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John Baez <johnb@ucr.edu>

stacks

21 messages

John Baez <john.baez@ucr.edu> Sat, Mar 5, 2022 at 9:45 AM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>

Hi -

On Twitter a guy I know, Simon Pepin Lehalleur, wrote:

> The stacky POV is not so essential to study line bundles in isolation (one reason being that "the Picard stack is a Gm-gerbe over the Picard scheme"). It really comes into its own when studying the interaction of line bundles with other more complicated moduli problems.

I guess I understand this - Gm is the multiplicative group scheme - but I'd like to understand it better, both from the "standard" viewpoint and any viewpoints we might have, like "the free 2-rig on a line object". You've occasionally threatened/promised to talk more about "highbrow" topics, and this would fit in there I guess.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sat, Mar 5, 2022 at 12:33 PM
To: John Baez <baez@math.ucr.edu>

you're opening up a huge can of worms here, which is a good thing of course except maybe that we already have another half dozen or so recently opened worm-cans lying around. but let's proceed anyway, for the moment at least.

first a brief word about the terminology and concept of "gerbe", of which i have only very limited understanding so far:

recently we've been making a lot of progress in understanding holomorphic line bundles over a complex projective variety v from a certain lowbrow/ish viewpoint which emphasizes connecting them to the hodge diamond of v (and in fact that's what motivates the epithet "lowbrow", in that the hodge diamond is telling us something about the lowbrow concepts of ordinary singular cohomology and de rham cohomology and the relationship between them!); in particular to the point (1,1) in that diamond, but also probably to the whole "shadow" of that point (i hope this looks ok typographically):

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(we've been focusing on the case where v is an abelian variety, but we have some mild confidence that this is a sort-of "universal" case through which the more general case systematically factors; or in any case we know that this abelian case is interesting and beautiful and simple enough that our effort in understanding it isn't wasted.)

of course recently we've been learning about how the actual rank of the neron-severi group may be lower than the upper bound obtained from a glance at (1,1) in the hodge diamond, but that doesn't (much) lessen the importance of the hodge diamond here.

so, the above was all just preamble to some idle speculation that "the next step" after holomorphic line bundles is gerbes (and/or possibly "holomorphic gerbes", whatever that means), and that we should try to use the hodge diamond as a guide to understanding gerbes in a way analogous to how we use it as a guide to understanding holomorphic line bundles.

that is: when i look at some definition of "gerbe" it looks naively like a somewhat "turn-the-crank" attempt to come up with "the logical next step after holomorphic line bundles"; and this makes me wonder whether there is some particular point in the hodge diamond at (and/or beneath) which information about gerbes is concentrated, analogous to the point (1,1) at and beneath which information about holomorphic line bundles is concentrated.

but when i actually try to guess what such a point might be, i start doubting that i'm on the right track. a very simpleminded guess might be to look at the point (2,2), but does that sound like it's related to gerbes (whatever they are ....)?? what "lives" at (2,2); is it something like "algebraic cycles of dimension (and/or co-dimension) 2"??

i tried a bit of mindless googling here, and i wound up with:

"This aim of this paper is to define higher categorical invariants (gerbes) of codimension two algebraic cycles and provide a categorical interpretation of the intersection of divisors on a smooth proper algebraic variety. This generalization of the classical relation between divisors and line bundles sheds some light on the geometric significance of the classical Bloch-Quillen formula (5.1.3) relating Chow groups and algebraic K-theory."

from "Cup Products, the Heisenberg Group, and Codimension Two Algebraic Cycles" by Ettore Aldrovandi and Niranjan Ramachandran (from approx 2015).

(hmm, maybe we _should_ try actually reading this paper a bit further; there's some suggestive stuff in it.)

but anyway, this is just something i've been meaning to tell you about "gerbes" and their possible hodge-theoretic analogousness to holomorphic line bundles. but i _don't_ actually have much reason yet to believe that gerbes are actually "good" for anything; it's possible that lehalleur might be giving a hint here about something they're good for (namely for studying certain kinds of stacks that are "only mildly stacky"), but i'll have to think about it some more.

but anyway, this is _not_ my main response to your message below; rather it's just a side-comment about "gerbes" that i've been meaning to say for a while already, with lehalleur's mention of gerbes serving as my excuse for trying to fit it into the discussion here. hopefully i'll manage to send my actual main response (getting into actual "highbrow" stuff about using 2-rigs to understand moduli stacks of vector bundles and so forth) relatively soon.

