizing the same phase space. The phase space in question is described by

$$x^2 + y^2 - z^2 = j^2,$$

with real x, y, z and a constant j^2 (which after quantization is interpreted as s^2). There is an obvious “spin 1” action of $SO(2, 1)$ (whose double cover is $SL(2, \mathbb{R})$) on the triple x, y, z . There is an $SO(2, 1)$ -invariant symplectic form $\omega = \varepsilon_{ijk} x^i dx^j dx^k$ (where x^i are x, y, z). So we have a classical phase space with $SO(2, 1)$ symmetry and we can hope that quantization will give a unitary representation of $SO(2, 1)$, or possibly of its double cover $SL(2, \mathbb{R})$.

To quantize, we have to pick a polarization, and it turns out that there are two nice $SO(2, 1)$ -invariant polarizations. One is described by

$$q = \frac{j - x}{y - z}$$

and one by

$$q' = \frac{j - y}{x - z}.$$

Following the logic of geometric quantization, in more detail than I have explained, one finds that quantization with one polarization leads to the representation by $\frac{1}{2} + i$ s densities and the other to the representation by $\frac{1}{2} - i$ s densities. So this is another example of nontrivial equivalence between quantization of the same phase space in two different ways.

(4) My last example is the pentagon identity of Faddeev and Kashaev for the quantum dilogarithm. One definition of the quantum dilogarithm is

$$\phi(u) = \prod_{n=0}^{\infty} (1 - uq^n)$$

and it obeys a variety of identities including the Faddeev-Kashaev identity

$$\phi(v)\phi(u) = \phi(u)\phi(-vu)\phi(v).$$

This function and the identities it satisfies are important in many of the integrable models of statistical mechanics and many-body physics in 2 space or spacetime dimensions. The identity can be regarded as a consequence of a statement that 5 different ways to quantize the same phase space are equivalent.

As I have said, there is no accepted general theory of when different quantizations are going to be equivalent. However Sergei Gukov and I provided a partial answer a number of years ago (“Branes and Quantization,” arXiv:0809.0305). Recently I have returned to this subject with Davide Gaiotto (work to appear). The motivation for doing so was a specific application, involving recent work of E. Frenkel, D. Kazhdan, and P. Etinghof on the geometric Langlands program; it will be the subject of lectures 2 and 4.

By way of motivation, something all four of our “nice” examples have in common is that the classical phase space M can be complexified to a complex symplectic manifold Y which, intuitively, is in some sense “complete.” Technically they are complete in that they have complete hyper-Kähler metrics with some additional properties. Moreover the distinguished functions that are being quantized extend to holomorphic functions on Y , and the polarizations that are used for quantization extend to holomorphic polarizations of the phase space.

Let us just review the four examples. (1) The phase space was \mathbb{R}^{2n} . We picked an affine linear structure, meaning a choice of what we mean by a function on \mathbb{R}^{2n} being at most linear. Then the preferred functions are the linear functions x^1, \dots, x^{2n} and for those functions Poisson brackets matched with commutators. Once we pick an affine linear structure on \mathbb{R}^{2n} , we can complexify it to \mathbb{C}^{2n} by viewing the x^k as complex variables, and therefore, these functions analytically continue to holomorphic functions on \mathbb{C}^{2n} .

Please note that one does *not* know how to complexify \mathbb{R}^{2n} to \mathbb{C}^{2n} before picking some additional structure beyond its symplectic structure. That is clear from the fact that most symplectomorphisms of \mathbb{R}^{2n} do not extend to holomorphic automorphisms of \mathbb{C}^{2n} .

(2) In our second example, the phase space is the classical phase space of Chern-Simons gauge theory with compact gauge group G .

$$I = \frac{k}{4\pi} \int d^2x dt \text{Tr} \left(A \wedge dA + \frac{2}{3} A^3 \right).$$

This has a nice complexification, which is the phase space of the same theory with gauge group the complexification $G_{\mathbb{C}}$ of G .

