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John Baez <johnb@ucr.edu>

minkowski and lattices

65 messages

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Feb 11, 2022 at 5:27 AM
To: john.baez@ucr.edu

you probably know that i have a not very falsified rule of thumb that says that when a mathematician seems to be famous for two different things, closer examination tends to show that they're not as different as they seem.

offhand i'm still guessing that minkowski might be a sort of exception, in that they weren't consciously calling on their work on lattices in number theory ("minkowski's theorem" that founded the so-called "geometry of numbers") when they did their work on minkowski space; but from our current discussion about the "minkowski structure" on the neron-severi lattice of an abelian surface, i'm getting less sure about that.

(well, i just did a quick search on "minkowski" + "abelian surface" but i didn't immediately see any relevant hits.)

but anyway, i'm realizing now that what you were telling me yesterday about large-ish lattice-preserving subgroups of so(3,1) such as sl(2, gaussian integers) and sl(2, eisenstein integers) seems indeed very relevant to the study of anomalously symmetric abelian surfaces (and concomitantly also to the study of anomalously symmetric genus 2 curves).

i think that generically the automorphism group of an abelian surface (preserving the abelian group structure and thus a basepoint, so not including translations; but not necessarily preserving a polarization) is just z/2, generated by the involution "-1" that you mod out by to get the kummer surface. special abelian surfaces can have anomalous extra symmetry, though, and that extra symmetry gets functorially inherited by their "minkowski-structured" neron-severi lattices; so we should try to work out this rough correspondence between anomalously symmetric abelian surfaces and anomalously symmetric minkowski-structured lattices.

(of course we can come up with all sorts of other good conceptual motivations that i might try to tell you more about later for being interested in anomalously symmetric abelian surfaces, but we don't have to worry about that yet.)

so here's a brief stab at surveying the kinds of anomalous extra symmetry that an abelian surface can have ....

first of all, the product of two different elliptic curves is an abelian surface, and this gives us a 2-complex-dimensional substack of the 3-complex-dimensional moduli stack of abelian surfaces. as far as i can tell these product surfaces have just a bit of extra symmetry, with klein-4 symmetry coming from being able to apply -1 separately in each factor. of course each factor could itself have a bit of extra symmetry if it's a gauss or eisenstein elliptic curve.

then there's the 1-dimensional substack of the moduli stack given by the abelian surfaces which are squares of elliptic curves. these have _a lot_ of extra symmetry, generically sl(2,z) i think. that can go up to sl(2,gaussian integers) for the square of the gaussian elliptic curve and up to sl(2,eisenstein integers) for the square of the eisenstein elliptic curve.

so this _is_ sounding reminiscent of your account of the anomalously symmetric minkowski-structured lattices, i think. this makes me want to see this rough correspondence polished up a bit ....

there are some isolated other anomalously symmetric points in the moduli stack, for example ones coming from the algebraic integers in the 5th cyclotomic field or in the 12th cyclotomic field, i think. i don't remember you mentioning anything on the minkowski side that might correspond to those, but there should be something interesting there, i think.

(also, what _about_ the d4/tony-smith-flavored example or examples?)

then there's also the issue of how the possibly infinite symmetry group of an abelian surface allegedly always shrinks to a finite subgroup when you put a polarization structure on it. this corresponds to how the possibly infinite symmetry group of a minkowski-structured spacetime shrinks to a finite subgroup when you pick a minkowski observer (thus getting a "space" from the "spacetime"). picking a polarization on an abelian surface is closely related to making it the jacobian of a genus 2 curve, so this is related to how the symmetry group of a genus 2 curve relates to the mckay correspondence ....

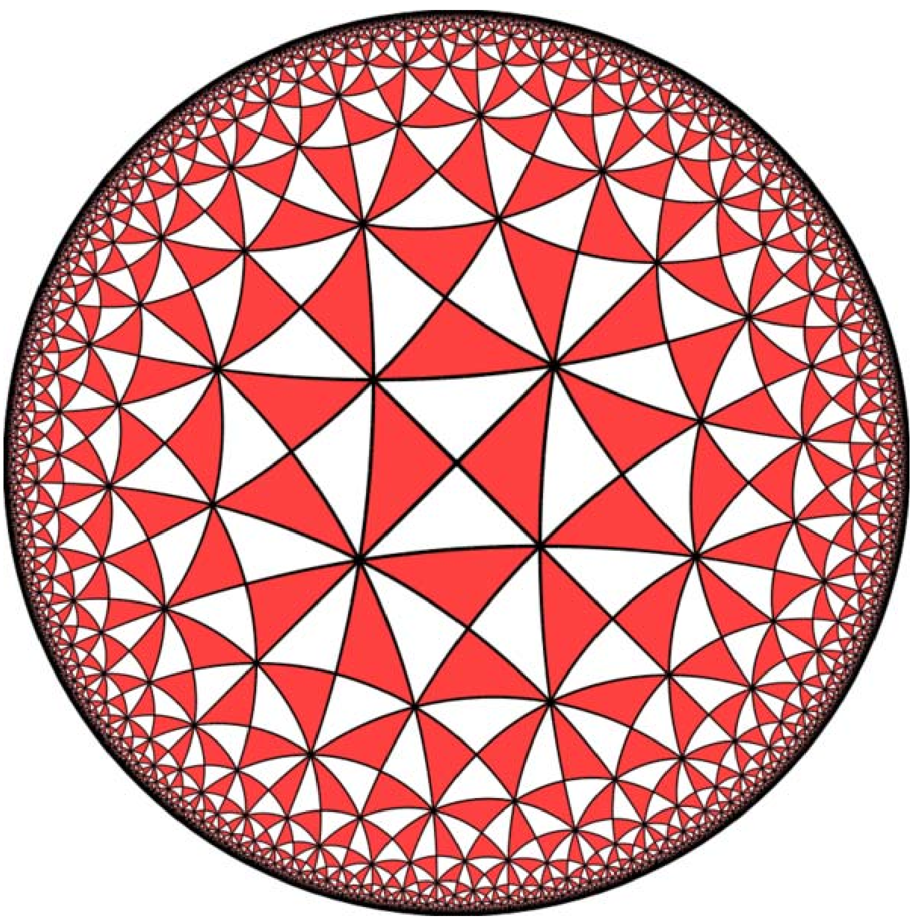
John Baez <john.baez@ucr.edu> Fri, Feb 11, 2022 at 10:12 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>

Hi -

That's all really interesting and I'll need to think about it more.

I remember that I read about some relevant stuff in Quadratic integers and Coxeter groups by Norman W. Johnson and Asia Ivic Weiss.

They say a lot about PSL_2(G) and PSL_2(E), where G is the Gaussian integers and E is the Eisenstein integers. They mainly talk about how these groups are the even parts of Coxeter groups (that is, the parts generated by pairs of reflections.) They also talk about how they preserve hyperbolic honeycombs, 3d analogues of things like this:



They give some history, which shows a lot of cool guys studied this stuff:

> Felix Klein [16, pp. 120–121] proved that PSL_2(Z) (the "modular group") is isomorphic to the group of rotations of the regular hyperbolic tessellation {3,∞}. Emile Picard [26] considered the analogous group PSL_2(G) (the "Picard group"). Luigi Bianchi [2], [3] showed that if D is an imaginary quadratic integral domain, the group PSL_2(D) acts discontinuously on hyperbolic 3-space. Fricke & Klein [10, pp. 76–93] identified

> PSL_2(G) with a subgroup of the rotation group of the regular honeycomb {3, 4, 4}.

They're using "rotation group" to mean what I called the "even subgroup" of a Coxeter group.

The term "Picard group" is pretty dangerous!

The Eisenstein case has a less noble pedigree (much as Eisenstein is less of a bigshot than Gauss):

> Benjamin Fine [8, chap. 5] undertook a thorough algebraic treatment of PSL_2(G). Fine & Newman [9] investigated normal subgroups of PSL_2(G), and Roger Alperin [1] did likewise for PSL_2(E). Schulte & Weiss [29, p. 246] showed that PSL_2(E) can be identified with a subgroup of the rotation group of {3, 3, 6}.

I need to figure out why PSL_2(G) and PSL_2(E) are just *subgroups* of the even subgroups of Coxeter groups. And I should figure out how this 3d hyperbolic honeycomb picture is connected to the 4d lattice picture. Obviously the 3d hyperbolic space can be seen as one sheet of a hyperboloid in the 4d Minkowski space, but where does the honeycomb come from?

Best,
jb

[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Feb 11, 2022 at 11:43 AM
To: John Baez <baez@math.ucr.edu>

"They say a lot about PSL_2(G) and PSL_2(E), where G is the Gaussian integers and E is the Eisenstein integers. They mainly talk about how these groups are the even parts of Coxeter groups (that is, the parts generated by pairs of reflections.)"

did you change your mind about that further down below? i should read your whole email more carefully. either way though it seems very interesting ....

i need to think more about all of this ....

by the way, i tried searching on "image of the chow group" (on the theory that the image of the nth chow group in integral cohomology might be interesting (assuming that it actually makes sense in the first place). sort of seemed to give interesting results ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Feb 11, 2022 at 11:48 AM
To: John Baez <baez@math.ucr.edu>

"And I should figure out how this 3d hyperbolic honeycomb picture is connected to the 4d lattice picture. Obviously the 3d hyperbolic space can be seen as one sheet of a hyperboloid in the 4d Minkowski space, but where does the honeycomb come from?"

well, i don't know, yet, but this all sounds promising ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Feb 11, 2022 at 12:20 PM
To: John Baez <baez@math.ucr.edu>

"And I should figure out how this 3d hyperbolic honeycomb picture is connected
to the 4d lattice picture. Obviously the 3d hyperbolic space can be seen as one sheet of a
hyperboloid in the 4d Minkowski space, but where does the honeycomb come from?"

don't quote me on this yet, but i have a vague memory that in certain circumstances similar to this, the "mirrors" are essentially just points in a dual lattice .... or something like that ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Feb 11, 2022 at 2:14 PM
To: John Baez <baez@math.ucr.edu>

it's also annoying that i missed the idea that the automorphism group of the square of an elliptic curve with complex multiplication by D is sl(2,D) (or something like that) even when D is other than the gaussian or eisenstein integers ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Fri, Feb 11, 2022 at 2:36 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

On Fri, Feb 11, 2022 at 11:43 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> "They say a lot about PSL_2(G) and PSL_2(E), where G is the Gaussian integers and
> E is the Eisenstein integers. They mainly talk about how these groups are the even parts
> of Coxeter groups (that is, the parts generated by pairs of reflections.)"
>
> did you change your mind about that further down below?

Yes, it turns out

PSL_2(G) is a subgroup of index 2 in the even part of the Coxeter group [3, 3, 4],

and

PSL_2(E) is a subgroup of index two in the even part of the Coxeter group [3, 3, 6].

I bet I understand this, though I haven't proved my guess. Remember how I reminded you that
PSL_2(C) is just a subgroup of index two in SO(3,1)? I think this is just like that.

O(3,1) has four components, containing

the identity
time reversal
space reversal
time reversal x space reversal

SO(3,1) has two components, containing

the identity
time reversal x space reversal

PSL_2(C) has just one component.

I think this whole story works for the Coxeter groups [3, 3, 4] and [3, 3, 6] except that we can't
talk about "components". The Coxeter group has an "even part" of index two, which in turn has

a subgroup of index two which is PSL_2(G) or PSL_2(E).

Best,
jb

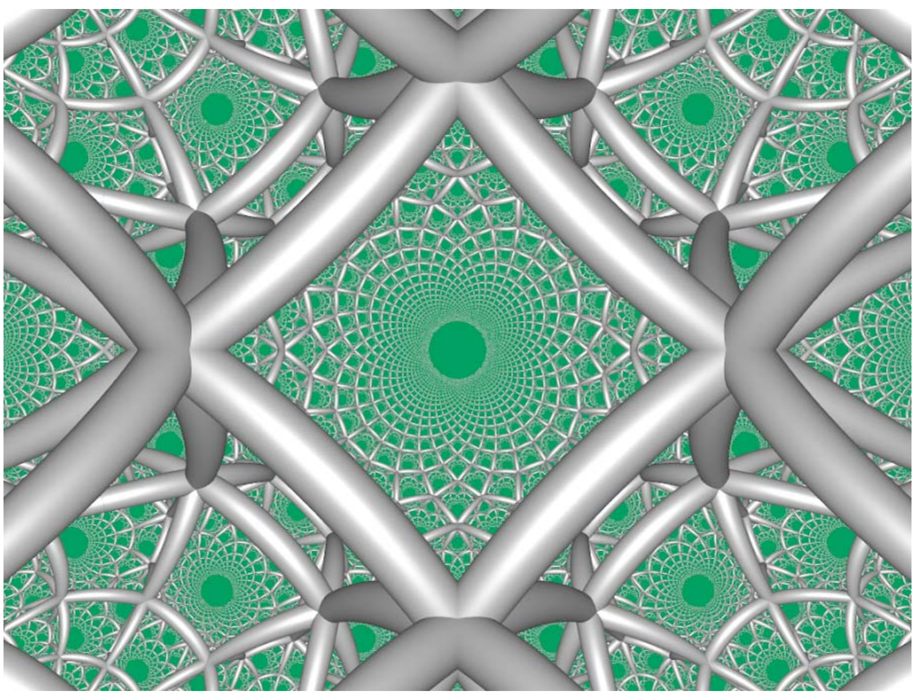
John Baez <john.baez@ucr.edu> Fri, Feb 11, 2022 at 2:56 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

Typo alert:

> Yes, it turns out
>
> PSL_2(G) is a subgroup of index 2 in the even part of the Coxeter group [3, 3, 4] - should be [3, 4, 4]
>
> and
>
> PSL_2(E) is a subgroup of index two in the even part of the Coxeter group [3, 3, 6].

