John Baez <johnb@ucr.edu>

Hodge conjecture

17 messages

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

Thu, Mar 24, 2022 at 10:38 PM

HI -

> you:
>
> "If you have a shortage of things to say on Monday I can explain some ideas connected to line bundles:
>
> 1. The Hodge conjecture. ...."
>
> i have an ultra-naive question for you about the hodge conjecture; the sort of question that's almost impossible to ask without immediately seeing why the idea is wrong, but i'll try to ask it anyway ....
>
> the question is something like this: why doesn't even just the little bit that we've learned so far about the neron-severi groups of abelian varieties already disprove the hodge conjecture?

That took me aback for a bit, but I think the answer is just this: if X is a smooth complex projective variety, every integral element of H^{1,1}(X) comes from a codimension-1 subvariety, aka "divisor" - but not all of them come from holomorphic line bundles.   Those that do form the Neron-Severi group.

In the case of curves, every divisor corresponds to a line bundle, but this fails in higher dimensions.

Here I was using some stuff you probably know, but just in case: H^{1,1}(X) is a Doulbeault cohomology group, a sub-vector-space of the cohomology group X with complex coefficients, H^2(X,C). There's a map from H^2(X,Z) to H^2(X,C).   Guys in H^{1,1}(X) in the image of that are what I'm calling "integral".   Also: by Poincare duality and some other stuff, codimension-1 subvarieties, which are really homology classes, give integral elements of the cohomology H^{k,k}(X).

The original Hodge conjecture was that every integral element of H^{k,k}(X) comes from a codimension-k subvariety. I just said this is true for k = 1. But this was disproved for higher k in 1961 by Atiyah and Hirzebruch.   So the modern version is a fallback position: every *rational* element of H^{k,k}(X) comes from a rational linear combination of codimension-1 subvarieties.

The fact that this is a fallback position makes me suspicious somehow, or maybe disappointed. I probably just don't understand this stuff enough, but the original one seemed beautiful and the modern one seems like a way to avoid admitting defeat.

Best,
jb

> well, that's not a very precise question yet, and presumably if i actually try to make it precise then the question will vanish ....
>
> but what's bugging me is, vaguely, something about divisors on abelian surfaces and what the hodge conjecture might say about them .... or something like that ....
>
> ....
> On Fri, Mar 18, 2022 at 3:02 PM John Baez <john.baez@ucr.edu> wrote:

> Hi -
>
> If you have a shortage of things to say on Monday I can explain some ideas connected to line bundles:
>
> 1. The Hodge conjecture.
> 2. The Poincare bundle of a complex torus.
> 3. The analytic and rational representations of a map between complex tori.
> 4. Maybe more about the Rosati involution if I make some progress.
>
> What I have to say about 1-3 is a lot less intimidating than the words make it sound.
>
> Best,
> jb
>
> On Thu, Feb 24, 2022 at 2:55 PM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:
>
>> ok, thanks!
>>
>> ....
>>
>> On Thu, Feb 24, 2022 at 5:43 PM John Baez <john.baez@ucr.edu> wrote:
>>
>>> Hi -
>>>
>>> Here's a page about our conversations:
>>>
>>> https://math.ucr.edu/home/baez/conversations/
>>>
>>> I may not have the energy to add so many links all the time, but at least it's really easy for me to add email threads or videos to this page.
>>>
>>> Best,
>>> jb
>>>
>>> On Wed, Feb 23, 2022 at 7:57 PM John Baez <john.baez@ucr.edu> wrote:
>>>
>>>> On Wed, Feb 23, 2022 at 1:17 PM JAMES DOLAN <james.dolan1@students.mq.edu.au> wrote:
>>>>
>>>>> offhand making the emails publicly accessible sounds ok .... it would save me the trouble of having to occasionally think about whether i should feel free to forward some part of one of your emails to me to someone else ....
>>>>>
>>>>> i think i feel confident enough that you'll be slow enough in handling the logistical details of the task that i can say: go ahead and do it, to the extent that you have the logistical energy to accomplish it; that'll still give me enough time to tell you to cancel (or modify) the project in case i change my mind.
>>>>
>>>> At first I thought it was going to be a real pain in the butt to do this and have it look halfway decent, but then I realized I could just get Google to print a whole discussion thread to a PDF. So that's what I'll do, and let's try to mainly keep boring or scurrilous discussions in separate threads like this one.
>>>>
>>>> Best,
>>>> jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 25, 2022 at 1:26 AM
To: John Baez <baez@math.ucr.edu>

you:

"That took me aback for a bit, but I think the answer is just this: if X is a smooth complex projective variety, every integral element of H^{1,1}(X) comes from a codimension-1 subvariety, aka "divisor" - but not all of them come from holomorphic line bundles. Those that do form the Neron-Severi group.

