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John Baez <johnb@ucr.edu>

digression on jordan cone of 3-by-3 hermitian complex matrixes

17 messages

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 3, 2022 at 2:38 PM
To: john.baez@ucr.edu

this is a somewhat digressive exploration of the naive intuitive geometry of the jordan cone of 3-by-3 positive-definite hermitian matrixes aka "unnormalized density matrixes" ....

(i'm not sure offhand to what extent you and i are using the terminology "cone" the same way in various parts of this discussion. in this case i mostly mean something like "convex cone", referring to the "interior" (or perhaps "closure of the interior") of a cone; in contrast to some other contexts where "cone" might refer to something like the "boundary surface" rather than to the interior. nevertheless i'm hoping that this won't cause serious confusion.)

this part of the discussion is "digressive" in that i'm focusing on a sort of "coincidence" connecting something that we're studying now (neron-severi lattices of abelian varieties, construed in particular in terms of self-adjoints wrt a "rosati anti-involution") with something that we've already thought about since a long time ago (the geometry of "mixed states" of a quantum system, though making allowances now for the consideration of the unnormalized density matrixes in contrast to the normalized ones).

this coincidence manifests itself most clearly perhaps with examples such as the 3-dimensional abelian variety given as the cartesian cube of the "gaussian lemniscate" elliptic curve, where the cone of ample line bundles inside its neron-severi lattice looks a lot like a discretization of the jordan cone of unnormalized density matrixes for a "qutrit".

(i want to examine the qutrit case in contrast to the qubit case because on the one hand the qubit case is so familiarly visualizable in terms of the bloch ball of mixed states inside the riemann sphere of pure states, whereas whenever i've tried to think about the qutrit case, i've had a lot of trouble trying to understand it in any intuitive way.)

of course, secretly i have all sorts of crazy ideas in the back of my mind about how the "coincidence" here might be more than just a coincidence; that is, that there could be some deeper conceptual relationship between neron-severi lattices of abelian varieties on the one hand and the geometry of quantum measurement on the other hand. however, i'm _not_ going to pursue any such crazy ideas at the moment; rather i'm just thinking of the coincidence as enabling us to play a game in which we get to transport some of our knowledge and/or some of our ignorance across the bridge provided by the cryptomorphism here, connecting separate realms both of which we happen to be interested in.

so, here's where i start to get confused when i start thinking about qutrits:

a qubit has 2 dimensions worth of pure states and 3 dimensions worth of mixed states, so it makes perfect sense that the pure states form the boundary of the space of mixed states.

in contrast, a qutrit has 4 dimensions worth of pure states and 8 dimensions worth of mixed states, so the idea that the pure states might be the boundary of the space of mixed states fails, and already that confuses me a little.

but then i realize that in general there's a big difference between the boundary points of a convex set and the "extreme points", so that unconfuses me a little: in order for a qutrit mixed state to qualify as a qutrit pure state, it has to be not just a boundary point but an extreme point.

then i oscillate a bit more between confused and unconfused as i try to understand the whole boundary of the convex set of qutrit mixed states, and eventually i realize that i'd like a combinatorial understanding of the "walls" that form that boundary (if i can stretch the meaning of "wall" that far).

and then of course these "combinatorial walls" of the convex set of normalized density matrixes get reinterpreted as similar "combinatorial walls" of the convex cone of unnormalized density matrixes, and we start thinking about where the polarizations and principal polarizations (aka ample line bundles and "barely ample" line bundles) lie in relation to those walls.

so these are my rough guesses at the moment:

the pure states are the hermitian matrixes of signature +00, and there's 4 dimensions of them normalized or 5 dimensions unnormalized, and the corresponding line bundles have "polynomial sections" that are enough to separate points only up to some 1d quotient abelian variety.

then there are the hermitian matrixes of signature ++0, 7 dimensions normalized or 8 dimensions unnormalized, and the corresponding line bundles have polynomial sections that are enough to separate points up to some 2d quotient abelian variety, and these are "maximally non-ample" in some sense; needing just a slight push in the positive direction to hit a "barely ample" principal polarization right nearby.

