**John Baez <johnb@ucr.edu>**

# The kid who learned math on the street

14 messages

**John Baez** <john.baez@ucr.edu> Fri, Feb 18, 2022 at 1:05 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>

Hi -

A couple of people are claiming the Neron-Severi group of an abelian surface generically has rank 1, and has rank 4 only if the surface is the product of 2 isogenous elliptic curves. This would throw my world into a tizzy - I'll ask around.

Best,
jb

**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Fri, Feb 18, 2022 at 2:18 PM
To: John Baez <baez@math.ucr.edu>

that could easily be for all i know ! i need to think about it ....

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Fri, Feb 18, 2022 at 2:23 PM
To: John Baez <baez@math.ucr.edu>

it could also be that i have some terminology definitions screwed up ....

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Fri, Feb 18, 2022 at 3:16 PM
To: John Baez <baez@math.ucr.edu>

maybe i'm beginning to see what the situation is .... the 4d minkowski spacetime formed by the unitary structures is always there, and the 6d lattice of integer second homology is always "there" .... it's 2 somewhat different "there"s, though, according to what you're telling me .... i need to clarify the common ambient space (of complex de rham cohomology ....) that they're both living inside, and how the property of "being a product of isogenous elliptic curves" or not is supposed to control how the 4d spacetime and the 6d lattice intersect in either a 4d neron-severi lattice or a 1d one, if i get what those people are saying ....

of course i'm not at all sure yet to what extent i'm seeing the right picture yet .... but it's interesting / "good" if we can see how a "generalized bruhat classification" of hodge structures is controlling the dimension of the neron-severi lattice here .... or something like that ....

this is still all pretty close to the wild guess level, though !!

it seems like in principle, we could tell what the neron-severi lattice is by explicitly checking the extent to which "the usual" construction of theta functions goes through .... not completely sure what i mean by that yet, though, and might still be a bit complicated to carry out ....

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Fri, Feb 18, 2022 at 3:44 PM
To: John Baez <baez@math.ucr.edu>

the main thing i'm "disappointed" at so far is that the idea of contrasting curves sharing the same jacobian is seeming maybe a lot rarer than i was hoping ....
i guess it's still supposed to happen though .... still need to think about all this a lot more !

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Sun, Feb 20, 2022 at 12:28 PM
To: John Baez <baez@math.ucr.edu>

so the first thing is, there's no need to panic. we've piled wild guesses on top of wild guesses and the top layer of wild guesses has collapsed into oblivion, but we still have the bottom layer of wild guesses to use as a solid foundation to rebuild on. so i'm going to try thinking outloud here, trying to single out the more solid wild guesses from the more speculative ones .... and at this moment i'm finding it useful to try figuring it out for myself instead of looking in the back of the book yet ....

so, one of the main themes here for me is to work out the connection between the highbrow concept of "line object in a 2-rig" (somewhat ambiguously as to whether we're dealing with "absolute" or "total" 2-rigs or some other variant) and the lowbrow concept of "topological hermitian line bundle" in the presumably easy special case of abelian varieties; thus from a 2-rig we expect to get something like a "2-group" of line objects in it, and when that 2-rig is the 2-rig of quasi/coherent sheaves or holomorphic vector bundles over a (complex) abelian variety v, we then expect some sort of comparison 2-homomorphism to the 2-group of topological hermitian line bundles over the underlying topological torus of v, inducing an actual abelian group homomorphism on pi_0 of these 2-groups; and we christen the image of this abelian group homomorphism the "neron-severi lattice of v". and we'd like to see and calculate with this neron-severi lattice in very concrete ways, especially in the case where v is an abelian surface, and especially now in seeing how the dimension of this neron-severi lattice may correlate to some extent with the degree of symmetry of the abelian surface v.