In the case of curves, every divisor corresponds to a line bundle, but this fails in higher dimensions."

this doesn't sound right to me yet, but what do i know?

(in addition to being confused about concepts here i'm also worried about being confused about terminology ....)

i'm still thinking that "every divisor corresponds to a line bundle" persists into higher dimensions ....

but i suspect that what i'm really confused about here is the whole idea of "hodge structures" and how they involve orientations between filtrations and lattices and "real forms of complex vector spaces" (where the lattices can also be thought of as "integer forms of real vector spaces"), and how this relates to the web of relationships between ordinary cohomology, real and complex de rham cohomology, "dolbeault cohomology", and so forth. when we're focusing on just abelian varieties and picard groups and picard schemes we can get away to a great extent with soft-pedaling the abstract hodge-structure ideas but i suspect it's going to be important to bring those ideas out more clearly ....

also, i think i've been casually thinking a lot of the time that "there's probably enough duality floating around that it's not going to hurt if i accidentally look at the hodge diamond upside down", but as we're getting into more sophisticated (or at least less naive) stuff like neron-severi groups and possible higher analogs of them and so forth, maybe i need to be more careful about that .... reminding myself that there probably really is a significant distinction between dimension j and co-dimension j, so for example when j=1 between curves and divisors (as soon as you get beyond surfaces) ....

on the general theme of "cohomological degree-shifts that emerge when you add holomorphic structure to line bundles, gerbes, etc", maybe it's good to remember that "cohomological degree-shifts" aren't very exotic; we bump into them as soon as we start learning about cohomology operations. also, i think we shouldn't be too scared if we eventually have to think about spectral sequences or their conceptual equivalent, as they're of a similar flavor ....

i still need to think more about what "sheaf cohomology" (and/or "czech cohomology") is really about, conceptually ....

just rambling on a bit here, trying to convince ourselves that if things start to get scary it'll probably be more in a fun way than in an unpleasant way .... seems like an appropriate attitude for the modern world ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 25, 2022 at 1:33 AM
To: John Baez <baez@math.ucr.edu>

me: "i'm still thinking that "every divisor corresponds to a line bundle" persists into higher dimensions ...."

this is perhaps especially because my personal definition of "divisor" is "holomorphic structure on the unit 'meromorphic line bundle'". that's probably closer to "cartier divisor" than to "weil divisor" but they should all agree in vanilla cases.

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 25, 2022 at 1:41 AM
To: John Baez <baez@math.ucr.edu>

you: "So the modern version is a fallback position: every *rational* element of H^{k,k}(X) comes from a rational linear combination of codimension-1 subvarieties."

this vaguely reminds me of something i learned about complex cobordism during a brief period a year or two ago when i actually understood something about complex cobordism: the lazard generators that freely generate complex cobordism are almost but not quite just the complex projective spaces; they're off by an integer multiple or something like that, so that you _can_ use them as the generators after you tensor with the rationals.

....
On Fri, Mar 25, 2022 at 1:39 AM John Baez <john.baez@ucr.edu> wrote:
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Fri, Mar 25, 2022 at 6:37 AM
To: John Baez <baez@math.ucr.edu>

you:

"The original Hodge conjecture was that every integral element of $H^{k,k}(X)$ comes from a codimension-k subvariety. I just said this is true for k = 1. But this was disproved for higher k in 1961 by Atiyah and Hirzebruch."

i'd really like to see (and/or perhaps "see") some explicit counterexamples here. not that i'd necessarily be able to appreciate them at first, but i'd like to try.