and then there's the hermitian matrixes of signature +++ with the corresponding line bundles being the ample ones, 8 dimensions normalized or 9 dimensions unnormalized.

so is this anything like the way it really is? a 9d convex cone of unnormalized mixed states bounded by an 8d curved "wall" but with a 5d "corner", with the principal polarizations generally lying not near the 5d corner but rather on some 8d "non-quadratic generalized hyperboloid" near the wall, or something like that. this is in contrast to the qubit case where there's a 4d convex cone ("minkowski future cone") of unnormalized mixed states with a 3d curved boundary wall, with the principal polarizations lying on a 3d hyperboloid close to the wall.

or something like that .... feel free to point out any particularly horrific mistakes here ....

hmm, as a wild guess about the numerology, maybe the wall/corner dimension sequence for the next case up goes: 16,15,12,7, with the co-dimensions simply being square numbers ....

....

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 3, 2022 at 6:01 PM
To: john.baez@ucr.edu

i wrote:

"then there are the hermitian matrixes of signature ++0, 7 dimensions normalized or 8 dimensions unnormalized, and the corresponding line bundles have polynomial sections that are enough to separate points up to some 2d quotient abelian variety, and these are "maximally non-ample" in some sense; needing just a slight push in the positive direction to hit a "barely ample" principal polarization right nearby."

hmm, maybe i missed some obvious stuff here .... in particular that the "boundary"/"surface" of the cone is carved out by the equation saying that the discriminant of the quadratic form extracted from the hermitian form is zero. this seems suggestive in a lot of ways ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu>
Reply-To: baez@math.ucr.edu Thu, Mar 3, 2022 at 7:58 PM
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>

Hi -

> in contrast, a qutrit has 4 dimensions worth of pure states and 8 dimensions worth of mixed states, so the idea that the pure states might be the boundary of the space of mixed states fails, and already that confuses me a little.
>
> but then i realize that in general there's a big difference between the boundary points of a convex set and the "extreme points", so that unconfuses me a little: in order for a qutrit mixed state to qualify as a qutrit pure state, it has to be not just a boundary point but an extreme point.

Yes, this always confuses me too, mainly because I have trouble imagining these extreme points. I can certain imagine *some* examples of convex sets where the boundary has dimension much higher than the extreme points, like a simplex or hypercube. But CP^2 feels a lot more "round" than the corners of a simplex or hypercube.

I did a big study of this business with Greg Egan in the case of 3x3 matrices of octonions, btw. And I think we did the case of 3x3 matrices of complexes as a warmup. But it never feels completely intuitive.

> the pure states are the hermitian matrixes of signature +00, and there's 4 dimensions of them normalized or 5 dimensions unnormalized, and the corresponding line bundles have "polynomial sections" that are enough to separate points only up to some 1d quotient abelian variety.

I don't get the stuff about "have "polynomial sections" that are enough to separate points only up to some 1d quotient abelian variety", but the rest looks right. The normalized guys form CP^2, so they have dimension 4.

> then there are the hermitian matrixes of signature ++0, 7 dimensions normalized or 8 dimensions unnormalized, and the corresponding line bundles have polynomial sections that are enough to separate points up to some 2d quotient abelian variety, and these are "maximally non-ample" in some sense; needing just a slight push in the positive direction to hit a "barely ample" principal polarization right nearby.

That looks right, the dimension of the ++0 guys equals the dimension of the space of hermitian 3x3 matrices with determinant 0, which is the dimension of SU(3) which is 8. But I don't get "the corresponding line bundles have polynomial sections that are enough to separate points up to some 2d quotient abelian variety".

> and then there's the hermitian matrixes of signature +++ with the corresponding line bundles being the ample ones, 8 dimensions normalized or 9 dimensions unnormalized.

That's right, 9 is the dimension of the space of 3x3 hermitian matrices and the +++ guys are an open set in there.