(there's a whole bunch of more highbrow stuff to say here which i'll mostly try to resist saying in order to try to speed up the process of homing in on the key conceptual mistakes in the concrete calculations. however i'd like to point out the contrast between the concept of "hermitianness" of a line bundle which because it's a purely fiber-wise concept manages to flourish in the lowbrow context of pure homotopy theory and becomes a useful convenience there, vs the concept of "holomorphicness" of a line bundle which raises more middlebrow issues because of the way that it constrains "transition functions" and is thus non-fiber-wise. and as always, you shouldn't be too surprised if more lowbrow things start to seem more highbrow or vice versa. i'd also like to warn that there are potential different shades of meaning of the term "2-group" that are likely to become relevant at some point.)

so, the wild guesses that form the allegedly solid foundation that i'm going to try to rebuild on here include the idea that because we're choosing to deal first just with abelian varieties, we can get away with a lot of simplifying assumptions such as that we only have to worry about "flat" kaehler structures instead of more general ones, so that instead of doing differential geometry on the underlying (higher-dimensional) torus of our abelian variety we can get away with mostly just doing linear algebra on the vector space given as the universal cover of that torus, supplemented by integralness conditions relative to the period lattice in that vector space. there's also a lot of simplifying assumptions to exploit in calculating and seeing the integer cohomology of the torus, reducing it to the group cohomology of the period lattice and seeing for example how the normalized group 2-cocycles classify the hermitian line bundles and how both 2-cocycle and hermitian line bundle can be simultaneously visualized in terms of the geometry of a heisenberg commutator bracket (which more or less becomes the chern connection of the corresponding hermitian line bundle).

but let me try to get more precise for a bit here:

let x be a free abelian group of finite rank d. let b : x^2 -> Z be anti-symmetric bilinear.

we think of b simultaneously as a "carry-digit 2-cocycle" giving a (potentially) non-abelian but first-order-nilpotent ("heisenberg") refinement-group of the period lattice, as an "automorphy factor system" describing an action of that refinement-group on the total space of the trivial hermitian line bundle over the real vector space x#R, and as an "integral degenerate symplectic structure" on the real torus (x#R)/x.  the orbit space of that action is then the total space of a (typically non-trivial) hermitian line bundle over that real torus,and if i understand correctly then the process by which we've thus created this potentially non-trivial hermitian line bundle is sometimes called "descent from the universal cover".

in order to ask whether or not this "descent bundle" is holomorphic, though, we should first put some complex structure s on the real vector space x#R.  then the trivial hermitian line bundle over x#R automatically becomes holomorphic, right??  and then in order for the descent bundle to be holomorphic as well, all we should need is for the action of the refinement-group on the total space of that trivial bundle to be by holomorphic maps, isn't that right??

(so an action of the fundamental group (aka "period lattice" here) on the total space of a trivial bundle over the universal cover is what became of "the transition functions used for glueing" when we shifted from "cover by open sets" to "universal cover", evidently??)

then as a wild guess, perhaps the constraint that that action is by holomorphic maps is essentially equivalent to the real-bilinear extension of b being invariant under the action of u(1) on x#R via s???  and that in turn is equivalent to that real-bilinear extension being the imaginary part of a "pseudo-hermitian" form, just like hermitian except with possibly degenerate signature allowing besides "+" also "0" and "-"???

(maybe it's time for another stab at understanding the wikipedia article on "riemann form"?)

then maybe i'm claiming (modulo various question-marks here) that if (x#R)/x is an abelian variety via s, then the elements of its neron-severi lattice can be concretely identified with the sub-lattice of {b : x^2 -> Z anti-symmetric bilinear} consisting of those b satisfying that constraint wrt s??

so in the case of a complex abelian surface, x#R is a 4d real vector space, and {b : x^2 -> Z anti-symmetric bilinear} is a 6d lattice in the 6d real vector space {b : x^2 -> R anti-symmetric bilinear}, and the constraint subspace is the 4d real vector sub-space inside that of "imaginary parts of pseudo-hermitian forms"??  and that 4d sub-space _is_ a sort-of "4d minkowski spacetime", but the 6d lattice and the 4d sub-space could still mostly miss each other???  allegedly resulting in a 1d neron-severi lattice sometimes, though 4d in some special cases??

i guess it's possible i might have fallen victim here to a silly thinko that i've occasionally fallen into before, mistakenly thinking that it's difficult for a high-dimensional linear subspace to miss a high-dimensional lattice inside of an ambient linear space ....

i still need to think about all of this some more, though!