....

[Quoted text hidden]

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>

Sat, Mar 5, 2022 at 6:37 PM

Hi -

> that is: when i look at some definition of "gerbe" it looks naively like a somewhat "turn-the-crank" attempt to come up with "the logical next step after holomorphic line bundles";

There's a general notion of gerbe, which means roughly "locally connected (and nonempty) stack". And then there's something a bit more specific, a U(1) gerbe, which is "the logical next step after topological circle bundles": U(1) gerbes on a topological space X are classified by H^3(X,Z). And there's something similar but maybe more relevant here, a C* gerbe where C* is the group of invertible complex numbers, which is "the logical next step after topological complex linear bundles".

The last sort of gerbe probably comes in a holomorphic flavor - Brylinski wrote a paper about holomorphic gerbes a long time ago, which was probably about this. Here you probably need to think of C* as a group in complex manifolds. I know for sure it comes in an algebraic flavor where C* gets replaced by G^m, the "multiplicative group scheme", whose underlying affine scheme is the punctured affine line.

But anyway:

> but when i actually try to guess what such a point might be, i start doubting that i'm on the right track. a very simpleminded guess might be to look at the point (2,2), but does that sound like it's related to gerbes (whatever they are ....)?? what "lives" at (2,2); is it something like "algebraic cycles of dimension (and/or co-dimension) 2"??

I'm having trouble imagining holomorphic gerbes living at (2,2), since in the topological case C* gerbes like U(1) gerbes are classified by 3rd integral cohomology!

> i tried a bit of mindless googling here, and i wound up with:
>
> from "Cup Products, the Heisenberg Group, and Codimension Two Algebraic Cycles" by Ettore Aldrovandi and Niranjan Ramachandran (from approx 2015).
>
> (hmm, maybe we _should_ try actually reading this paper a bit further; there's some suggestive stuff in it.)

Hmm, that's interesting - the "codimension 2 algebra cycles" really do suggest something about (2,2) is going on.

So now I'm finally interested in what Brylinski said about holomorphic gerbes!

But, as you maybe sorta said, I think all this stuff goes in a different direction than Lehalleur's remark "the Picard stack is a G^m-gerbe over the Picard scheme". I think this is just trying to say that the Picard scheme forgets that holomorphic line bundles have symmetries, and making it into a stack requires sticking in those symmetries, which means throwing in G^m. I don't think it's so much about going up to 3rd cohomology or (2,2) cohomology or something like that.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Mar 22, 2022 at 6:26 AM
To: John Baez <baez@math.ucr.edu>
Cc: Todd Trimble <topological.musings@gmail.com>

[i'm cc-ing this to todd in part for the purpose of having him serve as a sort of "conscience" who can more easily recognize the many lapses in rigor here, and who might also be able to come up with possible remedies for such lapses. anyway, i admit that i haven't succeeded in making this exposition as accessible to either of you as i'd have hoped, and that trying to decipher it could become fairly annoying in some places. as usual i may have to resort to trying to explain to you in person the ideas that i've failed to explain in writing.]

i wrote:

"hopefully i'll manage to send my actual main response (getting into actual "highbrow" stuff about using 2-rigs to understand moduli stacks of vector bundles and so forth) relatively soon."

so here's a stab at following through on that, setting out some preliminary thoughts on how the concept of "moduli stack of bundles of a certain kind over a certain algebraic variety" looks from the viewpoint of total 2-rigs:

first, let's remind ourselves that given a 2-homomorphism of 2-rigs r1 -h-> r2, we can think of h as essentially "an interpretation of the theory r1 into the universe provided by r2", or somewhat more geometrically as "a bundle of r1-models over the spectrum of r2".

(this is especially so in the case where the spectrum of models of r1 is "purely stacky" while the spectrum of models of r2 is "purely unstacky", in which case h(x) is roughly speaking "the associated vector bundle of the group representation x wrt the principal fiber bundle given by h"; but it's morally true in greater generality than just that case.)

next, let's think for a moment about what the moral implications would be if the bicategory _2-affine 2-scheme_ (that is, the opposite of the bicategory of total 2-rigs) were cartesian-closed. in that case, the exponential spec(r1)^spec(r2) would morally be "the moduli stack of r1-model bundles over spec(r2)".

of course the moral force of this implication is lessened by the fact that the bicategory _2-affine 2-scheme_ _isn't_ actually cartesian-closed. however there's a recurring pattern in mathematics that when cartesian-closedness fails for some category (or bicategory) c, it doesn't always fail all at once, and that such a partial failure can sometimes be ameliorated. let me mention 3 prominent examples of this pattern: c := the category of hausdorff spaces, c := the category of affine schemes, and the current example where c := the bicategory of 2-affine 2-schemes.