[3, 4, 4] or really the isomorphic group [4, 4, 3] acts as symmetries of this hyperbolic honeycomb, called the square tiling honeycomb:



This is a "paracompact" honeycomb, since it's more infinite than the kind we usually think about: each edge touches four tilings of the plane by squares. These must be connected to the Gaussian integers.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 13, 2022 at 7:48 AM
To: John Baez <baez@math.ucr.edu>

[part 1 of n]

i want to write a long response here including comments of varying brow-level, with the lower-brow stuff tending more towards "look at the nice pictures of hyperbolic 3-space tilings" and the higher-brow stuff tending more towards "look at the nice pictures of hyperbolic cross-sections of neron-severi lattices of anomalously symmetric abelian surfaces". (of course we're interested in arching even higher than that but that's perhaps high enough for now.) however in order to have a chance of actually sending you any response at all i'll have to break it up into smaller parts, and this is part 1, which is just a very preliminary lowbrow throat-clearing where i mostly just try to reconstruct what is this "[a,b,c]" notation for 3-space tilings that you're using. presumably that notation is explainable by following the weblinks in your email, for example, but i'll understand it better if i reconstruct it for myself.

so, okay, i guess i get how the notation works: the coxeter diagrams that you're alluding to have 4 "fence-posts" but just 3 "fence-panels" in between them, and that's why you give a list of just 3 numbers to identify the coxeter diagram.

so for example, you say that sl(2,gaussian integers) is related somehow to [3,4,4]. so the initial segment [3,4] here is giving me octahedrons, and then i should tile 3-space by those with 4 of them sharing each edge; is that how it works?

on the other hand if i poincare-dualize by working right-to-left instead of left-to-right, then the final segment [4,4] is giving infinite 2-space tilings which look suspiciously just like the gaussian integers themselves; each such tiling enclosing a non-compact region of hyperbolic 3-space, with 3 of the tilings sharing each edge.

and then it gets even more suspicious when we look at the eisenstein case which is supposed to be related to [3,3,6]. left-to-right we're just tiling 3-space by tetrahedrons 6 of which share each edge, but right-to-left we're tiling it by non-compact regions whose boundaries look ultra-suspiciously like the eisenstein integers themselves, with 3 such regions sharing each edge.

so, hmm, maybe there's a very straightforward pattern here which extends to the case of sl(2,D) for D a pretty arbitrary quadratic number ring, just making some minor allowances for how the tiling of D itself is slightly less symmetric in general than it is in the special cases where D is the gaussian or eisenstein integers. something like, hyperbolic 3-space gets tiled by non-compact regions whose boundaries look somewhat like D itself, with 3 such regions sharing each edge.

but why (seeking either lowbrow or highbrow answers) should something like that work? D^2 is the period lattice over in the "spinors", but then how does D itself manage to manifest in a curvilinearly distorted way over in an energy hyperboloid of the minkowski spacetime corresponding to those spinors?

[hopefully i'll actually get around to actually sending you part j for some j > 1.]

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 13, 2022 at 8:14 AM
To: John Baez <baez@math.ucr.edu>

[part 1.1: some typo-issues]

so somewhere above you posted a "typo-alert", and i'm hoping that this actually gives us an opportunity to check that we're getting the same answers to certain questions. however, i think that your typo-alert may have accidentally re-committed more or less the same typo that you were alerting us to, or else i myself have screwed up in some way, so i'm not sure how much straightening-out we still might have to do here.

so, one piece of good news is that after i wrote part 1 of my response, i noticed that part of what i wrote sounded very similar (modulo a possible typo) to part of what you wrote:

i wrote:

"on the other hand if i poincare-dualize by working right-to-left instead of left-to-right, then the final segment [4,4] is giving infinite 2-space tilings which look suspiciously just like the gaussian integers themselves; each such tiling enclosing a non-compact region of hyperbolic 3-space, with 3 of the tilings sharing each edge."

whereas you wrote:

"This is a "paracompact" honeycomb, since it's more infinite than the kind we usually think about: each edge touches four tilings of the plane by squares. These must be connected to the Gaussian integers."

so can i talk you down from each edge touching four copies of the gaussian integers, to each edge touching just three such copies?

i suppose we should be able to resolve this question just by staring at the picture of the square tiling honeycomb from the wikipedia article, but apparently i haven't been staring at it for long enough because i can't quite see the answer yet.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 13, 2022 at 1:53 PM
To: John Baez <baez@math.ucr.edu>

[part 2]

in trying to learn about the minkowski structure on the neron-severi lattice of an abelian surface, we've been focusing so far on the anomalously symmetric case, in part just because of the photogenic nature of anomalously symmetric objects. there are also other reasons to be interested in anomalously symmetric abelian surfaces and their neron-severi lattices, but in some ways i'm even more interested in the more generic ones that lack extra symmetry. the first question to address here though is: how generic is generic?

that is: is there some nice way to express the constraints satisfied by those lattices in minkowski space that arise as neron-severi lattices of abelian surfaces?

the automorphism group of a 4d real vector space has 16 dimensions; that goes down to 6 if you put a minkowski structure on it, or down to zero if you put a lattice structure on it. so the space of orientations between minkowski structure and lattice structure here has 16-6 = 10 dimensions; whereas the moduli stack of abelian surfaces has 6 (real) dimensions. so there must be 10-6 = 4 dimensions worth of constraint between neron-severi lattice and minkowski structure; so then what are these constraints ???

the dimension of the moduli stack of (unpolarized) abelian surfaces here can itself be expressed as 8-2 = 6, where 8 is the dimension of the stabilizer of a complex structure on a 4d real vector space, and thus also the dimension of the space of orientations between complex structure and period lattice; and where abelian surfaces satisfy a 2-dimensional constraint between complex structure and period lattice that says that there's a symplectic structure that's compatible with both of them. i'm not sure to what extent that might help in trying to understand the 4d "compatiblity" constraint on orientation between minkowski structure and neron-severi lattice for an abelian surface.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 13, 2022 at 2:07 PM
To: John Baez <baez@math.ucr.edu>

[slight correction to part 2]

i wrote:

"the dimension of the moduli stack of (unpolarized) abelian surfaces here can itself be expressed as 8-2 = 6, where 8 is the dimension of the stabilizer of a complex structure on a 4d real vector space, and thus also the dimension of the space of orientations between complex structure and period lattice; and where abelian surfaces satisfy a 2-dimensional constraint between complex structure and period lattice that says that there's a symplectic structure that's compatible with both of them."

it's true that the dimension of the stabilizer here is 8, but what's more relevant is that the co-dimension of that stabilizer in the 16-dimensional ambient group (and thus also the dimension of the resulting homogeneous space) is also 8.

by the way it does annoy me that the abelian surface constraint between complex structure and period lattice is expressed in such an awkward way here, existentially quantifying over a symplectic structure ....

another inelegance in exposition here is that i didn't distinguish very carefully between the two opposite-flavored 4d real vector spaces in the story, namely the minkowski spacetime vs the "spinors".

....
[Quoted text hidden]

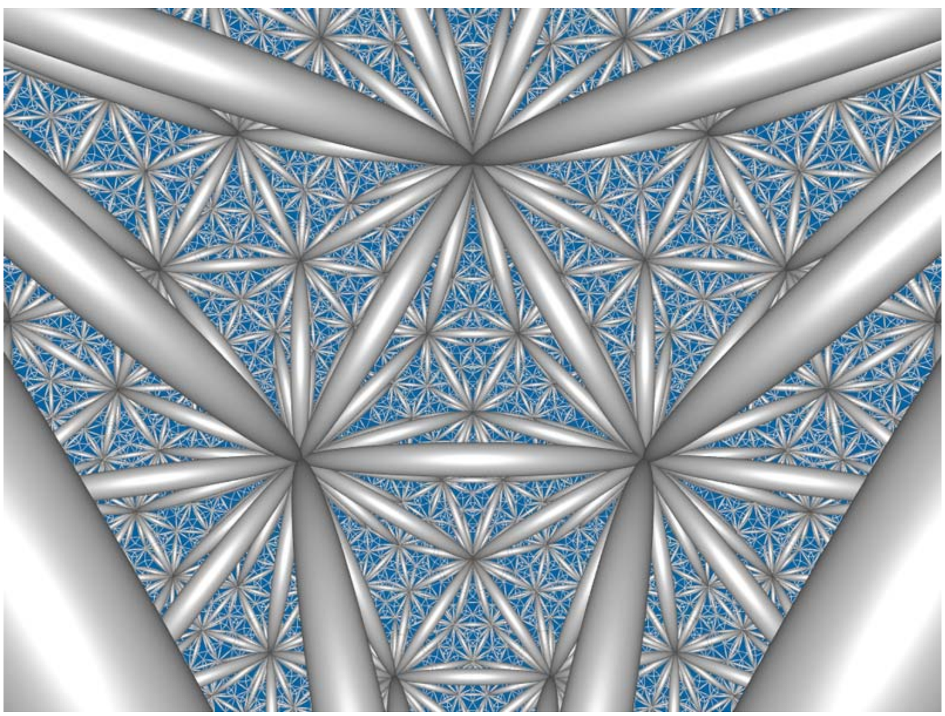
John Baez <john.baez@ucr.edu> Sun, Feb 13, 2022 at 2:12 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

This looks great! I only have time for a quick response now, and I haven't even read the whole email yet.

> so for example, you say that sl(2,gaussian integers) is related somehow to [3,4,4]. so the initial segment [3,4] here is giving me octahedrons, and then i should tile 3-space by those with 4 of them sharing each edge; is that how it works?

Yes, exactly - here's a picture of [3,4,4]:

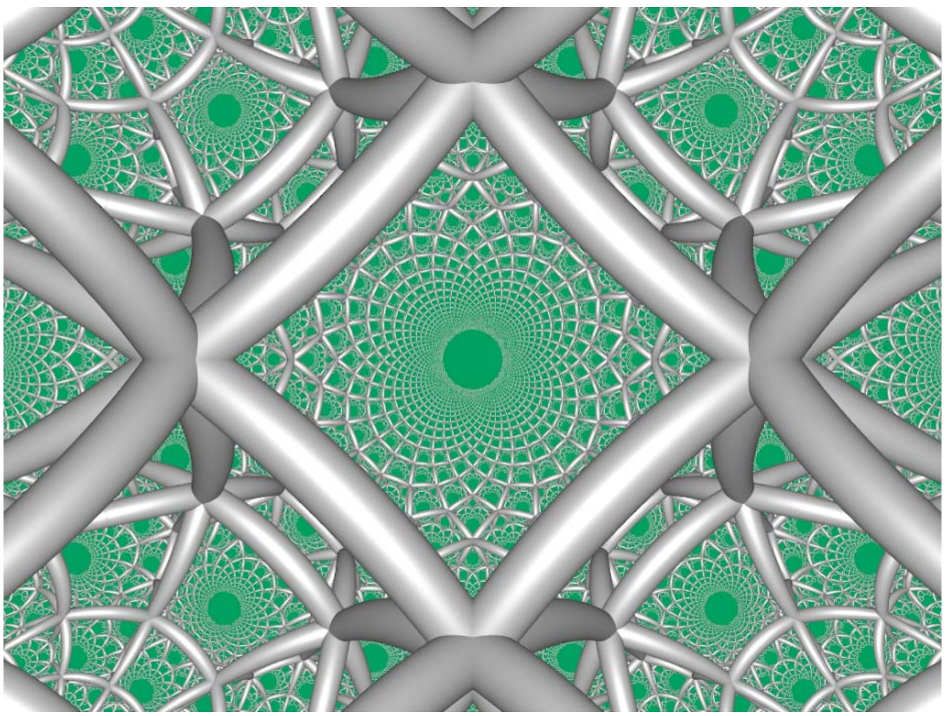


It's a bit busy, but apparently some of the octahedra's vertices are points at infinity, sort of like a 3d version of this general sort of thing:

Wikipedia calls [3,4,4] the "order-4 octahedral honeycomb":

https://en.wikipedia.org/wiki/Order-4_octahedral_honeycomb

A while back I showed you its dual, [4,4,3]:



This is the dual honeycomb, with an isomorphic Coxeter group.