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Sun, Feb 20, 2022 at 12:34 PM
To: John Baez <baez@math.ucr.edu>

i wrote:

"so, one of the main themes here for me is to work out the connection between the highbrow concept of "line object in a 2-rig" (somewhat ambiguously as to whether we're dealing with "absolute" or "total" 2-rigs or some other variant) and the lowbrow concept of "topological hermitian line bundle" in the presumably easy special case of abelian varieties; thus from a 2-rig we expect to get something like a "2-group" of line objects in it, and when that 2-rig is the 2-rig of quasi/coherent sheaves or holomorphic vector bundles over a (complex) abelian variety v, we then expect some sort of comparison 2-homomorphism to the 2-group of topological hermitian line bundles over the underlying topological torus of v, inducing an actual abelian group homomorphism on pi_0 of these 2-groups; and we christen the image of this abelian group homomorphism the "neron-severi lattice of v"."

?? in other words, my current neron-severi obsession has a lot to do with the idea that this is right where highbrow crashes into lowbrow ....

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**John Baez** <john.baez@ucr.edu> Sun, Feb 20, 2022 at 11:03 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'm reading your stuff and thinking about it...

> in order to ask whether or not this "descent bundle" is holomorphic, though, we should first put some complex structure s on the real vector space x#R. then the trivial hermitian line bundle over x#R automatically becomes holomorphic, right??

Yes.

> and then in order for the descent bundle to be holomorphic as well, all we should need is for the action of the refinement-group on the total space of that trivial bundle to be by holomorphic maps, isn't that right??

That sounds right.

This is where things get nice and concrete and potentially calculable, which is what I want now:

> then as a wild guess, perhaps the constraint that that action is by holomorphic maps is essentially equivalent to the real-bilinear extension of b being invariant under the action of u(1) on x#R via s???
> and that in turn is equivalent to that real-bilinear extension being the imaginary part of a "pseudo-hermitian" form, just like hermitian except with possibly degenerate signature allowing besides "+" also "0" and "-"???

> then maybe i'm claiming (modulo various question-marks here) that if (x#R)/x is an abelian variety via s, then the elements of its neron-severi lattice can be concretely identified with the sub-lattice of {b : x^2 -> Z anti-symmetric bilinear} consisting of those b satisfying that constraint wrt s??

That last one sounds right. The book by Lange and Birkenhake assures me the Nerson-Severi group of an abelian variety V/L for some lattice L in a complex vector space V can be identified with

1) the group of real-valued antisymmetric bilinear forms b on V that take integer values on L and obey b(iv,iw) = b(v,w)

This looks like just what you said. They say the Neron-Severi group can also be identified with

2) the group of hermitian forms h on V whose imaginary part takes integer values on L

Here "hermitian form" means a complex-valued, sesquilinear map h with h(v,w) being the complex conjugate of h(w,v).

I think this agrees with your guess about the "pseudo-hermitian" description of the Neron-Severi group. They're not assuming any positivity or definiteness in their "hermitian form".

The relation between 1) and 2) is that b is the imaginary part of h.

So I think you're right, and I think this stuff reduces the computation of the Neron-Severi group to something like linear algebra.