> so is this anything like the way it really is? a 9d convex cone of unnormalized mixed states bounded by an 8d curved "wall" but with a 5d "corner", with the principal polarizations generally lying not near the 5d corner but rather on some 8d "non-quadratic generalized hyperboloid" near the wall, or something like that. this is in contrast to the qubit case where there's a 4d convex cone ("minkowski future cone") of unnormalized mixed states with a 3d curved boundary wall, with the principal polarizations lying on a 3d hyperboloid close to the wall.

This all sounds right to me.

This is related to what Greg and I thought about: in Minkowski spacetime CP^1 is your "sky" (or "heavenly sphere"), but in the more exotic spacetime given by 3x3 hermitian matrices there's a "big sky" - the projectization of the hermitian matrices with det = 0, and inside that a "sky within the sky", namely CP^2. We were thinking about different wave equations on these 3x3 hermitian matrix spacetimes; they give you different kinds of massless particles, and depending on which particles you can see, you can see either the big sky or just the sky within the sky. Or both, which would be cool.

And there are even more nested skies when we go to nxn hermitian matrices with bigger n.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 3, 2022 at 10:38 PM
To: John Baez <baez@math.ucr.edu>

i wrote:

"then i oscillate a bit more between confused and unconfused as i try to understand the whole boundary of the convex set of qutrit mixed states, and eventually i realize that i'd like a combinatorial understanding of the "walls" that form that boundary (if i can stretch the meaning of "wall" that far)."

and then later you wrote:

"Yes, this always confuses me too, mainly because I have trouble imagining these extreme points. I can certain imagine *some* examples of convex sets where the boundary has dimension much higher than the extreme points, like a simplex or hypercube. But CP^2 feels a lot more "round" than the corners of a simplex or hypercube."

i had originally written a longer version where i described more of the oscillations, and what you're describing here sounds very much like some of those further oscillations. so now that you've brought it up i feel more free to wallow in it a bit more ....

so, i eventually realized that even in relatively low visualizable dimensions you can have a nice connected and "round"ish space of extreme points, and then a jump up to the dimension of the whole convex set. the example that i think helped me was the 3d convex hull of the 1d boundary of an idealized saddle.

(in some way this vaguely reminds me of stylized pictures of "crosscaps" in old-fashioned math-enrichment books. at some point i created some actual computer graphics of 3d crosscap projections, and in the process gained more respect for those old-fashioned stylized pictures of them.)

you wrote:

"I don't get the stuff about "have "polynomial sections" that are enough to separate points only up to some 1d quotient abelian variety",

well, ampleness = success in separating points in a certain way (namely via sections of positive tensor powers; i tentatively decided that it's reasonable to refer to this as "via polynomial sections" though i didn't think about it too hard), and failure in this regard isn't always abject; partial credit can be awarded for separating the points of some quotient variety.

in my follow-up message i switched gears from thinking about the "interior" of the cone as a convex set to thinking about the "boundary" of the cone as (something that can be analytically continued to) a singular algebraic variety. this fits with the algebro-geometric theme of the neron-severi group of an abelian variety, and it suggests all sorts of further ideas: that maybe we should try to interpret the ampleness cone of a projective variety as a jordan cone even when the variety isn't abelian, and that in the case where the variety is a surface we might get the special-relativistic clifford-jordan cones; and that in the case where the variety is a flag variety we might get something related to the "coxeter discriminant" of the corresponding simple group; and that the "non-quadratic generalized hyperboloid" where the principal polarizations live looks as though it relates to some sort of "resolution" of the singularity; and so forth. still very speculative at this point though ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Thu, Mar 3, 2022 at 11:07 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> you wrote:
>
> "I don't get the stuff about "have "polynomial sections" that are enough to separate points only up to some 1d quotient abelian variety",
>
> well, ampleness = success in separating points in a certain way (namely via sections of positive tensor powers; i tentatively decided that it's reasonable to refer to this as "via polynomial sections" though i didn't think about it too hard), and failure in this regard isn't always abject; partial credit can be awarded for separating the points of some quotient variety.

Okay, that makes sense; so how did you show (or guess) the dimension of the quotient variety here?