(an important warning here about the fact that this alleged "pattern" isn't completely worked out yet, at least not by me: i have many unanswered questions about to what extent this alleged pattern can and/or should be systematized and functorialized!   it might be a big project to clarify all of the similarities and contrasts between the examples.)

here's how the pattern typically runs:

first, we have some nice category c of "spaces", and we feel that it'd be nice if c were cartesian-closed, but then we discover that it isn't. but then we notice that there _are_ some special c-objects which are capable of serving as "exponents"; that is, the functor of taking cartesian product with one of these "exponent objects" e has a right adjoint "b |-> b^e" where we may call b^e "the exponential given by the base b to the power of the exponent e".

typically it's only exceptionally "small" objects that qualify as exponent-objects; for example when c := _hausdorff space_ the exponent-objects are the compact hausdorff spaces, while when c := _affine scheme_ the exponent-objects are the infinitesimal affine schemes (meaning those with finite-dimensional function-algebra).

however, we then discover some way to glue these exceptionally small exponent-objects into somewhat larger objects which we might call "generalized exponents", obtained (roughly) as some sort of formal colimits of the exponents, and we find that the category of these generalized exponents is a somewhat nice cartesian-closed approximation to the original category c of "spaces". thus for example when c := _hausdorff space_, the generalized exponents are the compactly generated hausdorff spaces, while when c := _affine scheme_, the generalized exponents are the cocommutative coalgebras over the base field. (notice that in this latter example the generalized exponents aren't a full subcategory of the original spaces.)

however, the main job of the generalized exponents isn't to replace the original spaces but rather to act as functorial operations on the original spaces.

an excellent example to think about here is the functorial operation on commutative rings which assigns to a ring r1 the ring of formal power serieses with coefficients in r1. this is a right-adjoint endofunctor on _commutative ring_ or equivalently a left-adjoint endofunctor on _affine scheme_, somewhat confusingly sometimes called a "bi-ring" in the context of plethories and tall-wraith monoids; however it's a very special kind of bi-ring coming from a cocommutative coalgebra. the corresponding right-adjoint endofunctor on _affine scheme_ assigns to an affine scheme s1 the affine scheme of "formal paths in s1". in other words, despite the fact that e1 := the spectrum of [formal power serieses with constant coefficients] is _not_ an exponent-object in _affine scheme_, we obtain a good substitute for the missing exponential "s1^e1".

this particular e1 is an example of a generalized exponent which is not a true exponent but rather is "glued together" from true exponents; thus geometrically e1 is the increasing union of "the walking tangent vector" >-> "the walking 2nd-order tangent vector" >-> "the walking 3rd-order tangent vector" >-> ... .

the fact that the generalized exponent e1 is not a true exponent but rather is glued together from true exponents is what i'm proposing to imitate when we categorify the situation by considering the bicategory _2-affine 2-scheme_ in place of the category _affine scheme_!!

thus within the bicategory _2-affine 2-scheme_, affine schemes qualify as exponent-objects, but the projective schemes glued together from affine schemes qualify only as _generalized_ exponents, which (somewhat conjecturally) we can construe as the categorified analog of cocommutative coalgebras; that is as "cosymmetric comonoidal total categories". thus instead of construing a projective scheme v as the total 2-rig of quasicoherent sheaves over v, we can construe it as a generalized exponent e_v := the "dual" object given by the cosymmetric comonoidal total category of "quasiherent cosheaves" over v. and in this way, we obtain a (2-)functorial operation on the bicategory _2-affine 2-scheme_ that assigns to a 2-affine 2-scheme s1 the generalized exponential s1^[e_v] which is morally "the moduli stack of bundles of s1-points over the projective scheme v".