PSL(2,gaussian integers) is a subgroup of index 4 in this Coxeter group, just as PSL(2,R) is an index-4 subgroup of O(2,1). There's a commutative square of groups here.

```
PSL(2,G) -----------> Coxeter group [3,4,4]
   |                       |
   V                       V
PSL(2,R)  ---------> O(2,1)
```

Best,
jb

John Baez <john.baez@ucr.edu> Sun, Feb 13, 2022 at 5:39 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 wrote:
>
> "on the other hand if i poincare-dualize by working right-to-left instead of left-to-right, then the final segment [4,4] is giving infinite 2-space tilings which look suspiciously just like the gaussian integers themselves; each such tiling enclosing a non-compact region of hyperbolic 3-space, with 3 of the tilings sharing each edge."
>
> whereas you wrote:
>
> "This is a "paracompact" honeycomb, since it's more infinite than the kind we usually think about: each edge touches four tilings of the plane by squares. These must be connected to the Gaussian integers."
>
> so can i talk you down from each edge touching four copies of the gaussian integers, to each edge touching just three such copies?

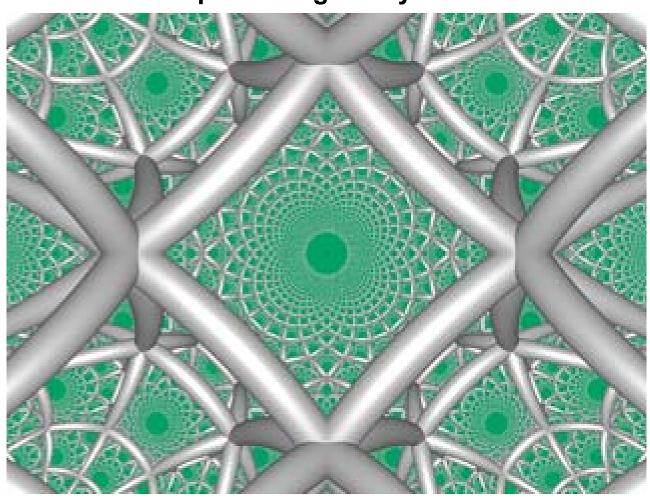
Yes, that was just a thinko. Wikipedia says 3, and that's what makes sense here.

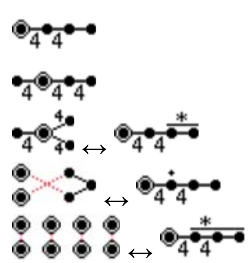
Best,
jb

Square tiling honeycomb

In the geometry of hyperbolic 3-space, the **square tiling honeycomb** is one of 11 paracompact regular honeycombs. It is called *paracompact* because it has infinite cells, whose vertices exist on horospheres and converge to a single ideal point at infinity. Given by Schläfli symbol {4,4,3}, it has three square tilings, {4,4}, around each edge, and six square tilings around each vertex, in a cubic {4,3} vertex figure.[

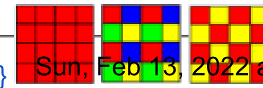
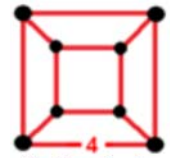
Square tiling honeycomb



Type	Hyperbolic regular honeycomb Paracompact uniform honeycomb
Schläfli symbols	{4,4,3} r{4,4,4} $\{4^{1,1,1}\}$
Coxeter diagrams	

John Baez <john.baez@ucr.edu>
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Sun, Feb 13, 2022 at 10:42 PM

Cells	{4,4} 
Faces	square {4}
Edge figure	triangle {3}
Vertex figure	 cube, {4,3}
Dual	Order-4 octahedral honeycomb
Coxeter groups	$\overline{R}_3$, [4,4,3] $\overline{N}_3$, [4^3] $\overline{M}_3$, [4^{1,1,1}]
Properties	Regular

Hi -

> so, okay, i guess i get how the notation works: the coxeter diagrams that you're alluding to have 4 "fence-posts" but just 3 "fence-panels" in between them, and that's why you give a list of just 3 numbers to identify the coxeter diagram.

Right, that's how this notation works. They use it on Wikipedia.

> so for example, you say that sl(2,gaussian integers) is related somehow to [3,4,4], so the initial segment [3,4] here is giving me octahedrons, and then i should tile 3-space by those with 4 of them sharing each edge; is that how it works?

Right.

> on the other hand if i poincare-dualize by working right-to-left instead of left-to-right, then the final segment [4,4] is giving infinite 2-space tilings which look suspiciously just like the gaussian integers themselves; each such tiling enclosing a non-compact region of hyperbolic 3-space, with 3 of the tilings sharing each edge.

Right.

I showed you pictures of these two dual things. Each has its own Wikipedia page, since there are some tiling and honeycomb fanatics there/

> and then it gets even more suspicious when we look at the eisenstein case which is supposed to be related to [3,3,6]. left-to-right we're just tiling 3-space by tetrahedrons 6 of which share each edge, but right-to-left we're tiling it by non-compact regions whose boundaries look ultra-suspiciously like the eisenstein integers themselves, with 3 such regions sharing each edge.

Right.

> so, hmm, maybe there's a very straightforward pattern here which extends to the case of sl(2,D) for D a pretty arbitrary quadratic number ring, just making some minor allowances for how the tiling of D itself is slightly less symmetric in general than it is in the special cases where D is the gaussian or eisenstein integers. something like, hyperbolic 3-space gets tiled by non-compact regions whose boundaries look somewhat like D itself, with 3 such regions sharing each edge.

There aren't "uniform hyperbolic honeycombs" in those cases, so I don't know what happens.

> but why (seeking either lowbrow or highbrow answers) should something like that work? D^2 is the period lattice over in the "spinors", but then how does D itself manage to manifest in a curvilinearly distorted way over in an energy hyperboloid of the minkowski spacetime corresponding to those spinors?

This may not be the right thing, but there's a way to take two spinors and "multiply" them to get a vector in Minkowski spacetime. In "week196" I wrote about how to square a spinor and get a lightlike vector. The Polish expert on spinors, Trautman, called this trick "Pythagorean spinors" in the case of the usual integers Z, because a lightlike 2+1-dimensional vector of integers is a Pythagorean triple. But now I want to think about it for the Gaussian and Eisensteinian cases:

> In 4 dimensions we do it like this: we take a left-handed spinor ψ, take its conjugate transpose to get a right-handed spinor ψ*, and form
>
> ψ ⊗ ψ*

which we can think of as a 2×2 hermitian matrix. If you're a fancy mathematical physicist you know that the space of 2×2 hermitian matrices is the same as 4d Minkowski spacetime, with the matrices of determinant zero corresponding to the lightlike vectors, so you're done! Otherwise, you can work out the above matrix explicitly:

```
ψ = (a)                                              (a column vector)
    (b)

ψ = (a*, b*)                                         (a row vector)


ψ ⊗ ψ* = (aa* ab*)                                   (a 2×2 matrix)
         (ba* bb*)
```

This matrix is hermitian, so you can write it as a real linear combination of Pauli matrices:

$$\psi \otimes \psi^* = t\, \sigma_t + x\, \sigma_x + y\, \sigma_y + z\, \sigma_z$$

where

$$\sigma_t = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}$$

$$\sigma_x = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}$$

$$\sigma_y = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}$$

$$\sigma_z = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}$$

You get a vector in Minkowski spacetime, (t,x,y,z). If you check that this vector is lightlike:

$$t^2 = x^2 + y^2 + z^2$$

you'll be done.

The trick in 3 dimensions is just the same except that now the components of ψ are real numbers, so things simplify: we don't need complex conjugation, and ψ ⊗ ψ* will be a *real* hermitian matrix. Real hermitian matrices are the same as vectors in 3d Minkowski spacetime, since we can write them as linear combinations of the three Pauli matrices without i's in them - namely, all of them except σ_y. So, we get a lightlike vector in 3d Minkowski spacetime: say, (t,x,z) with

$$t^2 = x^2 + z^2$$

Now for the really fun part: the trick works the same with Pythagorean spinors except now everything in sight is an integer...

... so (t,x,z) is a Pythagorean triple!!! And in fact, we get every Pythagorean triple this way, at least up to an integer multiple. And in fact, this trick was already known by Euclid.

Explicitly, if

$$\psi = \begin{pmatrix} a \\ b \end{pmatrix}$$

then

$$2\, \psi \otimes \psi^* = \begin{pmatrix} 2a^2 & 2ab \\ 2ab & 2b^2 \end{pmatrix}$$

$$= (a^2 + b^2)\, \sigma_t + 2ab\, \sigma_x + (a^2 - b^2)\, \sigma_z$$

so we get the Pythagorean triple

$$(t,x,z) = (a^2 + b^2,\ 2ab,\ a^2 - b^2)$$

> For example, if we take our spinor to be
>
> ```
> ψ = (2)
>     (1)
> ```
>
> we get the famous triple
>
> ```
> (t,x,z) = (5,4,3)
> ```
>
> By the way, you'll notice I had to insert a fudge factor of "2" in that formula up there to get things to work. I'm not sure why.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 4:23 AM
To: John Baez <baez@math.ucr.edu>

you wrote:

"This may not be the right thing, but there's a way to take two spinors and "multiply" them to get a vector in Minkowski spacetime."

that sounds interesting .... somewhat similar and/or different to what i've been trying .... not sure yet which if any is more on the right track ....

you also wrote:

"The Polish expert on spinors, Trautman, called this trick "Pythagorean spinors" in the case of the usual integers Z, because ...."

a vaguely-remembered phrase something like "integral spinors" has been rattling around in my mind recently presumably because i thought it might be related to all "this" stuff; now i'm considering the possibility that what you're describing here is what i was trying to remember.

before i really dive into reading what you wrote here, however, i'm going to keep trying to work out the ideas for myself a bit more, treating what you wrote as "back of the book" cheating material to be consulted only fleetingly. i'll try to let myself really dive into it before our next videochat, though, whenever that turns out to be; possibly a week from today (monday).

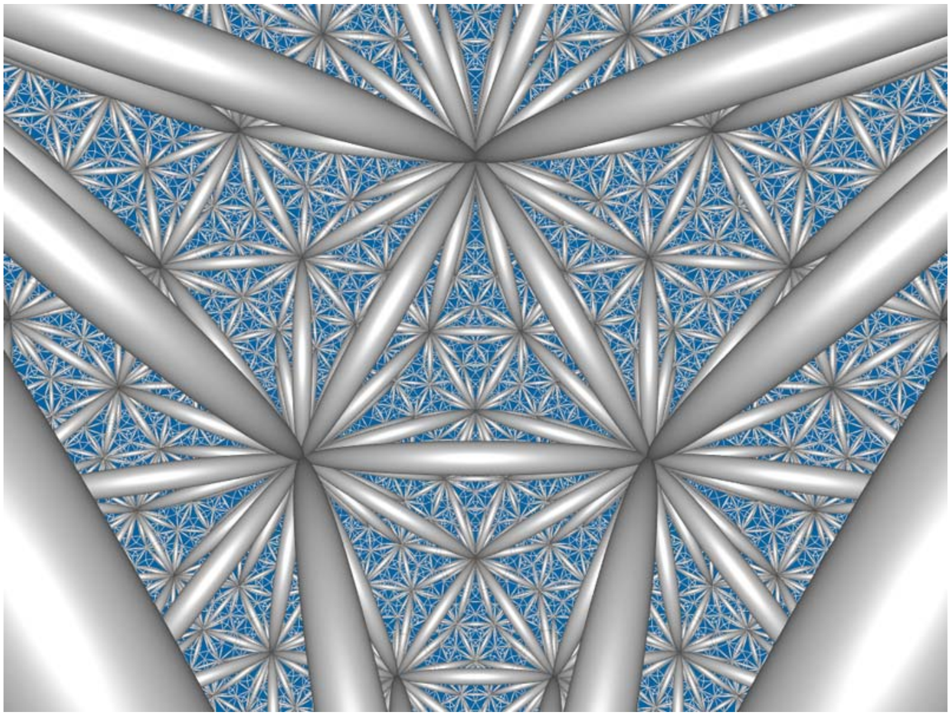
....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 5:49 AM
To: John Baez <baez@math.ucr.edu>

the comment i want to make here is related to some pictures you included in the email that i'm responding to, so i'm going to make a half-hearted attempt to use gmail's quoting mechanisms to re-include the pictures here, but everything that google does is so fucked up that i don't expect it to really work .... anyway here goes .... :