Maybe you know: the graded ring of sections of all the tensor powers of a line bundle is called the "pluricanonical ring"... when the line bundle is the canonical bundle. But people study this ring a bunch for arbitrary line bundles. I have big hopes for this sort of ring in quantum physics.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Fri, Mar 4, 2022 at 12:02 AM

"Okay, that makes sense; so how did you show (or guess) the dimension of the quotient variety here?"

perhaps just a guess, but doesn't it seem like a reasonable guess? i'm talking about in the case of the cartesian cube of an elliptic curve, which has lots of cartesian-product decompositions from which you can get lots of quotient abelian varieties, i think. like with the case of the square of an elliptic curve, where we had that theta function dimension numerology something like this:

```
1  1  2  3  4  5  .  .  .
1  1  2  3  4  5
2  2  4  6  8 10
3  3  6  9 12 15
4  4  8 12 16 20
5  5 10 15 20 25
.
.
.
```

the first row and the first column here are too "edgy" to give ample line bundles; the first column fails to separate points in the horizontal cartesian factor while the first row fails to separate points in the vertical cartesian factor. thus the "barely ample" principal polarization here is (i think) where that second "1" on the main diagonal lies, safely in the interior of the cone.

(i guess i'm tempted to mumble something about "segre embedding" here ....)

"Maybe you know: the graded ring of sections of all the tensor powers of a line bundle is called the "pluricanonical ring"... when the line bundle is the canonical bundle. But people study this ring a bunch for arbitrary line bundles. I have big hopes for this sort of ring in quantum physics."

so far the only "pluri-" thing i feel like i'm close to understanding is that "plurisubharmonic" stuff, which seems very interesting but which i suspect is pretty much completely unrelated .... i agree though that getting graded rings in this way is conceptually very interesting ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Fri, Mar 4, 2022 at 8:39 AM

you wrote:

"Okay, that makes sense; so how did you show (or guess) the dimension of the quotient variety here?"

and i replied:

perhaps just a guess, but doesn't it seem like a reasonable guess?

it's supposed to be a simple-minded analog of how hermitian signature +00 means "pure", ++0 means "slightly adulterated", and +++ means "thoroughly adulterated" in the context of statistical mixtures of quantum states ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Fri, Mar 4, 2022 at 9:05 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

On Fri, Mar 4, 2022 at 8:39 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> you wrote:
>
> "Okay, that makes sense; so how did you show (or guess) the dimension of the quotient variety here?"
>
> and i replied:
>
> perhaps just a guess, but doesn't it seem like a reasonable guess?

Okay, I like that rhetorical strategy.

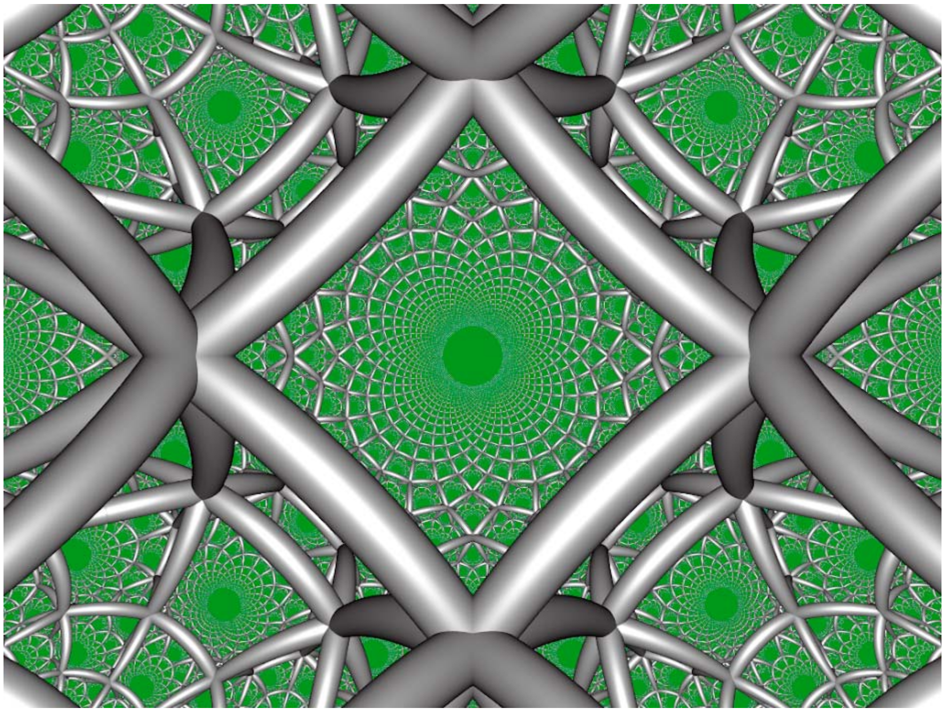
> it's supposed to be a simple-minded analog of how hermitian signature +00 means "pure", ++0 means "slightly adulterated", and +++ means "thoroughly adulterated" in the context of statistical mixtures of quantum states ....