thus as advertised we've placed the concept of "moduli stacks of bundles of a certain kind over a projective variety v" in the setting of total 2-rigs (and their formally dual objects the 2-affine 2-schemes); however our approach has the peculiar/interesting feature of bringing cosymmetric comonoidal total categories into the picture alongside their cousins the total 2-rigs.

my hope is to apply this approach to for example the "picard stack" (aka moduli stack of line bundles) of a projective variety v (for example an abelian variety), especially in combination with the "belief" method, in such a way as to gain conceptual and calculational insights ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Wed, Mar 23, 2022 at 4:46 AM
To: John Baez <baez@math.ucr.edu>

me: "but anyway, this is _not_ my main response to your message below; rather it's just a side-comment about "gerbes" that i've been meaning to say for a while already, with lehalleur's mention of gerbes serving as my excuse for trying to fit it into the discussion here."

you: "But, as you maybe sorta said, I think all this stuff goes in a different direction than Lehalleur's remark "the Picard stack is a G^m-gerbe over the Picard scheme"."

yes, that's sort-of what i said.

you: "There's a general notion of gerbe, which means roughly "locally connected (and nonempty) stack"."

there's all sorts of potential terminological confusion here .... let me tell you where i'm getting my information about "gerbe" from in this context: an old ams "what is ..." article by nigel hitchin: https://www.ams.org/notices/200302/what-is.pdf

here's what hitchin says: "A holomorphic gerbe is then the geometrical object whose equivalence classes are elements
in the next sheaf cohomology group H^2(M,O^*)."

hmm, so perhaps we can blame hitchin for some things here: hitchin's title is "what is ... a gerbe?" without any mention of holomorphicness, but then they seem to take it for granted that the interesting ones are just the holomorphic ones, perhaps similarly to the way an algebraic geometer (of a certain kind ....) might feel about non- vs -holomorphic line bundles.

by the way as you might guess hitchin's whole article looks interesting and it's only two pages ....

so anyway, from my slowly acquired and still limited understanding of holomorphic vs non-holomorphic line bundles i tend now to be mentally prepared for some sort of "cohomological level-shifts" in dealing with holomorphic objects vs non-holomorphic ones .... i admit that it still seems unlikely for (2,2) in the hodge diamond to connect somehow with holomorphic gerbes, but i'm keeping an open mind about it while searching for general patterns ....

i suppose one question here is: is there some sort of analog of the appell-humbert short exact sequence involving H^2(M,O^*) instead of H^1(M,O^*), and if so then what does that exact sequence (or whatever it is) look like??

by the way, when i just searched on "higher picard groups" i stumbled across a 2012 paper by Alex Chirvasitu and Theo Johnson-Freyd on "The fundamental pro-groupoid of an affine 2-scheme" that looks very interesting .... i'm not actually sure how relevant it is to this particular part of our discussion, but it does look annoyingly relevant to the larger discussion .... (annoying in the sense that it's always annoying trying to catch up on what other people seem to have done a relatively long time ago) ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Wed, Mar 23, 2022 at 11:07 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

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> here's what hitchin says: "A holomorphic gerbe is then the geometrical object whose equivalence classes are elements
> in the next sheaf cohomology group H^2(M,O^*)."

You can also define topological gerbes, which are entities that exist over any topological space M and are classified by H^3(M,Z). But Hitchin, going straight to the holomorphic case, is talking about a thing classified by H^3(M,O^*).

Brylinski gives a more geometrical, more categorical definition of a holomorphic gerbe here:

https://ncatlab.org/nlab/files/Brylinski94.pdf

> by the way as you might guess hitchin's whole article looks interesting and it's only two pages ....

I should read that. I seem to recall he has a rather crass, avoid-all-the-categorical-concepts approach to gerbes compared to some other people like Brylinski or Urs Schrieber, but he probably does some interesting things with them.

> i suppose one question here is: is there some sort of analog of the appell-humbert short exact sequence involving H^2(M,O^*) instead of H^1(M,O^*), and if so then what does that exact sequence (or whatever it is) look like??

One way to think about the Appell-Humbert short exact sequence is that you can take the abelian group H^1(M,O^*) and make it into a topological group (arising from the Picard scheme), and then look at

0 -> identity component of H^1(M,O^*) -> H^1(M,O^*) -> {connected components of H^1(M,O^*)} -> 0

Then the identity component of H^1(M,O^*) is the Picard group of M and the group of connected components of H^1(M,O^*) is the Neron-Severi group.