....
On Fri, Mar 25, 2022 at 1:39 AM John Baez <john.baez@ucr.edu> wrote:
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Fri, Mar 25, 2022 at 4:11 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 curves, every divisor corresponds to a line bundle, but this fails in higher dimensions."
>
> this doesn't sound right to me yet, but what do i know?
>
> (in addition to being confused about concepts here i'm also worried about being confused about terminology ....)
>
> i'm still thinking that "every divisor corresponds to a line bundle" persists into higher dimensions ....

If you find any evidence of this let me know! As counterevidence, the Wikipedia page on divisors never says that. And more tellingly, the Neron-Severi group is the subgroup of the integral part of $H^{1,1}$ that *does* come from hololorphic line bundles, and it's a proper subgroup in general... while the Wikipedia page on the Hodge conjecture says every integral element of $H^{1,1}$ comes from a divisor.

Best,
jb
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Mar 27, 2022 at 9:52 AM
To: John Baez <baez@math.ucr.edu>

you:

"If you find any evidence of this let me know! As counterevidence, the Wikipedia page on divisors never says that. And more tellingly, the Neron-Severi group is the subgroup of the integral part of $H^{1,1}$ that *does* come from hololorphic line bundles, and it's a proper subgroup in general... while the Wikipedia page on the Hodge conjecture says every integral element of $H^{1,1}$ comes from a divisor."

so before i try to describe what i suspect's going on here, let me unnecessarily remind us not to assume i actually know what i'm talking about. (it's not so much that i learned math on the street as that i learned it on a deserted street trying to make sense of the graffiti left behind ....) anyway here goes:

for a nonsingular complex projective variety X, let's temporarily define s := the set of integral elements of $H^{1,1}$

then it seems like we agree that there's a surjective map divisors -f->> s, and we agree that there's an injective map neron-severi >-g-> s. however we're in conflict because i think that f lifts along g (thus forcing g to be bijective), whereas you think that g isn't bijective.

thus you're asking me "why does f always have to lift along g?" while i'm asking you "why can't g always be bijective?".

so here's my preliminary attempt to say why f always has to lift along g: roughly speaking it's because cartier divisors manifestly give invertible sheaves, and over a nonsingular complex projective variety, "cartier divisor" is equivalent to pretty much any other kind of divisor (for example "weil divisor"), while "invertible sheaf" is equivalent to "holomorphic line bundle". thus a cartier divisor is (i think) essentially an invertible subsheaf of the quasicoherent sheaf of "rational" functions.

another way to paraphrase this is to say that a (cartier) divisor is a "holomorphic form of the unit meromorphic line bundle" or a "regular form of the unit rational line bundle". i also sometimes use the description "holomorphic structure on the unit meromorphic line bundle". here "meromorphic vector bundle" is just a suggestive alternative name for "vector space over the field of meromorphic functions".

i was then about to speculate that maybe the wikipedia article on divisors declines to assert that divisors always give line bundles because of agonizing over the distinction between cartier divisors and weil divisors , or something like that; and a brief look at the article seems to support that, more or less.

(weil divisors sound kind of dumb to me because they apparently don't generally give line bundles. i tend to excuse weil for doing "dumb" stuff like that on account of having been a pioneer who sometimes had to work with crude tools; for example https://en.wikipedia.org/wiki/Foundations_of_Algebraic_Geometry claims that one reason they invented "abstract varieties" was because jacobians of curves hadn't yet been proved to be projective varieties in general. but anyway, when we try to understand "motives" and "adequate equivalence relations on divisors" and stuff like that we might very well have to think about the contrast between weil divisors and cartier divisors ....)

but let me turn now to the question that i'm asking you, "why can't g always be bijective?"; but let me make it more insistent: "why _can't_ g always be bijective, pretty-please??". that is, i'm beginning to hope that it'd be very nice if the neron-severi group really always is just the integral elements in H^{1,1}, once we properly understand what "integral" means in this context. it seems like i'm suggesting that the whole statement of for example the appell-humbert theorem can be wrapped up into a neat package saying something like "the rank 2 hodge structure of a complex abelian variety is in some straightforward sense essentially just the exterior square of its rank 1 hodge structure", and that (along the lines of the pattern you suggested for n-gerbes for arbitrary n following that ben-bassat paper) it keeps going like that for the higher exterior powers ....

well, i'm not stating things very clearly yet but i hope you can get some sense of what i'm trying to say here ....