Okay, I'll agree that *if* the dimension of the quotient variety whose points are separated by the "plurisections" of your line bundle depends only on whether the bundle is of type 000, +00, ++0 or +++, this dimension has to be 0,1,2,3 depending on this type. (That's easy to prove using the stuff you said.)

So the real question is whether the dimension depends only on the type.

I think I made a mistake related to this a while back. Let's go back to an abelian surface whose Neron-Severi group has rank 4. It's sitting in a Minkowski spacetime where the metric g comes from the intersection pairing. I guessed that a polarization P is principal iff g(P,P) = 2. This made me happy because it reminded me of "roots" in the theory of Lie algebras. But I don't think it's true!

Thanks to the discretized Lorentz group action, there will often be infinitely many points P in the future cone with g(P,P) = 2, forming a nice picture related to this:



But I've read that there are only finitely many principal polarizations, and it makes a lot of sense if you think about it. Right?

So, it seems the dimension of the space of sections of a polarization can't be "Lorentz-invariant" - invariant under the discrete subgroup of the Lorentz group that acts on the Neron-Severi lattice.

So, in the abelian 3-fold case, I'm a bit wary of claims that stuff about polarizations depends only on SL(3,C)-invariants like the "type" 000, +00, ++0 or +++.

Or at least I want to understand this issue: what about polarizations is invariant under the action of the "generalized Lorentz group" and what is not?

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 4, 2022 at 10:01 AM
To: John Baez <baez@math.ucr.edu>

"But I've read that there are only finitely many principal polarizations, and it makes a lot of sense if you think about it. Right?"

let me think about it a bit!

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Fri, Mar 4, 2022 at 11:33 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

On Fri, Mar 4, 2022 at 10:02 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> "But I've read that there are only finitely many principal polarizations, and it makes a lot of sense if you think about

> it. Right?"
>
> let me think about it a bit!

Now it seems completely wrong to me. I'm not sure where I read this....

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 4, 2022 at 12:25 PM
To: John Baez <baez@math.ucr.edu>

"This is related to what Greg and I thought about: in Minkowski spacetime CP^1 is your "sky" (or "heavenly sphere"), but in the more exotic spacetime given by 3x3 hermitian matrices there's a "big sky" - the projectization of the hermitian matrices with det = 0, and inside that a "sky within the sky", namely CP^2. We were thinking about different wave equations on these 3x3 hermitian matrix spacetimes; they give you different kinds of massless particles, and depending on which particles you can see, you can see either the big sky or just the sky within the sky. Or both, which would be cool."

i have to think about this some more ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 4, 2022 at 1:01 PM
To: John Baez <baez@math.ucr.edu>

"Now it seems completely wrong to me. I'm not sure where I read this...."

right, i think you were right (or at least much closer to right) the first time ....

i was going to suggest that maybe you could get that book (or whatever it was) to say instead that there's only finitely many orbits of principal polarizations under the action of the symmetry group; that might be semi-plausible on the basis of my limited knowledge.

i was also going to reiterate that that big/ish "discrete lorentzian" symmetry group (in the special/ish cases that we often think about) isn't just a symmetry group of the neron-severi lattice but rather of the unpolarized abelian surface itself, so "everything" has to be invariant under that group .... that was in response to my attempted interpretation of where you wrote "So, it seems the dimension of the space of sections of a polarization can't be "Lorentz-invariant" - invariant under the discrete subgroup of the Lorentz group that acts on the Neron-Severi lattice".