If we can make the abelian group H^3(M,O^*) into a topological group we're bound to get an analogous short exact sequence

0 -> identity component of H^2(M,O^*) -> H^1(M,O^*) -> {connected components of H^2(M,O^*)} -> 0

so it might be fun to look at this in some examples. I'll conjecture that the identity component is the "2-Picard group", namely the group of isomorphism classes of holomorphic gerbes that have trivial underlying topological gerbes. And I'll conjecture that the group of connected components is the "2-Neron Severi group", namely the group of isomorphism classes of topological gerbes that actually come from holomorphic gerbes.

(Here I'm just copying the Appell-Humber theorem and betting it still works one level up.)

> by the way, when i just searched on "higher picard groups" i stumbled across a 2012 paper by Alex Chirvasitu and Theo Johnson-Freyd on "The fundamental pro-groupoid of an affine 2-scheme" that looks very interesting .... i'm not actually sure how relevant it is to this particular part of our discussion, but it does look annoyingly relevant to the larger discussion .... (annoying in the sense that it's always annoying trying to catch up on what other people seem to have done a relatively long time ago) ....

I read this paper kinda carefully when Joe Moeller and I was starting to write our paper on 2-plethories, because were looking for some information on 2-rigs, and this paper proposed a theory of them. Then we got bogged down, and then Todd saved us when he joined our project and proposed using absolute 2-rigs. But there's nice stuff in this paper. When I read it, I felt like it was inspired by you. Maybe it was inspired by Martin Brandenburg's paper Tensor categorical foundations of algebraic geometry -he admits to being inspired by your approach using 2-groups and 2-rigs.

Best,
jb
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 24, 2022 at 12:47 AM
To: John Baez <baez@math.ucr.edu>

you: ".... I seem to recall he has a rather crass, avoid-all-the-categorical-concepts approach to gerbes compared to some other people like Brylinski or Urs Schrieber, ..."

"crass" here might be very approximately what i sometimes mean by "lowbrow", but of course it's all very relative (wrt not only translations but also reflections). anyway it's not unusual for me to get some key ideas more quickly from lowbrow discussions than from highbrow ....

you:

"One way to think about the Appell-Humbert short exact sequence is that you can take the abelian group H^1(M,O^*) and make it into a topological group (arising from the Picard scheme), and then look at

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Then the identity component of H^1(M,O^*) is the Picard group of M and the group of connected components of H^1(M,O^*) is the Neron-Severi group.

If we can make the abelian group H^3(M,O^*) into a topological group we're bound to get an analogous short exact sequence

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so it might be fun to look at this in some examples. I'll conjecture that the identity component is the "2-Picard group", namely the group of isomorphism classes of holomorphic gerbes that have trivial underlying topological gerbes. And I'll conjecture that the group of connected components is the "2-Neron Severi group", namely the group of isomorphism classes of topological gerbes that actually come from holomorphic gerbes.

(Here I'm just copying the Appell-Humber theorem and betting it still works one level up.)"

ok, that seems helpful and i'll have to think more about it, but maybe what i should have said is that after you identify this possible "2-neron-severi group", can you relate it to some dolbeault cohomology groups analogously to how ordinary neron-severi is related to (1,1) dolbeault? (i should probably say "how ordinary neron-severi is allegedly related to (1,1) dolbeault" since there's a lot about this that i'm not very clear on yet, like for example what dolbeault cohomology really is ....)

also i'm vacillating about whether i should be thinking more about the "appell-humbert theorem" which you and others seem to be telling me is only about complex toruses, vs about some similar statement which is only about complex projective varieties (or only about something more general, maybe) ....

(for a moment there i thought maybe it was vaccillating and they were insinuating that cows are indecisive ....)