[start of excerpt from your email]

Yes, exactly - here's a picture of [3,4,4]:



It's a bit busy, but apparently some of the octahedra's vertices are points at infinity, sort of like a 3d version of this general sort of thing:

[end of excerpt]

in the 2d picture, though, _all_ of the vertexes are points at infinity, which seems "right". (on the other hand if you then barycentrically subdivide to show the "coxeter simplexes" then only the vertex barycenters lie on the boundary while the edge barycenters and face barycenters lie in the interior.)

so for a 3d picture of [3,4,4] i think i'd expect _all_ the octahedron vertexes to lie on the boundary at infinity, whereas you suggest maybe only some of them should, and the 3d picture itself suggests (as far as i can tell from staring at it) that _none_ of them should lie at infinity.

so there's apparently some confusion here that i haven't managed to resolve yet ....

hmm, in addition to that, maybe there's another typo or thinko or something further below? you wrote:

[start of excerpt]

PSL(2,gaussian integers) is a subgroup of index 4 in this Coxeter group, just as PSL(2,R) is an index-4 subgroup of O(2,1). There's a commutative square of groups here.

```
PSL(2,G) -----------> Coxeter group [3,4,4]
   |                      |
   V                      V
PSL(2,R)  ---------> O(2,1)
```

[end of excerpt]

my first naive guess is .... no wait, i don't even have a guess yet as to how to straighten this out. maybe you should replace the real numbers "r" here by the complex numbers "c", and o(2,1) by o(3,1), or something? but then i also want to fit o(2,1) and psl(2,r) and psl(2,z) and pso(2,1) and pso(1,2) and so forth into the diagram somewhere. it does get somewhat busy i guess.

actually though, i seriously am wondering a bit about what happens when you try to fit pgl(2,D) into the hyperbolic minkowski tilings instead of just psl(2,D), for D some quadratic number ring (including the gaussian and eisenstein cases where the tilings are "uniform hyperbolic honeycombs" but also the other cases where i'm hoping they're only slightly non"uniform").

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 5:59 AM
To: John Baez <baez@math.ucr.edu>

i wrote:

"but then i also want to fit o(2,1) and psl(2,r) and psl(2,z) and pso(2,1) and pso(1,2) and so forth into the diagram somewhere."

i should have included psu(1,1;D), of course.

i deny that there's a hidden smiley in there, by the way.

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Feb 14, 2022 at 8:44 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> so for a 3d picture of [3,4,4] i think i'd expect _all_ the octahedron vertexes to lie on the boundary at infinity, whereas you suggest maybe only some of them should, and the 3d picture itself suggests (as far as i can tell from staring at it) that _none_ of them should lie at infinity.

Yes, I was and am confused about this. Wikipedia says that *all* of them lie at the boundary of infinity, but the picture doesn't look like that, so I feebly said that *some* of them. So maybe we should trust you and Wikipedia and not the picture.

And yes, this was wrong:

> PSL(2,gaussian integers) is a subgroup of index 4 in this Coxeter group, just as PSL(2,R) is an index-4 subgroup of O(2,1). There's a commutative square of groups here.
>
> ```
> PSL(2,G) -----------> Coxeter group [3,4,4]
>    |                      |
>    V                      V
> ```

> PSL(2,R) ---------> O(2,1)

but it's easy to fix:

PSL(2,gaussian integers) is a subgroup of index 4 in this Coxeter group, just as PSL(2,C) is an index-4 subgroup of O(3,1). There's a commutative square of groups here.

```
PSL(2,G) -----------> Coxeter group [3,4,4]
   |                      |
   V                      V
PSL(2,C)  ---------> O(3,1)
```

By the way, I know all the things I'm telling you are somewhat digressive. You wanted to think about the Neron-Severi group of a highly symmetric abelian surface as a lattice in Minkowski spacetime, and get to know the lattice points in the future cone. I'm telling you about discrete subgroups of Lorentz groups associated to the Gaussian and Eisenstein integers, and how they act on honeycombs in hyperbolic space, and also how to get points on the lightcone by squaring spinors. The reason is that I just happen to know about this stuff and I feel it's related. But I haven't linked it up to your questions as clearly as I should!

Best,
jb

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Mon, Feb 14, 2022 at 12:15 PM

you wrote:

"By the way, I know all the things I'm telling you are somewhat digressive. You wanted to think about the Neron-Severi group of a highly symmetric abelian surface as a lattice in Minkowski spacetime, and get to know the lattice points in the future cone. I'm telling you about discrete subgroups of Lorentz groups associated to the Gaussian and Eisenstein integers, and how they act on honeycombs in hyperbolic space, and also how to get points on the lightcone by squaring spinors. The reason is that I just happen to know about this stuff and I feel it's related. But I haven't linked it up to your questions as clearly as I should!"

it doesn't feel excessively digressive to me (more just digressive in a good way), presumably because i'm pretty optimistic (maybe about as much as you are) that these topics are related in a nicely synergistic way.

i certainly do want to get to the highbrow-ish interpretation of these pictures as telling us about holomorphic hermitian line bundles over abelian surfaces (and also over genus 2 curves); it's mostly my job to take us in that direction but if i'm being slow in getting us there then it's probably mostly because it involves a certain amount of work (i consider writing emails to be work, to a great extent) rather than because you're nudging us in the direction of some other nearby sights.

roughly speaking i'm thinking of the neron-severi lattice of an abelian variety as a sort of decategorified picture of a 2-rig of holomorphic vector bundles (well, that's an extremely rough description; i'll have to try to explain it better later but you probably already know very roughly what i mean). and i mean "picture" somewhat literally here, and the pictures that you're showing us here are getting very close to the pictures that i'm trying to see. i'm trying to see lattices in 4d minkowski space and the pictures you're showing us are getting very close to that, some sort of hyperbolic-ish projections of lattice-like structures in that minkowski space; that's not that bad considering how trying to see 4d objects with 3d eyes typically introduces some distortion or degradation of the image.

for at least several weeks (or perhaps several years depending how you calibrate it) i had the vague idea that the ample line bundles would form a sort of "cone" in the neron-severi lattice and that i wanted to see and understand what that cone looks like; then it came as a pleasant shock just this past week or so when i realized that in the abelian surface case that cone really is such a famous and photogenic cone as the future cone of minkowski space. or at least i hope that it is; i hope that i'm not making too many horrific conceptual mistakes here.

anyway, i really think that staring at the lattice points in the pictures and thinking about the clouds of divisors (and/or theta functions and so forth) associated with the corresponding line bundles is helpful in trying to understand all this stuff ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 12:50 PM
To: John Baez <baez@math.ucr.edu>

you wrote:

"Yes, I was and am confused about this. Wikipedia says that *all* of them lie at the boundary of infinity, but the picture doesn't look like that, so I feebly said that *some* of them."

hmm, now i'm wondering whether those vertexes that look like they're hanging in mid-air are really somehow at the boundary ....

i'm still having trouble seeing it that way though ....

i'm also trying to figure out why all the edges look straight. i thought it might be some sort of "3d beltrami-klein projection" but i'm not getting any confirmation of that.

oh well, still somewhat confused by these pictures ....

....
On Mon, Feb 14, 2022 at 11:45 AM John Baez <john.baez@ucr.edu> wrote:
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 1:10 PM
To: John Baez <baez@math.ucr.edu>

i wrote:

"it doesn't feel excessively digressive to me (more just digressive in a good way), presumably because i'm pretty optimistic (maybe about as much as you are) that these topics are related in a nicely synergistic way."

there's also still more to say about potentially entertaining speculative ideas about how the physics pedigree of minkowski space might not be out of place here ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Feb 14, 2022 at 1:40 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 wrote:

> "By the way, I know all the things I'm telling you are somewhat digressive. You wanted to think about the Neron-Severi group of a highly symmetric abelian surface as a lattice in Minkowski spacetime, and get to know the lattice points in the future cone.   I'm telling you about discrete subgroups of Lorentz groups associated to the Gaussian and Eisenstein integers, and how they act on honeycombs in hyperbolic space, and also how to get points on the lightcone by squaring spinors.   The reason is that

> I just happen to know about this stuff and I feel it's related. But I haven't linked it up to your questions as clearly as I should!"

Here's one simple obvious thing I forgot to say about "how to get the honeycomb in hyperbolic space from the lattice in Minkowski spacetime".

Take any lattice point in the future cone. This maps to a point in the hyperboloid, namely by drawing a straight line to the origin and seeing here it hits the hyperboloid.

Then, act on your lattice point by the whole group PSL(2,G) or PSL(2,E) or whatever - the relevant subgroup of the isometry group of your lattice. You get a bunch of lattice points, and thus a bunch of points in the hyperboloid. These are the vertices of a honeycomb!

So, the vertices of your honeycomb aren't pictures of *all* the lattice points in the future cone - but they are pictures of those in one orbit.

But now I'm wondering: what does the isometry group of your lattice have to do with Neron-Severi theory? Why is there this group acting on the 2-group of holomorphic line bundles, preserving the sub-category of ample ones? Does this group have a name?

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 14, 2022 at 7:06 PM
To: John Baez <baez@math.ucr.edu>

let me try to state a very strong conjecture here, strong enough to probably be guaranteed to be false, but nevertheless indicative of the kind of thing i'd like to be true. (and if nothing at all like it is true, then i'd really like to understand why.)

let x be an abelian surface. then the principal polarizations on x "are" precisely the points of minimum strictly positive proper time in the neron-severi lattice of x.

or something like that ....

but first maybe we should try specializing this conjecture to the case where x is the square of the "square" elliptic curve, and try to understand how this specialization relates to the pictures of the square tiling honeycomb ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Feb 14, 2022 at 11:12 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> let x be an abelian surface. then the principal polarizations on x "are" precisely the points of minimum strictly positive proper time in the neron-severi lattice of x.

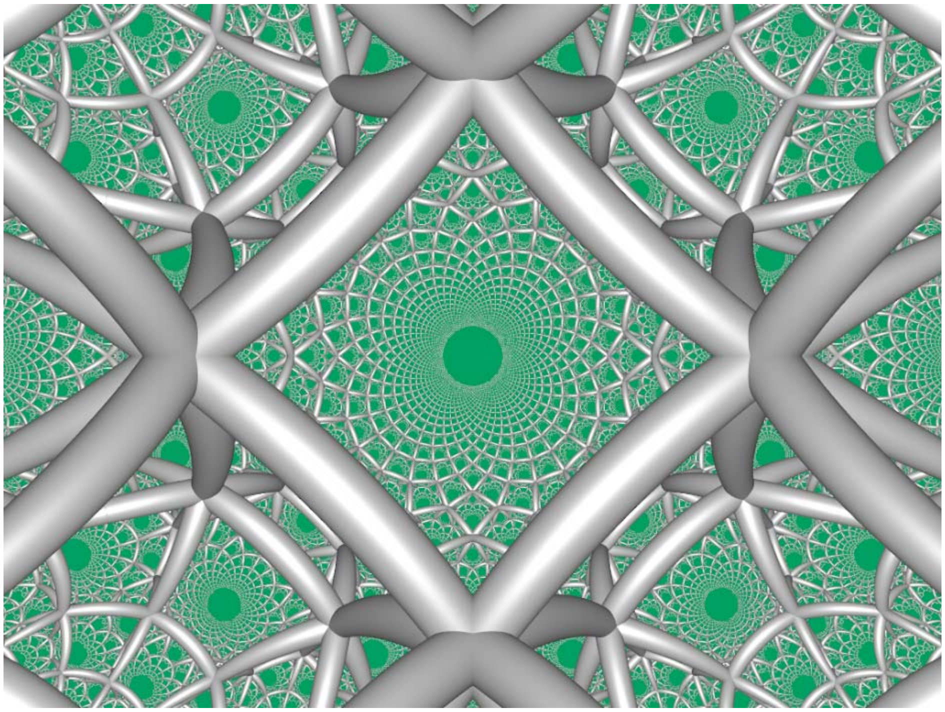
That's a nice conjecture. By cheating and looking around a bit I think I've found something with a similar flavor.

I believe that the principal polarizations are the polarizations D with D.D = 2. Here I'm thinking of polarizations as equivalence classes of divisors with a certain property, and . is what people use for the "intersection number", which I think is our "Minkowski metric" (right?).

So I don't know if the principal polarizations have minimal strictly positive proper time, but they seem to have proper time squared equal to 2.

If this is true, here's a nice spinoff. It follows that the discrete subgroup of the Lorentz group acting on the Neron-Severi lattice maps principal polarizations to principal polarizations!