(we still have the issue of to what extent section-space dimensions depend on the continuous parameter (picard variety) as opposed to the discrete parameter (neron-severi group) in general; i still get slightly confused about that but i doubt it's a significant issue here ....)

i was also wondering whether you might have been reading something focusing on different special cases than the ones we've been tending towards ....

also i'm pretty sure i've read (and/or confirmed for myself somehow) that symmetry groups of complex abelian varieties are definitely sometimes infinite before imposing a polarization, but always finite after imposing one. doesn't that give an abstract-nonsense pigeonhole/ish proof that the orbit of a principal polarization under that big/ish symmetry group must sometimes be infinite? maybe i'm coming close to repeating myself here though.

also, however, i was going to claim that we've already implicitly been giving pretty good answers to that question i raised earlier:

"by the way, one of the mini-projects i'd really like to clarify here is how to explicitly take one of those neron-severi jordan-self-adjoints and explicitly turn it into a textbook "riemann form"! assuming that that makes sense ...."

and that that helps in resolving the current issue, because in the "neron-severi jordan-self-adjoints" picture it seems pretty obvious that there's an infinite number of principal polarizations, i think. but anyway we should still try to make our implicit solutions of that mini-project more explicit and outloud .... i hope i'll get around to that soon ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Fri, Mar 4, 2022 at 3:15 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> i was going to suggest that maybe you could get that book (or whatever it was) to say instead that there's only finitely many orbits of principal polarizations under the action of the symmetry group; that might be semi-plausible on the basis of my limited knowledge.

That sounds better.

> i was also going to reiterate that that big/ish "discrete lorentzian" symmetry group (in the special/ish cases that we often think about) isn't just a symmetry group of the neron-severi lattice but rather of the unpolarized abelian surface itself,

Right - I re-realized that before saying my claim about finitely many principal polarizations seemed completely wrong. Since the group is infinite in these special-ish case, the only way there could be finitely many principal polarizations is if they all had infinite stabilizers. But that seems ludicrously unlikely.

If you think of them as Kahler metrics on the abelian variety they *do* have an infinite symmetry group, namely the translation group. But I think I can show that beyond this, their symmetries can only form a finite group. The idea is to look at symmetries that fix a point. By looking at the tangent space of this point, you can see these symmetries form a subgroup of some U(n). So they form a compact group. But in fact only a finite subgroup of this U(n) can be acting as automorphisms of the abelian variety... I'm gettig tired of explaining this, you can probably see it.

> so "everything" has to be invariant under that group .... that was in response to my attempted interpretation of where you wrote "So, it seems the dimension of the space of sections of a polarization can't be "Lorentz-invariant" - invariant under the discrete subgroup of the Lorentz group that acts on the Neron-Severi lattice".

Right. That claim was wrong.

> (we still have the issue of to what extent section-space dimensions depend on the continuous parameter (picard variety) as opposed to the discrete parameter (neron-severi group) in general; i still get slightly confused about that but i doubt it's a significant issue here ....)

I would bet they don't at all.

> i was also wondering whether you might have been reading something focusing on different special cases than the ones we've been tending towards ....

I don't know.

> also i'm pretty sure i've read (and/or confirmed for myself somehow) that symmetry groups of complex abelian varieties are definitely sometimes infinite before imposing a polarization, but always finite after imposing one.

You mean aside from the translation symmetries?

> doesn't that give an abstract-nonsense pigeonhole/ish proof that the orbit of a principal polarization under that big/ish symmetry group must sometimes be infinite? maybe i'm coming close to repeating myself here though.

Yeah. I wrote the stuff above before reading this, btw: we're on the same page.