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 24, 2022 at 12:51 AM
To: John Baez <baez@math.ucr.edu>

me:

"ok, that seems helpful and i'll have to think more about it, but maybe what i should have said is that after you identify this possible "2-neron-severi group", can you relate it to some dolbeault cohomology groups analogously to how ordinary neron-severi is related to (1,1) dolbeault? (i should probably say "how ordinary neron-severi is allegedly related to (1,1) dolbeault" since there's a lot about this that i'm not very clear on yet, like for example what dolbeault cohomology really is ....)"

i guess it's possible that you _did_ secretly already address that, and that i might have to read what you said more carefully to see if so ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Thu, Mar 24, 2022 at 6:16 AM
To: John Baez <baez@math.ucr.edu>
Cc: Todd Trimble <topological.musings@gmail.com>

i wrote:

"however, the main job of the generalized exponents isn't to replace the original spaces but rather to act as functorial operations on the original spaces."

so maybe i can now summarize my main message a bit more clearly now, using the above as a hint; something like the following:

"it would be nice" if the projective schemes were exponent-objects in the bicategory _2-affine 2-scheme_, because then for a 2-affine 2-scheme x we could express the moduli stack of x-point bundles over a projective scheme s as the exponential x^s, but unfortunately they're not; instead only the affine schemes are exponent-objects in _2-affine 2-scheme_. however even though s is not an exponent-object in _2-affine 2-scheme_, there's nevertheless a (2-)functorial operation taking a 2-affine scheme x to another 2-affine 2-scheme which is morally "the moduli stack of x-point bundles over s". this functorial operation is encoded by the cosymmetric comonoidal total category obtained by correspondingly glueing together the cosymmetric comonoidal total categories obtained as duals of the symmetric monoidal total categories of quasicoherent sheaves over the affine pieces from which s is glued together.

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Thu, Mar 24, 2022 at 11:47 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

On Wed, Mar 23, 2022 at 11:07 PM John Baez <john.baez@ucr.edu> wrote:

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> so it might be fun to look at this in some examples. I'll conjecture that the identity component is the "2-Picard group", namely the group of isomorphism classes of holomorphic gerbes that have trivial underlying topological gerbes. And I'll conjecture that the group of connected components is the "2-Neron Severi group", namely the group of isomorphism classes of topological gerbes that actually come from holomorphic gerbes.
>
> (Here I'm just copying the Appell-Humbert theorem and betting it still works one level up.)

Actually I wasn't copying the Appell-Humbert theorem, which gives us more detailed information when M is a complex torus. I was just copying some preliminary stuff about the Picard group and Neron-Severi group.

But then I tried to copy the Appell-Humbert theorem. And then I found a paper:

- Oren Ben-Bassat, Gerbes and the holomorphic Brauer group of complex tori

which is really all about a gerbe analogue of the Appell-Humbert theorem!

The original Appell-Hulmber theorem describes the Neron-Severi group as consisting of alternating bilinear forms A: L x L -> Z, where L is the lattice associated to our complex torus, that are "compatible with the complex structure" in the sense that

A(ix,iy) = A(x,y).

So, I guessed the 2-Neron Severi group consists of alternating triilinear forms A: L x L x L -> Z such that

A(ix,iy,iz) = A(x,y,z)

But this paper says the right equation is

A(ix,iy,iz) = A(ix,y,z) + A(x,iy,z) + A(x,y,iz)

I don't know what that means.

This paper also describes the 2-Picard group as a certain torus.

So it's all a lot like the classical case, and it must go on for n-gerbes.

> "ok, that seems helpful and i'll have to think more about it, but maybe what i should have said is that after you identify this possible "2-neron-severi group", can you relate it to some dolbeault cohomology groups analogously to how ordinary neron-severi is related to (1,1) dolbeault? (i should probably say "how ordinary neron-severi is allegedly related to (1,1) dolbeault" since there's a lot about this that i'm not very clear on yet, like for example what dolbeault cohomology really is ....)"
>
> i guess it's possible that you _did_ secretly already address that, and that i might have to read what you said more

> carefully to see if so ....

I didn't really say much about that. I really just said that just as the Neron-Severi group was a subgroup of H^2 so the 2-Neron-Severi group is a subgroup of H^3. But thanks to this paper I can say more. The Neron-Severi group is a subgroup of H^{1,1}, and the 2-Neron Severi group is a subgroup of H^{1,2} + H^{2,1}. Ha! So it cleverly dodges the obvious and obviously false guess that it's a subgroup of H^{1.5, 1.5}.

I don't know how these facts fit with the "compatibility with multiplication by i" stuff I was mentioning above. E.g., it must be true that any antisymmetric bilinear forms with

A(ix,iy) = A(x,y).

is in H^{1,1}, but not conversely. But I don't know what condition on A is just enough to imply it's in H^{1,1} !