Best,
jb

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Mon, Feb 21, 2022 at 6:40 AM
To: John Baez <baez@math.ucr.edu>

i wrote (quoted by you):

"then as a wild guess, perhaps the constraint that that action is by holomorphic maps is essentially equivalent to the real-bilinear extension of b being invariant under the action of u(1) on x#R via s??? and that in turn is equivalent to that real-bilinear extension being the imaginary part of a "pseudo-hermitian" form, just like hermitian except with possibly degenerate signature allowing besides "+" also "0" and "-"???"

so i really want to work out the proofs of these now. even just a calculational proof would probably be somewhat helpful if i work it out for myself, but nice conceptual proofs would be better.

it does seem amazing being able to understand so much of this after it seeming so opaque for so long; it's so much easier when you have some decent big-ish conceptual pictures as a guide.

i did finally allow myself to actually read in detail the short paragraph on "complex tori" in the wikipedia article on "neron-severi group":

## For complex tori [ edit ]

Complex tori are special because they have multiple equivalent definitions of the Neron-Severi group. One definition uses its complex structure for the definition[1]pg 30. For a complex torus {\displaystyle X=V/\Lambda } , where V is a complex vector space of dimension n and \Lambda is a lattice of rank 2n embedding in V , the first Chern class c_{1} makes it possible to identify the Neron-Severi group with the group of Hermitian forms H on V such that

{\displaystyle {\text{Im}}H(\Lambda ,\Lambda )\subseteq \mathbb {Z} }

Note that {\displaystyle {\text{Im}}H} is an alternating integral form on the lattice \Lambda

(this is embedded in a slightly longer section on "First Chern class and integral valued 2-cocycles".)

the cryptic wikipedia article on "riemann form" (cryptic enough to probably qualify as _wrong_ , i think, given how they require positive-definiteness of the hermitian form while also saying "The alternatization of the Chern class of any factor of automorphy is a Riemann form" and linking to only a very generic discussion of automorphy when you click on "factor of automorphy") is also seeming a lot less cryptic now ....

it's pretty useful to me reading these paragraphs for confirmation now but it'd have been very difficult for me to actually learn the ideas by reading them.

but anyway, the point of this is supposed to be to make the neron-severi lattice of a complex abelian variety concrete enough to readily settle questions about its dimension (and/or about the cone where the positive-definite hermitian forms and the ample line bundles live) in particular and/or general cases. at the moment i'm somewhat agnostic about what that dimension "should" be and/or what i want it to be for general abelian surfaces; in some sense it seems good enough for it to be co-compact inside the minkowski spacetime just in specially symmetric cases where we can produce actual helpful pictures ....

....
[Quoted text hidden]

**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Mon, Feb 21, 2022 at 6:49 AM

To: John Baez <baez@math.ucr.edu>

i want to look at the notes by chris peters that tim hosgood linked to (the notes where allegedly "they explicitly use the phrase “light cone”" ....) but i have some technical problems getting .djvu files to work. i might be able to get it to work though.

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**John Baez** <john.baez@ucr.edu> Mon, Feb 21, 2022 at 8:55 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> "then as a wild guess, perhaps the constraint that that action is by holomorphic maps is essentially equivalent to the real-bilinear extension of b being invariant under the action of u(1) on x#R via s??? and that in turn is equivalent to that real-bilinear extension being the imaginary part of a "pseudo-hermitian" form, just like hermitian except with possibly degenerate signature allowing besides "+" also "0" and "-"???"
>
> so i really want to work out the proofs of these now. even just a calculational proof would probably be somewhat helpful if i work it out for myself, but nice conceptual proofs would be better.

Great! I'm interested in this too.

> i did finally allow myself to actually read in detail the short paragraph on "complex tori" in the wikipedia article on "neron-severi group":

I should check that out - it came out looking bad in your email.

One thing I like about Lange and Birkenhake's book *Complex Abelian Varieties* is that they do a lot of stuff for complex tori before specializing to complex abelian varieties. It's kind of nice to have a "fall guy", a similar but less specialized and beautiful concept, to compare the beautiful one to. But the phrase "Neron-Severi group of a complex torus" sounds funny to me. Is it trivial for complex tori that aren't abelian varieties?

> the cryptic wikipedia article on "riemann form" (cryptic enough to probably qualify as _wrong_ , i think, given how they require positive-definiteness of the hermitian form while also saying "The alternatization of the Chern class of any factor of automorphy is a Riemann form" and linking to only a very generic discussion of automorphy when you click on "factor of automorphy") is also seeming a lot less cryptic now ....