> also, however, i was going to claim that we've already implicitly been giving pretty good answers to that question i raised earlier:
>
> "by the way, one of the mini-projects i'd really like to clarify here is how to explicitly take one of those neron-severi jordan-self-adjoints and explicitly turn it into a textbook "riemann form"! assuming that that makes sense ...."
>
> and that that helps in resolving the current issue, because in the "neron-severi jordan-self-adjoints" picture it seems pretty obvious that there's an infinite number of principal polarizations, i think. but anyway we should still try to make our implicit solutions of that mini-project more explicit and outloud .... i hope i'll get around to that soon ....

Speaking of Riemann forms, in his book *Abelian Varieties* Mumford uses them to get the Rosati involution - that is, the star-ring structure on End(X) where X is an abelian variety equipped with a polarization.

He notices End(X) includes into End(V) where V is the vector space that's the universal cover of X. But a polarization on X gives a Riemann form on V, whose hermitian version makes V into a complex inner product space. So we can really think of End(X) as a subalgebra of End(V), which is a star-algebra in the usual way.

Well, I guess I don't see why End(X) is closed under the star operation. But for some reason it is - and this makes it into a star-ring!

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 4, 2022 at 3:31 PM
To: John Baez <baez@math.ucr.edu>

me: "also i'm pretty sure i've read (and/or confirmed for myself somehow) that symmetry groups of complex abelian varieties are definitely sometimes infinite before imposing a polarization, but always finite after imposing one."

you: "You mean aside from the translation symmetries?"

i think so. i think i'm going to claim that i was talking about group automorphisms of abelian varieties.

you: "Speaking of Riemann forms, in his book Abelian Varieties Mumford uses them to get the Rosati involution ...."

sorry, i'm temporarily hands-over-ears on this one till i try clearing it up for myself-- hopefully very soon! but thanks for suggesting this as another reference to look at.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 4, 2022 at 3:40 PM
To: John Baez <baez@math.ucr.edu>

me: "(we still have the issue of to what extent section-space dimensions depend on the continuous parameter (picard variety) as opposed to the discrete parameter (neron-severi group) in general; i still get slightly confused about that but i doubt it's a significant issue here ....)"

you: "I would bet they don't at all."

i had been betting that (at least for abelian varieties) until i started thinking more carefully about sections of the trivial bundle. i'm hoping that my change-of-mind about it is a sign that i'm getting better at understanding that continuous parameter.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Fri, Mar 4, 2022 at 7:08 PM

me: "i had been betting that (at least for abelian varieties) until i started thinking more carefully about sections of the trivial bundle. i'm hoping that my change-of-mind about it is a sign that i'm getting better at understanding that continuous parameter."

i guess this is one of those things where (as we've probably previously hinted) i'm hoping that the answer comes out smoother (and in particular independent of the continuous parameter) only if you apply some sort of "euler-riemann-roch correction".

also, i vaguely hope that this connects somehow with the "borel-weil-bott theorem" and more particularly with the "bott" part of it, in that the cone where the ample line bundles lie might be just one of multiple "chambers" carved out by the analytic continuation of the boundary of the main chamber ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu>
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Tue, Mar 8, 2022 at 10:19 AM

Hi -

By the way, if X is an abelian surface, I think End(X) tensored with the rationals can be, not only the thing we talk about all the time (a subring of 2x2 complex matrices), but also a subring of the quaternions.

And this is nicely connected to the Rosati involution! The self-adjoints in the 2x2 complex matrices form a 4d real space. The self-adjoints in the quaternions form just a 1d space.

So if we're looking for 1d Neron-Severi groups, the quaternions may come in handy.

Now that I know this stuff I want to revisit our old (and somewhat forgotten) friend, the abelian surface given by the quaternions mod the Hurwitz quaternions. The Hurwitz quaternions act by right multiplications on this surface, preserving the complex structure given by left multiplication by the quaternion i. So End(X) is at least the Hurwitz quaternions.

But I think the Gaussian integers also act by left multiplications. So I seem to be guessing that End(X) is

Hurwitz direct sum Gauss

and then the Neron-Severi group for the "obvious guess" Rosati involution would be Z + Z.

Best,
jb

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