So now I want a condition on antisymmetric n-linear forms that makes them be elements of H^{j,k} where j+k=n.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 25, 2022 at 12:05 AM
To: John Baez <baez@math.ucr.edu>

this is all great and really helpful, especially the part about:

"Ha! So it cleverly dodges the obvious and obviously false guess that it's a subgroup of H^{1.5, 1.5}."

we really need to think and talk about this stuff a lot more .... really trying to get the concrete examples to give us a sense of the big conceptual picture and vice versa ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sat, Mar 26, 2022 at 3:10 PM
To: John Baez <baez@math.ucr.edu>
Cc: Todd Trimble <topological.musings@gmail.com>

i wrote:

"i'm cc-ing this to todd in part for the purpose of having him serve as a sort of "conscience" who can more easily recognize the many lapses in rigor here, and who might also be able to come up with possible remedies for such lapses."

so, the stratagem of cc-ing todd here did succeed in getting todd to catch various mistakes here. so at some point i should attempt to say something about these mistakes and how to overcome them .... i'm hopeful that it doesn't cause huge problems ....

....
On Tue, Mar 22, 2022 at 9:26 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Mar 28, 2022 at 9:04 AM
To: John Baez <baez@math.ucr.edu>

so i have a question now about holomorphic gerbes ....

(i don't have a corresponding question about holomorphic n-gerbes yet; this is just about holomorphic gerbes so far.)

so is there some nice way of construing a holomorphic gerbe over a complex projective variety X as being some sort of nice (probably locally-presentable ....) abelian category which is something like an "invertible 2-module" of the 2-rig of quasicoherent sheaves over X?

or something like that? not sure how close this is to being on the right track ....

....
On Fri, Mar 25, 2022 at 2:47 AM John Baez <john.baez@ucr.edu> wrote:
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Mar 28, 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 -

On Mon, Mar 28, 2022 at 9:04 AM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:

> so i have a question now about holomorphic gerbes ....
>
> (i don't have a corresponding question about holomorphic n-gerbes yet; this is just about holomorphic gerbes so far.)
>
> so is there some nice way of construing a holomorphic gerbe over a complex projective variety X as being some sort of nice (probably locally-presentable ....) abelian category which is something like an "invertible 2-module" of the 2-rig of quasicoherent sheaves over X?
>
> or something like that? not sure how close this is to being on the right track ....

Here's a lower-brow fact that may give you what you want: you can take tensor products of holomorphic gerbes, and for any holomorphic gerbe G there's an isomorphism

I tensor G → G

where I is the trivial gerbe.

Let's think about this one level down: for any holomorphic line bundle L there's an isomorphism

I tensor L → L

where L is the trivial line bundle. This winds up implying that you can multiply any section of L by a holomorphic function and get a new section. Or better: the sheaf of sections of L is acted on by the sheaf of holomorphic functions.

I bet we can categorify this idea and get something like what you want, maybe after a bit of "cocompletion" or something.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Mar 29, 2022 at 4:22 AM
To: baez@math.ucr.edu

you:

"Here's a lower-brow fact that may give you what you want: you can take tensor products of holomorphic gerbes, and for any holomorphic gerbe G there's an isomorphism

I tensor G → G

where I is the trivial gerbe.

Let's think about this one level down: for any holomorphic line bundle L there's an isomorphism

I tensor L → L

where L is the trivial line bundle.   This winds up implying that you can multiply any section of L by a holomorphic function and get a new section. Or better: the sheaf of sections of L is acted on by the sheaf of holomorphic functions.

I bet we can categorify this idea and get something like what you want, maybe after a bit of "cocompletion" or something."

all right, let me think about this a bit ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Mar 29, 2022 at 4:58 AM
To: John Baez <baez@math.ucr.edu>

me: "all right, let me think about this a bit ...."

i guess so far i'm confused about some of the de/categorification analogies here because the inverse of a holomorphic line bundle is holomorphic whereas the inverse of a holomorphic function isn't usually holomorphic ....

i think there's other things confusing me here too but they seem harder to articulate ....

i might be tempted to think of ample line bundles as a categorification of holomorphic functions except the tensor unit is rarely ample whereas the multiplicative unit is always holomorphic .... not sure what to make of that, though here are two (probably very incompatible) silly guesses:

1 think of _sectionless_ bundles as a categorification of holomorphic functions ??