So, in the case where that discrete subgroup is PSL(2,G), if one vertex here is a principal polarization, all the rest are too!



By the way, I don't know why the intersection pairing on the Neron-Severi lattice of an abelian surface has signature +++- So far I'm just believing you on this! If it has signature ++++, all the stuff about hyperbolic space is irrelevant.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 4:41 AM
To: John Baez <baez@math.ucr.edu>

this looks great, and i heartily approve of your cheating as much as you feel like and in whatever ways you feel like (partially on the grounds that as a team we often work best together by splitting up to cover more ground, or at least to cover the same ground in different ways), but once again i had to quickly avert my eyes as soon as i got a quick glance at the direction/s you seem to be going in .... i'll un-avert my eyes very soon (before the next time we talk presumably), again, but first i need to think it over a bit more ....

also maybe it's worth mentioning outloud that the tension here between lowbrow and highbrow directions to explore is supposed to be a good thing .... part of the fun being when highbrow insights give you an excuse to explore lowbrow directions .... i think of this as being a sort of "shoulders of giants" phenomenon, or perhaps brows of giants ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 5:31 AM
To: John Baez <baez@math.ucr.edu>

an ultra-vague comment:

if we're getting (?? not necessarily "uniform" in the case of a general quadratic number ring D ??) minkowski-hyperboloid tilings for sl(2,D) with non-compact "tiles" whose boundaries look suspiciously like "D itself" (in some curvilinearly distorted way) then this might be associated in a reasonable way with the occurrence in sl(2,D) of the "shear" matrixes with 1's on the main diagonal and an arbitrary element in one of the other corners; and the general pattern of how this works might be intuitively visualizable already in the context of the 2+1 minkowski geometry of sl(2,Z) ....

maybe that was slightly less vague but also slightly more obvious than i was expecting ....

(i'm continuing to be sloppy here about the extent to which we're actually dealing with sl vs psl vs pgl and so forth ....)

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 5:40 AM
To: John Baez <baez@math.ucr.edu>

by the way i accidentally un-averted my eyes from this:

"By the way, I don't know why the intersection pairing on the Neron-Severi lattice of an abelian surface has signature +++- So far I'm just believing you on this! If it has signature ++++, all the stuff about hyperbolic space is irrelevant."

i'm not too sure about _any_ of this, but i'm beginning to be pretty sure that "it" has signature +++-. i don't think i was really conscious of the idea that the "it" in question is "the intersection pairing on" the neron-severi lattice until you just told me that, but hopefully that will eventually become retrospectively obvious ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 8:17 AM
To: John Baez <baez@math.ucr.edu>

here are some more wild guesses and/or questions based on such guesses ....