Yeah, I got fed up with that article. I think sometime I'll edit it to make it correct, and make it say something useful about how Riemann forms can be used to describe line bundles on complex abelian varieties.

Best,
jb

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Mon, Feb 21, 2022 at 10:09 AM
To: John Baez <baez@math.ucr.edu>

"I should check that out - it came out looking bad in your email."

i thought the sentence that says "Complex tori are special because they have multiple equivalent definitions of the Neron-Severi group" was still visible. so it's pretty definitely worth looking at, particularly because it's short.

"One thing I like about Lange and Birkenhake's book Complex Abelian Varieties is that they do a lot of stuff for complex tori before specializing to complex abelian varieties. It's kind of nice to have a "fall guy", a similar but less specialized and beautiful concept, to compare the beautiful one to."

i misread at first and thought that the "fall guy" you were talking about was non-abelian varieties, in response to which i was going to mention the general theme of studying those aspects of varieties that "factor through" their abelianization/albanization; maybe that's still relevant here .... there's some particular wikipedia article that i noted down (though at the moment can't find where i noted it down ....) that discusses this theme ....

"But the phrase "Neron-Severi group of a complex torus" sounds funny to me. Is it trivial for complex tori that aren't abelian varieties?"

if not then i must be misunderstanding something .... seems like "complex torus with nontrivial neron-severi group" is a definition of "complex abelian variety" ....

if it's really true that abelian surface neron-severi lattices aren't always 4d then that might look interesting in the larger context of 2d complex toruses; the general trend of the examples getting less generic as the neron-severi dimension goes up ....

"Yeah, I got fed up with that article. I think sometime I'll edit it to make it correct, and make it say something useful about how Riemann forms can be used to describe line bundles on complex abelian varieties."

on the talk page it says:

"Add relation to complex tori and abelian varieties. Expand relation to line bundles, Chern classes, and factors of automorphy. RobHar (talk) 22:22, 15 January 2008 (UTC)

Last edited at 22:22, 15 January 2008 (UTC)."

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**JAMES DOLAN** <james.dolan1@students.mq.edu.au> Wed, Feb 23, 2022 at 12:40 PM
To: John Baez <baez@math.ucr.edu>

i wrote:

"i misread at first and thought that the "fall guy" you were talking about was non-abelian varieties, in response to which i was going to mention the general theme of studying those aspects of varieties that "factor through" their abelianization/albanization; maybe that's still relevant here .... there's some particular wikipedia article that i noted down (though at the moment can't find where i noted it down ....) that discusses this theme ...."

i think i found the wikipedia excerpt i was thinking of, in the article on "abelian varieties":

"The study of differential forms on C, which give rise to the abelian integrals with which the theory started, can be derived from the simpler, translation-invariant theory of differentials on J. The abelian variety J is called the Jacobian variety of C, for any non-singular curve C over the complex numbers."

that might sound a bit bland, but i have the feeling that they're actually saying something important in the context of understanding the conceptual and historical role of abelian varieties ....

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**John Baez** <john.baez@ucr.edu> Wed, Feb 23, 2022 at 12:49 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> "The study of differential forms on C, which give rise to the abelian integrals with which the theory started, can be derived from the simpler, translation-invariant theory of differentials on J. The abelian variety J is called the Jacobian variety of C, for any non-singular curve C over the complex numbers."
>
> that might sound a bit bland, but i have the feeling that they're actually saying something important in the context of understanding the conceptual and historical role of abelian varieties ....

That sounds right! There's a description of the Jacobian of a variety in terms of 1-forms ("holomorphic differentials") and I guess that's how it arose. A more modern description would be "the dual of the abelianization".

Btw, I still find it cool/mysterious that besides the concept of Pontryagin dual of a free abelian group, which is a torus, we have the concept of "dual lattice" of a lattice in a vector space, and "dual torus" of a torus that's the quotient of a vector space by a lattice.

Best,
jb