2 adjust our intuitions about what's "usual"/"rare" in a de/categorified way ??

just thinking outloud and throwing out silly guesses ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Mar 29, 2022 at 6:22 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 original Appell-Hulmber theorem describes the Neron-Severi group as consisting of alternating bilinear forms A: L x L -> Z, where L is the lattice associated to our complex torus, that are "compatible with the complex structure" in the sense that

> A(ix,iy) = A(x,y).
>
> So, I guessed the 2-Neron Severi group consists of alternating triilinear forms A: L x L x L -> Z such that
>
> A(ix,iy,iz) = A(x,y,z)
>
> But this paper says the right equation is
>
> A(ix,iy,iz) = A(ix,y,z) + A(x,iy,z) + A(x,y,iz)
>
> I don't know what that means.

Now I do. Ben-Bassat says this equation characterizes those antisymmetric trilinear forms that correspond to elements of H^{1,2} + H^{2,1} for our complex torus. So it's a kind of "trick".

This makes me feel sure that back in the original line bundle case, the equation A(ix,iy) = A(x,y) characterizes the antisymmetric bilinear forms that correspond to elements of H^{1,1}.

Just for fun I want to understand rather explicitly which antisymmetric n-linear forms correspond to H^{p,q} of a complex torus. This is sort of a "Hodge structures meet exterior algebra" question, since the cohomology of a torus is an exterior algebra. So, it should have a pretty answer.

Best,
jb
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Thu, Mar 31, 2022 at 6:49 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

I now know more about how to classify n-gerbes on an abelian variety: namely, what the n-gerbe version of the Neron-Severi group is. I can talk about that a bit on Monday; it shouldn't take too long, especially if you just want to know the answer, not how to get it.

Best,
jb
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Apr 1, 2022 at 2:14 PM
To: John Baez <baez@math.ucr.edu>

you: "I now know more about how to classify n-gerbes on an abelian variety: namely, what the n-gerbe version of the Neron-Severi group is. I can talk about that a bit on Monday"

sure, that sounds interesting .... although i probably have a feeling that we already have some answers to that question, some of them already maybe fairly explicit ... in principle ....

for my part of the exposition i'm tentatively planning to try lots and lots of examples of the belief method .... also trying to explain the ideas, but mainly through examples .... and trying to make the examples connected to other ideas that we're working on (and/or to other ideas that we _should_ be working on) ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Apr 1, 2022 at 2:19 PM

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

a vague question:

in the case of the neron-severi group of a complex torus, we have some intuition about what it's like "generically" and about what kind of "specialness" of the complex torus tends to make its neron-severi group nongeneric. is there anything analogous to that that you can say for these "higher neron-severi" groups?

....
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John Baez <john.baez@ucr.edu> Fri, Apr 1, 2022 at 3:01 PM
Reply-To: baez@math.ucr.edu
To: JAMES DOLAN <james.dolan1@students.mq.edu.au>
Cc: John Baez <baez@math.ucr.edu>

Hi -

> in the case of the neron-severi group of a complex torus, we have some intuition about what it's like "generically" and about what kind of "specialness" of the complex torus tends to make its neron-severi group nongeneric. is there anything analogous to that that you can say for these "higher neron-severi" groups?

I don't really know, but my feeling is that it'll be rather similar.   For "specialness" I know we want the stars to align so that integral cohomology classes live neatly in certain sums of Hodge cohomology groups H^{p,q}. I'm planning to explain that stuff in more detail, so I'd like to keep you in some suspense about that these "certain sums" are.   I don't really know what makes the stars align, so I'll naively guess that having lots of symmetry helps.

I really do want to fully nail down the examples we were talking about, of abelian surfaces with big Neron-Severi groups. And I want to proceed and study the gerby analogues.

Ben-Bassat studies a few concrete examples of gerby Neron-Severi groups for abelian varieties, so let me look at those!   Okay, he shows that just as every elliptic curve has Z as its Neron-Severi group, every abelian surface has Z^4 as its gerby Neron-Severi group.

Z^4 is the integral H^3 of any abelian surface, just like Z is the integral H^2 of any elliptic curve.

So, basically nothing can go wrong in this kind of example - the stars align because there's not enough room for the stars to misalign.   The proof is really easy using the stuff I told you last time; I'd prefer to explain it out loud than here, if you're interested.

The "fun" starts for gerby Neron-Severi groups on 3d abelian varieties, and Ben-Bassat looks at these a bit too.

Best,
jb
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