the dimension sequence of the graded algebra of theta functions for an elliptic curve x is 1,1,2,3,4,5,... . there are interesting stories about the "stutter" at the beginning, but i'm not going to worry about that right now; instead i'm just going to use it to calculate the dimension table for part of the algebra of theta functions over the abelian surface x^2. thus the algebra of theta functions over x^2 is graded by the neron-severi lattice of x^2 (or something like that; i'm mostly ignoring the fact that the neron-severi lattice accounts for only the discrete aspect and not the continuous aspect of the classification of line bundles, because i doubt that the continuous aspect is important here), and inside of that there's a sub-lattice with minkowski signature 1+1 generated by the "fundamental" line bundles for the two separate factors in the cartesian product x^2; and the dimension table for the theta functions living in that sub-lattice should just be the "product table" of the row 1,1,2,3,4,5,... ; thus (hoping that the typographical formatting of the table doesn't get too screwed up here) the table looks something like:

```
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

.
.
.

then by the action of sl(2,z) on the neron-severi lattice, preserving the minkowski structure, we can propagate the data in the table around to inside the whole future cone of the lattice, and inside that cone it seems a bit like the minkowski quadratic form though maybe with some glitches, for example near the boundary of the cone.

and my question is something like this: can we and/or should we apply some sort of "riemann-roch"-like correction to the data in this table of dimensions of spaces of theta functions so as to smooth out the glitches, not only inside the future cone but throughout the whole minkowski space?

and of course i'm hoping that in some way we can get roughly the same basic ideas and pictures and dimension-numerology to apply somehow to the case of more general abelian surfaces rather than just to those with anomalous extra symmetry.

i'm hoping that what i just wrote here comes close to making some sense ....

....
On Tue, Feb 15, 2022 at 8:40 AM JAMES DOLAN
<james.dolan1@students.mq.edu.au> wrote:
>
> by the way i accidentally un-averted my eyes from this:
>
> "By the way, I don't know why the intersection pairing on the Neron-Severi lattice of an abelian surface has signature +++-   So far I'm just believing you on this! If it has signature ++++, all the stuff about hyperbolic space is irrelevant."
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> i'm not too sure about _any_ of this, but i'm beginning to be pretty sure that "it" has signature +++-. i don't think i was really conscious of the idea that the "it" in question is "the intersection pairing on" the neron-severi lattice until you just told me that, but hopefully that will eventually become retrospectively obvious ....
>
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>>
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>>
>> if we're getting (?? not necessarily "uniform" in the case of a general quadratic number ring D ??) minkowski-hyperboloid tilings for sl(2,D) with non-compact "tiles" whose boundaries look suspiciously like "D itself" (in some curvilinearly distorted way) then this might be associated in a reasonable way with the occurrence in sl(2,D) of the "shear" matrixes with 1's on the main diagonal and an arbitrary element in one of the other corners; and the general pattern of how this works might be intuitively visualizable already in the context of the 2+1 minkowski geometry of sl(2,Z) ....
>>
>> maybe that was slightly less vague but also slightly more obvious than i was expecting ....
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>> (i'm continuing to be sloppy here about the extent to which we're actually dealing with sl vs psl vs pgl and so forth ....)
>>
>>
>> ....
>> On Tue, Feb 15, 2022 at 7:41 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:
>>>

>>> this looks great, and i heartily approve of your cheating as much as you feel like and in whatever ways you feel like (partially on the grounds that as a team we often work best together by splitting up to cover more ground, or at least to cover the same ground in different ways), but once again i had to quickly avert my eyes as soon as i got a quick glance at the direction/s you seem to be going in .... i'll un-avert my eyes very soon (before the next time we talk presumably), again, but first i need to think it over a bit more ....
>>>
>>> also maybe it's worth mentioning outloud that the tension here between lowbrow and highbrow directions to explore is supposed to be a good thing .... part of the fun being when highbrow insights give you an excuse to explore lowbrow directions .... i think of this as being a sort of "shoulders of giants" phenomenon, or perhaps brows of giants ....
>>>
>>>
>>> ....
>>> On Tue, Feb 15, 2022 at 2:12 AM John Baez <john.baez@ucr.edu> wrote:
>>>>
>>>> Hi -
>>>>
>>>>> let x be an abelian surface. then the principal polarizations on x "are" precisely the points of minimum strictly positive proper time in the neron-severi lattice of x.
>>>>
>>>>
>>>> That's a nice conjecture.   By cheating and looking around a bit I think I've found something with a similar flavor.
>>>>
>>>> I believe that the principal polarizations are the polarizations D with D.D = 2.   Here I'm thinking of polarizations as equivalence classes of divisors with a certain property, and . is what people use for the "intersection number", which I think is our "Minkowski metric" (right?).
>>>>
>>>> So I don't know if the principal polarizations have minimal strictly positive proper time, but they seem to have proper time squared equal to 2.
>>>>
>>>> If this is true, here's a nice spinoff.   It follows that the discrete subgroup of the Lorentz group acting on the Neron-Severi lattice maps principal polarizations to principal polarizations!
>>>>
>>>> So, in the case where that discrete subgroup is PSL(2,G), if one vertex here is a principal polarization, all the rest are too!
>>>>
>>>>

[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 8:28 AM
To: John Baez <baez@math.ucr.edu>

[i think maybe gmail decided to do something annoying with the line breaks in the email i just sent you (though it's hard to tell for sure because i can't see what it looks like at your end) so i'm going to try re-sending it in a way that might change the line breaks.]

here are some more wild guesses and/or questions based on such guesses ....

the dimension sequence of the graded algebra of theta functions for an elliptic curve x is 1,1,2,3,4,5,... . there are interesting stories about the "stutter" at the beginning, but i'm not going to worry about that right now; instead i'm just going to use it to calculate the dimension table for part of the algebra of theta functions over the abelian surface x^2. thus the algebra of theta functions over x^2 is graded by the neron-severi lattice of x^2 (or something like that; i'm mostly ignoring the fact that the neron-severi lattice accounts for only the discrete aspect and not the continuous aspect of the classification of line bundles, because i doubt that the continuous aspect is important here), and inside of that there's a sub-lattice with minkowski signature 1+1 generated by the "fundamental" line

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should just be the "product table" of the row 1,1,2,3,4,5,... with
itself; thus (hoping that the typographical formatting of the table
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 8:41 AM
To: John Baez <baez@math.ucr.edu>

[gmail is still doing annoying glitchy things due to its lack of transparency; this is another attempt to get it to behave, though i'm not completely sure you received the first attempt, and i can't tell what kind of improvement and/or deterioration you're actually seeing at your end.]
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Feb 15, 2022 at 10:15 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> by the way i accidentally un-averted my eyes from this:

It's okay if occasionally you accidentally read my emails.

> "By the way, I don't know why the intersection pairing on the Neron-Severi lattice of an abelian surface has signature +++-   So far I'm just believing you on this! If it has signature ++++, all the stuff about hyperbolic space is irrelevant."
>
> i'm not too sure about _any_ of this, but i'm beginning to be pretty sure that "it" has signature +++-. i don't think i was really conscious of the idea that the "it" in question is "the intersection pairing on" the neron-severi lattice until you just told me that, but hopefully that will eventually become retrospectively obvious ....

Do you have some other way of thinking about this "it"?   If you didn't know the intersection pairing way, maybe you knew some other way.   This intersection pairing business is a very divisor-centered way of thinking about it, and I'd really enjoy having a line-bundle-centered way.

Best,
jb
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Feb 15, 2022 at 10:29 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> here are some more wild guesses and/or questions based on such guesses ....
>
> the dimension sequence of the graded algebra of theta functions for an
> elliptic curve x is 1,1,2,3,4,5,... .

Thanks. This is a great way to to exploit our (rather feeble, in my case) knowledge
of elliptic curves to get some (even more feeble) understanding of abelian surfaces,
at least those that are products of elliptic curves.

So you seem to be saying that for an abelian surface, the dimension of the
space of sections of a line bundle is *almost* a quadratic form on the Neron-Severi

lattice, with some glitchiness, but little enough that you can see what the quadratic form is.

And just to come out and say it, the 2 in "quadratic" comes from the 2-dimensionality of the abelian surface.

So yeah, if *this* quadratic form were of signature ++++ there wouldn't be lots of line bundles with *no* sections, so it has to have some other signature.

And yeah, I think I can use my (still feeble) understanding of the Riemann-Roch theorem for surfaces to understand the glitchiness if I work at it. This theorem is one of the main ingredients for proving that wonderful fact I emailed you last night - the one you're acting like you didn't read. I always found Riemann-Roch rather cryptic, and still kinda do, but I've been seeing lately how it's a real workhorse in algebraic geometry.

> [i think maybe gmail decided to do something annoying with the line breaks in the email i just sent you (though it's hard to tell for sure because i can't see what it looks like at your end) so i'm going to try re-sending it in a way that might change the line breaks.]

Actually it looked fine the first time.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 12:52 PM
To: John Baez <baez@math.ucr.edu>

so let's see, i seem to be suggesting that the minkowski quadratic form on the neron-severi lattice of an abelian surface is equal modulo riemann-roch to the number of theta functions, and you seem to be suggesting that it's the "self-intersection pairing" on divisors. so are we suggesting that the self-intersection pairing is equal modulo riemann-roch to the number of theta functions, or something like that? not sure i recognize such a circle of ideas yet ....

you wrote:

"And just to come out and say it, the 2 in "quadratic" comes from the 2-dimensionality of the abelian surface."

yes. certain further patterns and ideas emerge when we start thinking about abelian d-folds for higher d; the physics inspirations start looking more like quantum mechanics than like special relativity. by the way is that also why q looks like 2 in palmer handwriting?

you wrote:

"So yeah, if *this* quadratic form were of signature ++++ there wouldn't be lots of line bundles with *no* sections, so it has to have some other signature."

yes, that's what i think.

you wrote:

"And yeah, I think I can use my (still feeble) understanding of the Riemann-Roch theorem for surfaces to understand the glitchiness if I work at it. This theorem is one of the main ingredients for proving that wonderful fact I emailed you last night - the one you're acting like you didn't read. I always found Riemann-Roch rather cryptic, and still kinda do, but I've been seeing lately how it's a real workhorse in algebraic geometry."

well, that all sounds very promising, and all of this is giving me more reason to actually finish reading those emails that i've been semi-averting my eyes from.

maybe riemann-roch just inherits its crypticness from homological algebra? not sure ....

you wrote:

"Actually it looked fine the first time."

ok. i have to deal with a lot of glitchiness from the software and hardware that i interact with ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 12:55 PM
To: John Baez <baez@math.ucr.edu>

"Do you have some other way of thinking about this "it"? If you didn't know the intersection pairing way, maybe you knew some other way. This intersection pairing business is a very divisor-centered way of thinking about it, and I'd really enjoy having a line-bundle-centered way."

yes, i have an "other" way, except of course i can't guarantee that it really is other when you examine it closely. i'll definitely try to say more about this other way relatively soon. if we had a recording of our discussion (i guess this is good propaganda for why i should try to get my internet connection working better to the point where we can make useful recordings of the discussions) then i think we could just go back to the point where i started talking about the minkowski structure on the neron-severi lattice of an abelian surface and see what i said, but in principle i think i can more or less reconstruct what i said. i think it had a lot to do with my ad-hoc interpretation of [the hodge decomposition in row 2 of the hodge diamond of the abelian surface] as a sort of "branching rule" wrt the composite inclusion gl(2,c) >-> gl(4,r), and then recognizing the resulting 4d middle-column representation of sl(2,c) as the tautological representation of so(3,1) in disguise, or something like that. so very "kleinian" rather than using any explicit syntax ....

(for a more line-bundle-centered way we may have to work out more of the details of that idea about interpreting the minkowski quadratic form on the neron-severi lattice as the number of corresponding theta functions, modulo riemann-roch subtleties.)

this might be beginning to fall into the backlog of ideas that are a bit too talky and/or hand-wavy for me to say much about via email before we get a chance to talk again, though.

another item that might be in that backlog is the issue of the "alignment" constraint between a lattice and a minkowski structure, that automatically holds for the neron-severi lattice of an abelian surface. i tried to start explaining this in those emails labeled "part 2" but i don't think i've managed to explain it very well so far. i'm fairly sure that the final answer will be very simple but i'm still at some intermediate stage between confusion and clarity on this particular topic.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 15, 2022 at 1:02 PM
To: John Baez <baez@math.ucr.edu>

sorry, i just want to fix a sort-of typo ....

i wrote:

"i think it had a lot to do with my ad-hoc interpretation of [the hodge decomposition in row 2 of the hodge diamond of the abelian surface] as a sort of "branching rule" wrt the composite inclusion gl(2,c) >-> gl(4,r), and then recognizing the resulting 4d middle-column representation of sl(2,c) as the tautological representation of so(3,1) in disguise, or something like that."

i wrote "composite inclusion" there because i was thinking about the composite inclusion sl(2,c) >-> gl(2,c) >-> gl(4,r), but then i edited the text to remove explicit mention of that composite, and forgot to edit out "composite".

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Wed, Feb 16, 2022 at 7:06 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

I did some talking to people and it turns out tons of stuff I/we were guessing is right. I won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion".

> i think it had a lot to do with my ad-hoc interpretation of [the hodge decomposition in row 2 of the hodge diamond of the abelian surface] as a sort of "branching rule" wrt the composite inclusion gl(2,c) >-> gl(4,r), and then recognizing the resulting 4d middle-column representation of sl(2,c) as the tautological representation of so(3,1) in disguise, or something like that. so very "kleinian" rather than using any explicit syntax ....

That makes some sense, but I don't know why we think why groups like SO(3,1) or PSL(2,C) or discrete subgroups of these
like PSL(2,G) or PSL(2,E) should act as symmetries here. For example, in our hoped-for Gaussian example of an abelian
surface, I suspect that PSL(2,G) acts on all of these:

* the Neron-Severi lattice - or better, the 2-group of holomorphic line bundles
* the positive cone in this lattice - so, the commutative monoidal groupoid of ample line bundles
* the 'minimal' guys in the positive cone - which I'm thinking are the principal polarizations

But I don't really know *why* it acts on these. I don't think it acts on the underlying abelian surface - that'd be great, but it doesn't sound right.

Do you know what's going on here?

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 16, 2022 at 11:32 AM
To: John Baez <baez@math.ucr.edu>

"I did some talking to people and it turns out tons of stuff I/we were guessing is right."

ok, that sounds great.

"I won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion"."

ok, thanks. (i guess i'm wondering now to what extent those riemann-roch/ish ideas are in the picture here.)

you know, i noticed something a bit peculiar here. when we started talking about this stuff, we advertised the main topic to ourselves as "polarizations on abelian varieties", more or less; and recently it seems like we really have been learning a whole lot about polarizations on abelian varieties. i'm not sure i expected the discussion to stay on topic that much.

"But I don't really know *why* it acts on these. I don't think it acts on the underlying abelian surface - that'd be great, but it doesn't sound right.

Do you know what's going on here?"

i think i know some of what's going on here, and i have some inklings about more of it. i think GL(2,G) really _does_ act on that abelian surface, for incredibly simple reasons: the tautological action of GL(2,G) is of course on a rank 2 free module M over G, and as a 2d complex torus, that abelian surface is just (M#C)/M. (here "#" is tensoring over G, and C is the complex numbers).

part of what's going on here is that the _inverse_ of "taking spinors" is actually slightly more functorial (by a factor of 2) than the direct process is, in this context at least. the period lattice lives in the spinors, while the neron-severi lattice lives in the minkowski spacetime.

things are very simple in this supremely symmetric case, but it should be very interesting to see how it all works out in slightly less symmetric cases like the abelian surface (M#C)/M where M is rank 2 free over some quadratic number ring other than G or E, as well as in maximally non-symmetric cases. i have inklings about these cases .... they should work out quite nicely, but i'm not sure i understand them well enough to explain it that well yet. i really want to see pictures of those slightly less symmetric and/or maximally non-symmetric cases!