(3) In our third example, the phase space was

$$x^2 + y^2 - z^2 = j^2$$

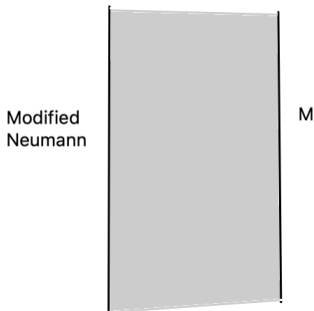
with real x, y, z . The preferred functions are x, y, z , which after quantization become the $SL(2, \mathbb{R})$ generators. We complexify by considering the same equation with x, y, z as complex variables.

(4) I explained less fully the example with the quantum dilogarithm. In that case, the phase space is \mathbb{R}^2 , but the complexification one should use is not \mathbb{C}^2 but rather $\mathbb{C}^* \times \mathbb{C}^*$ where $\mathbb{C}^* = \mathbb{R} \times S^1$. In other words, one complexifies $\mathbb{R}^2 = \mathbb{R} \times \mathbb{R}$ by replacing each \mathbb{R} with $\mathbb{C}^* = \mathbb{R} \times S^1$, which you can also think of as the complex z -plane with an equivalence $z \cong z + 2\pi i$.

Any real analytic classical phase space, symplectic form, observable function, and polarization could be complexified a little bit. That is more or less the definition of real analytic. What is exceptional here is that there are complexifications that are in some sense “complete” (they admit a complete hyper-Kähler metric, and likewise the functions and polarizations considered have analytic continuations that are holomorphic everywhere).

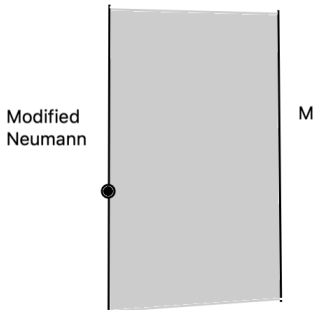
Why is it useful if the classical phase space M and all the associated structures (symplectic form, the observables we are trying to study, and the polarizations whose equivalence we want to prove) extend to a “complete” complex manifold Y ? The proposal by Gukov and me is that the problem of quantization of M can be embedded in a bigger and seemingly much more complicated problem – the two-dimensional A -model with target Y . In that context, it is understood that if Y admits a complete hyper-Kähler metric, then the quantum σ -model is well-defined – a true quantum field theory. A standard “twisting” of it gives the A -model.

The embedding of the problem of quantization in the σ -model is roughly as follows: Quantizing the σ -model on a spatial interval with appropriate boundary conditions gives a Hilbert space \mathcal{H} which can be interpreted as the quantization of the underlying classical phase space M :



Note that these boundary conditions correspond to “branes” in the two-dimensional σ -model and this approach to quantization can be called “quantization via branes.”

Observables - which correspond to holomorphic functions on Y - are inserted on the left boundary of the strip:



this construction tomorrow.

I will explain more about

An interesting fact about this construction, which also explains some subtleties of quantization but in this case subtleties that I haven't had time to explain, is this: The same Y can sometimes be regarded as a complexification of two different real submanifolds. When this happens, we can get two inequivalent quantum systems that provide inequivalent representations of the same algebra of observables (viewed just as an algebra, ignoring the $*$ operation). For instance the complex phase space

$$x^2 + y^2 - z^2 = j^2$$

is the complexification of a manifold M with x, y, z real, but it is also the complexification of a manifold M' with x, y imaginary and z real. In this case we should be taking $j^2 < 0$.

In summary, I have tried to explain that, contrary to the impression one might get from textbooks, quantization of a classical phase space is not a clear, algorithmic procedure. It requires some additional structure, which in geometric quantization is a choice of polarization, and which in brane quantization is the choice of a suitable complexification of the classical phase space. Geometric quantization formalizes the textbook recipes. Its main drawback is that it does not give much insight about whether/why two different quantizations may be equivalent. Brane quantization is only applicable in the best cases, but when it is applicable it probably gives the most illuminating results.