i have some more quite lowbrow stuff (plus some less lowbrow stuff) to tell you about the coxeter tilings, that i'm hoping will help a lot with understanding all this stuff ....

you know, a pretty long time ago i learned (probably from reading joan birman's stuff) that real toruses have more automorphisms than you might guess at first because they inherit them from free modules over the integers. similarly, complex toruses with complex multiplication by a number ring D inherit automorphisms from free D-modules.

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Wed, Feb 16, 2022 at 12:15 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 won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion"."
>
> ok, thanks. (i guess i'm wondering now to what extent those riemann-roch/ish ideas are in the picture here.)

Yes, people seem to use Riemann-Roch a lot while proving all these fancier things. I haven't really followed these proofs, but I've looked at them.

> you know, i noticed something a bit peculiar here. when we started talking about this stuff, we advertised the main topic to ourselves as "polarizations on abelian varieties", more or less; and recently it seems like we really have been learning a whole lot about polarizations on abelian varieties. i'm not sure i expected the discussion to stay on topic that much.

I have this tendency these days, maybe more than when I was young, to want to burrow into concrete examples. I guess one reason is that I feel it's hopeless to master "algebraic geometry" in all its vast glory, but I could become really good at "highly symmetrical abelian surfaces".

> "But I don't really know *why* it acts on these. I don't think it acts on the underlying abelian surface - that'd be great, but it doesn't sound right.
>
> Do you know what's going on here?"
>
> i think i know some of what's going on here, and i have some inklings about more of it. i think GL(2,G) really _does_ act on that abelian surface, for incredibly simple reasons: the tautological action of GL(2,G) is of course on a rank 2 free module M over G, and as a 2d complex torus, that abelian surface is just (M#C)/M. (here "#" is tensoring over G, and C is the complex numbers).

Ugh! That's obviously true! My problem was that for some stupid reason I was imagining these surfaces as equipped with Kaehler structures, which rigidifies them a lot more. I guess I was also influenced by how elliptic curves have a lot fewer automorphisms than their underlying real tori. But again this is sort of stupid.

Okay, good, things are clearer now.

> part of what's going on here is that the _inverse_ of "taking spinors" is actually slightly more functorial (by a factor of 2) than the direct process is, in this context at least. the period lattice lives in the spinors, while the neron-severi lattice lives in the minkowski spacetime.

I think this part is not what was confusing me. It should work out okay.

> things are very simple in this supremely symmetric case, but it should be very interesting to see how it all works out in slightly less symmetric cases like the abelian surface (M#C)/M where M is rank 2 free over some quadratic number ring other than G or E, as well as in maximally non-symmetric cases.

I'm a bit scared of these because I've never thought hard about elliptic curves with complex multiplication, which seem like a good warmup. But I don't mind getting into this stuff.

> you know, a pretty long time ago i learned (probably from reading joan birman's stuff) that real toruses have more automorphisms than you might guess at first because they inherit them from free modules over the integers. similarly, complex toruses with complex multiplication by a number ring D inherit automorphisms from free D-modules.

Neat. That thought about "real toruses have more automorphisms that you'd expect at first" was somehow actually *inhibiting me* from thinking about "complex toruses have more automorphisms that I'd expect" because those complex toruses have underlying real 4-toruses, which have more automorphisms than we want! This doesn't make much sense, but I'm trying to analyze my stupidity here....

Best,
jb
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 16, 2022 at 2:09 PM
To: John Baez <baez@math.ucr.edu>

> "My problem was that for some stupid reason I was imagining these surfaces as equipped with Kaehler structures, which rigidifies them a lot more."

by the way, notice that those flat integral kaehler structures (aka polarizations, more or less) are essentially lattice points in the future minkowski cone, so the extra rigidness you get from them is essentially the rigidness of a minkowski observer; that is, breaking the possible symmetry down from the lorentz group to the euclidean group. and "rigid" is a fairly apt description here because the compactness of the euclidean group causes closed discrete subgroups to be finite. that's why genus 2 curves having the abelian surface as their jacobian can only have finite symmetry groups even though the unpolarized abelian surface can have infinite symmetry group; picking such a genus 2 curve is more or less equivalent (i think ... modulo some minor degenerate cases and/or terminology glitches) to picking a principal polarization.

all this is also connected with why i keep insisting: don't get so distracted by the nice shiny highly symmetric abelian surfaces as to lose interest in the duller ones with lesser symmetry. the really highly symmetric ones like the cartesian square of the gaussian elliptic curve are so symmetric that maybe they even have only a single principal polarization (or maybe just a very few) up to the action of the symmetry group; that's "bad" for (some of) my purposes because i want to see an abelian surface with an interesting variety of genuinely _different_ genus 2 curves that all share it as their jacobian. for example consider the bolza curve; i'm hoping that we'll be able to see very concretely what some of its "jacobian-mates" are.

maybe part of what i'm saying is that, depending on my mood, i might be more interested in an abelian surface with a relatively small symmetry group having a relatively large intersection with the euclidean group (thus giving a genus 2 curve with relatively large finite symmetry group, but also having an interesting variety of jacobian-mates), than in one having just a large symmetry group. "don't squander all your symmetry in the lorentzian department; save some of it for the euclidean department" or something like that ....

of course at other times i'm very interested in the highly symmetric abelian surfaces too; there's a lot more games we should play with those .... hyperbolic tilings and so forth ....

"Yes, people seem to use Riemann-Roch a lot while proving all these fancier things. I haven't really followed these proofs, but I've looked at them."

i think i'm vaguely hoping for results that require some riemann-roch/ish fudging (i have some vague idea of what i think i mean by that ....) even just to _state_; not only in their proof ....

"I'm a bit scared of these because I've never thought hard about elliptic curves with complex multiplication, which seem like a good warmup. But I don't mind getting into this stuff."

complex multiplication on abelian varieties (aka "the jugendtraum" or perhaps "generalized jugendtraum", including hilbert's 12th problem and "complex multiplication on hodge structures" instead of just on abelian varieties) is really interesting!! i'm no expert but my vague impression is that jugendtraum ideas and langlands-program ideas are two huge fuzzy-outlined thematic programs in mathematics that overlap a lot in their area of application, but overlap somewhat less in their actual thematic content so far. i'm very interested in the possibility of somehow unifying these into a single thematic program (if i can even understand what the langlands program really is ....); for all i know someone has already done that without my being aware of it.

(for example in the context of elliptic curves, the overlap between the taniyama-weil modularity theorem of the langlands program on the one hand and complex multiplication on the other hand is the heegner numbers discovered by gauss .... i think ....)

anyway i'm hoping we get into this stuff at some point ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 16, 2022 at 2:39 PM
To: John Baez <baez@math.ucr.edu>

i'm really trying to get around to finishing reading all of your emails, including especially the parts i've been regarding as "cheating" parts for me! but even if i'm getting past my hesitancy to read them, i still might be slow in understanding some of them ....

i think i'm also ready to start actually trying to read mumford's "tata lectures on theta", now that i feel i've somewhat prepared myself for it by already understanding what i'm imagining mumford is going to say .... maybe mumford actually says much more than that though ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Thu, Feb 17, 2022 at 9:23 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 Wed, Feb 16, 2022 at 2:09 PM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> "My problem was that for some stupid reason I was imagining these surfaces as equipped with Kaehler structures, which rigidifies them a lot more."
>
> by the way, notice that those flat integral kaehler structures (aka polarizations, more or less) are essentially lattice

> points in the future minkowski cone, so the extra rigidness you get from them is essentially the rigidness of a minkowski observer; that is, breaking the possible symmetry down from the lorentz group to the euclidean group. and "rigid" is a fairly apt description here because the compactness of the euclidean group causes closed discrete subgroups to be finite.

Great! For some reason this hadn't percolated into all corners of my brain yet. This is nice because it means the stabilizer of a lattice point in the future cone acts on the corresponding abelian-surface-with-Kaehler-structure. I want to figure out what this stabilizer can be, explicitly, for the PSL(2,G) and PSL(2,E) examples. It should be doable.

Btw, did you notice how similar this is to the usual story about the weight lattice of a complex simple Lie group?

Each point in the weight lattice puts a line bundle on the complete flag variety of our group. This bundle only has nonzero sections if the point lies in the Weyl chamber - that's our cone here. Each point also gives the complete flag variety a "possibly degenerate Kaehler structure", which is an honest Kaehler structure when the point is in the interior of the Weyl chamber.

It seems the analogues of the principal polarizations are the simple roots... or maybe I mean "simple weights"??? There's one of them for each dot in the Dynkin diagram, and they usually give *degenerate* Kaehler structures because they're on the boundary of the Weyl chamber.

This makes it even more interesting that in the abelian surface case, the principal polarizations have B(L,L) = 2 where B is the bilinear form on the Neron-Severi lattice. For a simply laced Lie algebra the roots have B(L,L) = 2.

There's also some analogy between the hyperbolic Coxeter groups we're seeing when we study very symmetrical abelian surfaces, and the ordinary Coxeter groups we're seeing when we study complete flag varieties of simple Lie groups.

There are a bunch of disanalogies, too, but you can probably see those.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 20, 2022 at 12:58 PM
To: John Baez <baez@math.ucr.edu>

you wrote:

"Btw, did you notice how similar this is to the usual story about the weight lattice of a complex simple Lie group?"

i've been so distracted by the news that the neron-severi lattice of an abelian surface is 4d only in special cases that i've had little time so far to get back to fascinating ideas such as you're raising with this question .... (for example, with the idea of considering "associated holomorphic vector bundle" processes given as 2-homomorphisms from a 2-rig of complex simple lie group flavor to one of (for example) complex abelian variety flavor ....)

i'm hoping to get back to such ideas relatively soon. one opportunity is if we talk monday afternoon (as i think we have planned) 4:30pm-6:30pm. i'm going to make an attempt then to have my internet connection working well enough that we can use your zoom room to record the chat ....

also i think i've got the vague impression from you and/or todd that you've been chatting about this stuff with people somewhere .... maybe n-cat cafe or somewhere? i'll check around; maybe i should look at it .... or maybe it was mainly just you and todd who were chatting about it ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Sun, Feb 20, 2022 at 3:43 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> "Btw, did you notice how similar this is to the usual story about the weight lattice of a complex simple Lie group?"
>
> i've been so distracted by the news that the neron-severi lattice of an abelian surface is 4d only in special cases that i've had little time so far to get back to fascinating ideas such as you're raising with this question ....

That makes sense, since this question is more interesting when the Neron-Severi lattice of an abelian surface is 4d than when it's 1d.

> i'm hoping to get back to such ideas relatively soon. one opportunity is if we talk monday afternoon (as i think we have planned) 4:30pm-6:30pm. i'm going to make an attempt then to have my internet connection working well enough that we can use your zoom room to record the chat ....

That sounds good. That's 1:30-3:30 pm for me, and let's try to meet here:

https://ucr.zoom.us/j/7727771354

> also i think i've got the vague impression from you and/or todd that you've been chatting about this stuff with people somewhere .... maybe n-cat cafe or somewhere? i'll check around; maybe i should look at it .... or maybe it was mainly just you and todd who were chatting about it ....

I was asking about some stuff on the n-Cafe, basically trying to check if I understood it, and that's how I got some useful confirmations and buzzwords and also bumped into this claim that the Neron-Severi lattice of an abelian surface is only 4d for surfaces that are isogenous to a product of two curves.

Taking an approach that's kinda the opposite of yours, I also looked around until I found a book that I think will help me a lot: *Complex Abelian Varieties* by Christina Birkenhake and Herbert Lange. It's big and fat and has sections where they just talk, and it seems to have a whole lot of standard stuff in one place. I was getting sick of seeing little snippets here and there.

I'm wanting to understand the approach that says a polarization of an abelian surface S is an isogeny from S to its "dual" S* (which you can define if you know the concept of dual lattice) obeying a certain property.

Also lots of other stuff, like how you actually figure out the Neron-Severi group!

Best,
jb

Best,
jb
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Feb 20, 2022 at 4:27 PM
To: John Baez <baez@math.ucr.edu>

"... this claim that the Neron-Severi lattice of an abelian surface is only 4d for surfaces that are isogenous to a product of two curves."

wait a minute .... i was already jokingly going to ask whether you were sure you meant "product of two isogenous elliptic curves" (as i thought you said last time, and which sounded weirdly interesting-- yep, it looks like that's what you did say) vs "isogenous to a product of two elliptic curves" which might be more favorable to our cause. because i thought that "having jacobian isogenous to a product of n elliptic curves" was fairly close to a definition of "hyperelliptic genus n curve", and i vaguely thought that (over the complex numbers at least) pretty much all genus 2 curves are hyperelliptic, and pretty much all abelian surfaces are jacobians of curves .... or something like that; don't trust me on any of this, of course!

so there are still weird possibilities here ....

offhand it's not obvious to me exactly how saying a phenomenon is "generic" would work if you're not talking about over some nice mundane field like the complex numbers, though ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu>
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Sun, Feb 20, 2022 at 6:40 PM

On Sun, Feb 20, 2022, 4:27 PM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> "... this claim that the Neron-Severi lattice of an abelian surface is only 4d for surfaces that are isogenous to a product of two curves."
>
> wait a minute .... i was already jokingly going to ask whether you were sure you meant "product of two isogenous elliptic curves" (as i thought you said last time, and which sounded weirdly interesting-- yep, it looks like that's what you did say) vs "isogenous to a product of two elliptic curves" which might be more favorable to our cause.

Whoops - the guy said "product of two isogenous elliptic curves", like I said last time.

I want to get some idea of why/whether this is true, so I'm gonna look at some books.

Best,
jb
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Mon, Feb 21, 2022 at 7:07 AM

you wrote:

"I did some talking to people and it turns out tons of stuff I/we were guessing is right. I won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion"."

i finally got around to starting to look these up, and it's interesting that they both seem to deal (at least originally) with 2d surfaces in particular ....

is it interesting applying them to k3 surfaces, for example?

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au>
To: John Baez <baez@math.ucr.edu>
Mon, Feb 21, 2022 at 7:29 AM

so now i'm trying (somewhat ....) to understand your twf196 stuff about trautman's pythagorean spinors stuff, to try to see if we can use it to construct enough of the right kind of lattice-points in minkowski spacetime from lattice-points in spinor space to make the neron-severi lattice 4d for a general/ish abelian surface. if it doesn't work then it should be interesting to see what goes wrong ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 21, 2022 at 7:59 AM
To: John Baez <baez@math.ucr.edu>

i wrote:

"so now i'm trying (somewhat ....) to understand your twf196 stuff about trautman's pythagorean spinors stuff, to try to see if we can use it to construct enough of the right kind of lattice-points in minkowski spacetime from lattice-points in spinor space to make the neron-severi lattice 4d for a general/ish abelian surface. if it doesn't work then it should be interesting to see what goes wrong ...."

maybe i should mention that back when i was optimistic about neron-severi being 4d for an arbitrary abelian surface, i developed a conjecture that the correct "alignment constraint" between a lattice and a minkowski cone in order for it to be the neron-severi lattice of an abelian surface is something like "the lattice is generated by light-like points". naive dimension-counting suggests that that's 4 degrees of constraint on the 10-dimensional moduli space of lattices-up-to-minkowski-equivalence, leaving a 6-dimensional moduli stack to match the 6-dimensional moduli stack of abelian surfaces.

i think that that constraint would tend to make the lattice points somewhat "dense" in the other inhabited hyperboloids as well, as we probably sort-of see in the pictures of the hyperbolic square tiling honeycomb, for example.

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Feb 21, 2022 at 9:20 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> "I did some talking to people and it turns out tons of stuff I/we were guessing is right. I won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion"."
>
> i finally got around to starting to look these up, and it's interesting that they both seem to deal (at least originally) with 2d surfaces in particular ....
>
> is it interesting applying them to k3 surfaces, for example?

Yes! I don't know a lot about it, but Svetlana Makarova's General introduction to K3 surfaces touches on this stuff, and I think I saw more somewhere else (before I was ready to understand it at all).

Best,
jb

John Baez <john.baez@ucr.edu> Mon, Feb 21, 2022 at 9:24 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 Mon, Feb 21, 2022 at 7:29 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> so now i'm trying (somewhat ....) to understand your twf196 stuff about trautman's pythagorean spinors stuff, to try to see if we can use it to construct enough of the right kind of lattice-points in minkowski spacetime from lattice-points in spinor space to make the neron-severi lattice 4d for a general/ish abelian surface. if it doesn't work then it should be interesting to see what goes wrong ....

I still think this stuff should work pretty well replacing Z by the Gaussians Z[i]; I think it's related to the abelian surface that's the product of two copies of the Gaussian elliptic curve, and then we should get a 4d Neron-Severi lattice.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Feb 21, 2022 at 10:35 AM
To: John Baez <baez@math.ucr.edu>

"I still think this stuff should work pretty well replacing Z by the Gaussians Z[i]; I think it's related to the abelian surface that's the product of two copies of the Gaussian elliptic curve, and then we should get a 4d Neron-Severi lattice."

hmm, maybe that's even propaganda _against_ getting 4d neron-severi for a more generic abelian surface, if it looks like it's relying on the specialness of something like the gaussian integers ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Feb 22, 2022 at 12:28 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

About K3 surfaces:

> "I did some talking to people and it turns out tons of stuff I/we were guessing is right. I won't tell you what it is, but some important buzzwords are "Hodge index theorem" and "Nakai-Moishezon criterion"."
>
> i finally got around to starting to look these up, and it's interesting that they both seem to deal (at least originally) with 2d surfaces in particular ....
>
> is it interesting applying them to k3 surfaces, for example?

Yes! You may recall that a K3 surface has H^{1,1} of rank 20. It turns out the Neron-Severi group of a K3 surface is a lattice whose rank can be anything from 1 to 20. By the Hodge index theorem this lattice comes with a Minkowskian quadratic form. This is "even", meaning that this form applied to any lattice vector is an even integer. Some people know a lot more about what sort of options are possible.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Feb 22, 2022 at 5:52 AM
To: John Baez <baez@math.ucr.edu>

that's interesting ....

so we started with a vague guess that neron-severi is 4d minkowski for an abelian surface, based on the rough idea that there's already a continuous 4d minkowski continuous spacetime of flat kaehler structures in that case and it would be nice if the neron-severi forms occupied a lattice of full rank in there--after all we need a cone of ample line bundles so why not have it be the pre-eminent cone of our universe aka the future cone of 4d minkowski space?

actually it wasn't just that i thought it'd be "nice" for the lattice to be of full rank; i probably had some faulty argument that it was inevitable, even though i should have known that the embedding of a 4d subspace of flat kaehler forms into a 6d space of flat symplectic forms (by taking the imaginary part (aka "underlying symplectic form") of the kaehler form) could miss a 6d lattice of integral symplectic forms pretty badly ....

anyway it turned out that the lattice actually seems to be full rank only in specially photogenic and symmetrical cases, which give us those nice 3d minkowski-hyperboloidal tilings where the boundaries of the non-compact regions seem to be tiled by (hyperbolically distorted) voronoi cells of quadratic integers; in the more generic cases we seem to get only an arguably disappointing 1d lattice, at least according to rumor ....

but then now we're getting a different perspective on the situation by switching the emphasis from the abelian (variety) aspect of abelian surfaces to the (algebraic) surface aspect; now the (no longer necessarily 4d) "minkowski" aspect of the neron-severi lattice reveals itself in more general and more syntactic form as an "intersection-pairing" quadratic form (with signature appropriate to carving out a convex cone of ample line bundles?), instead of relying on the ad-hoc kleinian-geometry argument that told us we had a traditional 4d minkowski spacetime in the abelian surface case. now even those 1d neron-severi lattices of generic abelian and/or other surfaces seem "minkowski"; it's just that "1d minkowski spacetime" is really just "minkowski time", lacking space dimensions entirely ....

(stupid question: is all "this" in some way related to how string-theorists (in the sense of a somewhat old-fashioned physicist slumming in somewhat less old-fashioned algebraic geometry, perhaps) start to develop a taste for higher-dimensional spacetimes?)

anyway, this is making me start to wonder about how to carve out a convex cone of ample line bundles in the neron-severi lattice of an algebraic 3-fold, for example. to quote you from earlier in the thread:

"So you seem to be saying that for an abelian surface, the dimension of the space of sections of a line bundle is *almost* a quadratic form on the Neron-Severi lattice, with some glitchiness, but little enough that you can see what the quadratic form is.

And just to come out and say it, the 2 in "quadratic" comes from the 2-dimensionality of the abelian surface.

So yeah, if *this* quadratic form were of signature ++++ there wouldn't be lots of line bundles with *no* sections, so it has to have some other signature."

thus i'm tempted to paraphrase you as saying that "the 3 in "cubic" comes from the 3-dimensionality of the (abelian or otherwise) 3-fold". i'm not sure exactly what i mean by that yet (for example whether "intersection-tripling" is going to replace "intersection-pairing" in some way ....) but i have some vague suspicion from (thought-)experimenting with the neron-severi cone of an abelian 3-fold with complex multiplication that this is somewhat on the right track; i might try to say more about that later ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Feb 22, 2022 at 9:20 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

Thanks for all that! I ran into this:

> The precise description of which lattices can occur as Picard lattices of K3 surfaces is complicated. One clear statement, due to Viacheslav Nikulin and David Morrison, is that every even lattice of signature $(1, \rho - 1)$ with $\rho \leq 11$ is the Picard lattice of some complex projective K3 surface.

Here rho is just the rank of the lattice, aka the 'Picard number'. Remember, the rank can be anything from 1 to 20, and the lattice is always even. I get the feeling that for low rho "anything can happen", while for higher rho the choices "thin out"... perhaps because we need more highly symmetrical K3 surfaces to get a high rank??? (That wild guess is based on our experience with abelian surfaces.)

Best,
jb

[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, Feb 22, 2022 at 11:43 PM

HI -

> but then now we're getting a different perspective on the situation by switching the emphasis from the abelian (variety) aspect of abelian surfaces to the (algebraic) surface aspect; now the (no longer necessarily 4d) "minkowski" aspect of the neron-severi lattice reveals itself in more general and more syntactic form as an "intersection-pairing" quadratic form (with signature appropriate to carving out a convex cone of ample line bundles?), instead of relying on the ad-hoc kleinian-geometry argument that told us we had a traditional 4d minkowski spacetime in the abelian surface case. now even those 1d neron-severi lattices of generic abelian and/or other surfaces seem "minkowski"; it's just that "1d minkowski spacetime" is really just "minkowski time", lacking space dimensions entirely ....

That sounds right. I think your question " with signature appropriate to carving out a convex cone of ample line bundles?" is answered affirmatively by the Hodge Index Theorem:

> In mathematics, the **Hodge index theorem** for an algebraic surface *V* determines the signature of the intersection pairing on the algebraic curves *C* on *V*. It says, roughly speaking, that the space spanned by such curves (up to linear equivalence) has a one-dimensional subspace on which it is positive definite (not uniquely determined), and decomposes as a direct sum of some such one-dimensional subspace, and a complementary subspace on which it is negative definite.

And another question:

> (stupid question: is all "this" in some way related to how string-theorists (in the sense of a somewhat old-fashioned physicist slumming in somewhat less old-fashioned algebraic geometry, perhaps) start to develop a taste for higher-dimensional spacetimes?)

Umm, let me just say some stuff. For reasons I don't fully understand, superstrings like "space" to be a Calabi-Yau manifold of dimension 9, like Euclidean space of dimension d times a compact Calabi-Yau of dimension 9-d. (Time brings the total up to 10.) A K3 surface is a simply connected Calabi-Yau of dimension 4. (Real dimension 4, that is.) So, some string theorists like to study superstrings moving around on "space" that's 5d Euclidean space times a K3 surface. The Euclidean aspect is sort of boring so they focus on superstrings moving around on a K3 surface. The partition function of this theory is something like a modular form:

> a weak Jacobi form of index one and weight zero for Z/2×SL(2,Z)

and the coefficients of its Taylor series are mysteriously related to dimensions of representations of the Mathieu group M24. This is "Mathieu moonshine". People are struggly, rather feebly it seems, to understand this. So they're getting interested in different K3 surfaces, and their Neron-Severi lattices.

I'm not really very interested in this stuff - it's too hard for me - but it's produced some papers that have some nice information about the K3 surface that's the Kummer variety of the D4 abelian surface.

Best,
jb

John Baez <john.baez@ucr.edu>
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Wed, Feb 23, 2022 at 12:56 PM

Hi -

By the way, I'm making some progress getting people to figure out which points in hyperbolic space you get by taking one point and acting on it by all of PSL(2,Z[i]):

https://twitter.com/johncarlosbaez/status/1496208958092378112

Nan Ma claims you get a kind of "lattice" in hyperbolic space where each point has 8 nearest neighbors. They drew some quick and dirty pictures here:

https://twitter.com/mananself/status/1496576608089493504

Best,
jb

[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 1:28 PM
To: John Baez <baez@math.ucr.edu>

that looks interesting .... of course might take me a while to catch up on understanding all the pictures ....

by the way i'm pretty annoyed we're not all doing all this in 3d virtual reality ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 1:45 PM
To: John Baez <baez@math.ucr.edu>

by the way, here's another vague silly idea about something to try with some of the anomalously symmetric abelian surfaces we've been fooling around with ....

i vaguely recall some vague rumors that penrose tilings in their quasi-irregular glory are secretly ragged 3d boundaries of irregular domains in a regular 4d lattice-tiling, or something like that-- i could easily have this completely wrong. but if there's anything remotely similar to that that works, then i'm wondering if it connects in any way to any of the stuff about anomalously symmetric abelian surfaces that we've been running into, either in the real-4d "spinor space" or in the corresponding 4d minkowski spacetime ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 2:59 PM
To: John Baez <baez@math.ucr.edu>

a relatively long time ago (approx 12 days) i wrote (quoting you in the first paragraph):

""And I should figure out how this 3d hyperbolic honeycomb picture is connected
to the 4d lattice picture. Obviously the 3d hyperbolic space can be seen as one sheet of a
hyperboloid in the 4d Minkowski space, but where does the honeycomb come from?"

don't quote me on this yet, but i have a vague memory that in certain circumstances similar to this, the "mirrors" are essentially just points in a dual lattice .... or something like that ...."

i just want to take another brief stab at trying to semi-explain what i was trying to get at here; it's a somewhat quandle-oriented perspective on the situation. for people who find quandles annoyingly highbrow and/or just plain annoying, try to remember that conjugacy classes in groups are somewhat prototypical examples of quandles and that you can get a quandle from a homogeneous space equipped with "an equivariant way of assigning to each point a group-element that fixes that point". the homogeneity requirement isn't crucial and the generalization where you omit the fixed-point requirement is also interesting; then it's just "an equivariant way of assigning to each point a group-element". cases where the group-elements are order 2 are perhaps particularly ubiquitous, for example in coxeter-/escher-/martin-gardner-/lewis-carroll-like contexts where the binary "conjugation" operation is something like "reflecting one mirror across another", but these discrete examples tend to embed in continuous examples coming from pseudo-euclidean vector spaces where to each vector lying on some generalized hyperboloid we assign the "reflection" symmetry which is +1 on the eigenspace generated by that vector and -1 on the complementary eigenspace obtained as pseudo-euclidean perp of that vector. sometimes an opposite/ish convention is more useful; in some conventions we get a quandle of hyperbolic hyperplanes that function as mirrors (as in a coxeter-tiling) whereas in other conventions we get something more like "reflection _along_ a vector" (as in a corresponding "root system") instead of "reflection _across_ a hyperplane-mirror" .... obviously i'm being sloppy with the details as usual ....

but anyway, i'm claiming that the nice photogenic hyperbolic 3d tilings that we get from GL(2,G) and GL(2,E) acting on the minkowski spacetime where their neron-severi lattices live are straightforwardly explained by this quandle/ish business of putting eigenvalue +/-1 on the linear subspace generated by some 4d vector vs eigenvalue -/+1 on the pseudo-euclidean perp, where for the 4d vectors we choose to use whichever neron-severi lattice points we prefer, for example perhaps the principal polarizations. (i guess don't get too close to the light-cone if you want a nice clean eigenspace decomposition.) but also that this should (i'm wild-guessing here) generalize to the case of GL(2,D) for D a pretty arbitrary quadratic number-ring; i'm guessing that we'll still have a pretty mirror-ful 3d hyperbolic tiling but that the 2d boundaries of the non-compact 3d regions will be tiled by tiles that look like slightly stubby voronoi regions for the quadratic integers in D, or something like that ....

but this is probably close to what you were already guessing, i imagine ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 3:06 PM
To: John Baez <baez@math.ucr.edu>

one thing that bugs me slightly is some semi-apocryphal story about klein complaining to poincare about poincare naming something after fricke that allegedly fricke didn't have so much to do with and poincare retaliating by naming "kleinian groups" after klein; i'm worried now that the story is somewhat ruined if klein really did (by my standards at least) have a lot to do with kleinian groups. but maybe i should make some effort to learn what "kleinian group" actually means before worrying about it too much ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 3:19 PM
To: John Baez <baez@math.ucr.edu>

you wrote:

"I believe that the principal polarizations are the polarizations D with D.D = 2."

i thought at first that maybe i'm finally beginning to understand some of the conceptual motivation for this conjecture of yours, but then when i tried to confirm it by staring at that grid of numbers i got confused:

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	
.						
.						
.						

i was guessing that the principal polarization should be where that second "1" is on the main diagonal .... i'm still confused .... even though of course i'm not at all clear on how these numbers are supposed to relate to the alleged self-intersection-pairing ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 3:27 PM
To: John Baez <baez@math.ucr.edu>

so at some point i'd really like to see someone do stuff like what nan ma was doing for the gaussian integers but for the golden integers / kleinian integers / etc .... in 3d virtual reality that i can walk / fly around in ....

hmm, so can you give me a "purely / intrinsicly number-theoretic" definition of the neron-severi lattice in these special quadratic-number-ring cases? i guess you're trying to tell me that it's that slightly-generalized trautman "pythagorean spinors" stuff; whatever that means ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Wed, Feb 23, 2022 at 7:54 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> one thing that bugs me slightly is some semi-apocryphal story about klein complaining to poincare about poincare naming something after fricke that allegedly fricke didn't have so much to do with and poincare retaliating by naming "kleinian groups" after klein; i'm worried now that the story is somewhat ruined if klein really did (by my standards at least) have a lot to do with kleinian groups. but maybe i should make some effort to learn what "kleinian group" actually means before worrying about it too much ....

I always forget these things, but Wikipedia says a Kleinian group is a discrete subgroup of PSL(2,C).   Obviously Klein thought about a few of these.

Did I tell my joke theory that Klein's breakdown was caused by his competitor Poincare naming Kleinian groups after him and then someone else naming the Klein 4-group after him?   The idea is that the second of these, whichever it was, was the straw that broke the camel's back.

Best,
jb

John Baez <john.baez@ucr.edu> Thu, Feb 24, 2022 at 2:53 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> so at some point i'd really like to see someone do stuff like what nan ma was doing for the gaussian integers but for the golden integers / kleinian integers / etc .... in 3d virtual reality that i can walk / fly around in ....

Well, I think the first part should be pretty easy and I may try to get someone to do it.

> hmm, so can you give me a "purely / intrinsicly number-theoretic" definition of the neron-severi lattice in these special quadratic-number-ring cases? i guess you're trying to tell me that it's that slightly-generalized trautman "pythagorean spinors" stuff; whatever that means ....

Right. It's not that complicated:

Minkowski space consists of 2x2 self-adjoint complex matrices. There's an obvious sesquilinear map from C^2 x C^2 to 2x2 self-adjoint matrices. If A is any lattice sitting in C, A^2 is a lattice in C^2, and the image of the sesquilinear map is a lattice in 2x2 self-adjoint matrices.

A doesn't even need to be a quadratic number ring.

I'll need to think more about other stuff you said. People on Twitter found out some interesting stuff in the Gaussian and Eisensteinian cases.

Best,
jb