i also still have the vague feeling that it'd be nice of the relationship between co-dimension 1 subvarieties and holomorphic line bundles generalizes to some similarly nice relationship between co-dimension j subvarieties and .... _some_thing .... ?? maybe holomorphic j-gerbes, or holomorphic j-dimensional vector bundles, or some mixture of both, or something .... well, now i'm just throwing out random wild guesses ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Sun, Mar 27, 2022 at 4:02 PM
To: John Baez <baez@math.ucr.edu>

hmm, so our discussion inspired me to search on "hodge conjecture for abelian varieties", which seems to give interesting results ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Mar 28, 2022 at 7:09 AM

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

Hi -

You're beginning to win me over on that business about "does every integral element of H^{1,1} come from a line bundle?"   I'm gonna ask some people.   How come nobody comes out and just says the Neron-Severi group is the integral part of H^{1,1}?   But now that I look carefully, Wikipedia comes close, actually, at the start of their article on the Neron-Severi group:

> In algebraic geometry, the **Néron–Severi group** of a variety is the group of divisors modulo algebraic equivalence; in other words it is the group of components of the Picard scheme of a variety.

So this seems to be asserting the existence of that lift you were talking about, from divisors to the Neron-Severi group.   It's doing it by defining the latter as the quotient of the former by some equivalence relation.

If you click on "algebraic equivalence" you'll see there's a whole axiomatic framework for studying nice equivalence relations on divisors, but you'll get a very terse description of algebraic equivalence.

So maybe I was just underestimating or forgetting how the integral elements of H^2 can intersect H^{1,1} very awkwardly, i.e. not forming a full lattice in H^{1,1}. This helps me understand what's the big deal about Hodge structures, which at first sound like rather bland entities.

I agree it would be really cool to understand how "integral elements of H^{1,1}" boils down to the exact same thing as "integral elements of H^2 fixed by the Rosati involution".   Maybe the Rosati involution switches H^{2,0} and H^{0,2}. Maybe for a bunch of varieties there's a Rosati-like involution that flips the Hodge diamond in this way - "horizontally", unlike Poincare duality.

Best,
jb

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JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Mar 28, 2022 at 8:17 AM
To: John Baez <baez@math.ucr.edu>

you: "So this seems to be asserting the existence of that lift you were talking about, from divisors to the Neron-Severi group.   It's doing it by defining the latter as the quotient of the former by some equivalence relation."

right ....

you: "If you click on "algebraic equivalence" you'll see there's a whole axiomatic framework for studying nice equivalence relations on divisors, but you'll get a very terse description of algebraic equivalence."

right, those are those "adequate equivalence relations" i mentioned, allegedly giving rise to different kinds of "motives". it sounds very interesting but i'm not sure what to make of it yet!

you: "So maybe I was just underestimating or forgetting how the integral elements of H^2 can intersect H^{1,1} very awkwardly, i.e. not forming a full lattice in H^{1,1}."

right, this sounds a lot like that experience i went through where i erroneously thought at first that the neron-severi rank of an abelian surface would always be 4, before realizing that a more awkward "out-of-focus" intersection would be generic.

you: "This helps me understand what's the big deal about Hodge structures, which at first sound like rather bland entities."

yes!

you: "I agree it would be really cool to understand how "integral elements of H^{1,1}" boils down to the exact same thing as "integral elements of H^2 fixed by the Rosati involution"."

yes!

you: "Maybe the Rosati involution switches H^{2,0} and H^{0,2}. Maybe for a bunch of varieties there's a Rosati-like involution that flips the Hodge diamond in this way - "horizontally", unlike Poincare duality."

i need to think more about this .... lots of possibilities here ....

....
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JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Mar 28, 2022 at 8:30 AM
To: John Baez <baez@math.ucr.edu>

i guess i'm hoping that that "descent from the universal cover" idea applying to holomorphic line bundles over abelian varieties generalizes nicely to holomorphic n-gerbes, and that this will help clarify how "integral" elements in "abstract dolbeault cohomology groups of abstract hodge structures" relate to the classification of holomorphic n-gerbes .... or something like that ....

but is there a name for "the group of integral elements in dolbeault^{j,k}(X)" in some generality? it seems like there ought to be a standard name for that if i'm on the right track .... i could easily be over-generalizing or mis-generalizing or mis-understanding though ....

i also noticed that hitchin says something about "_unitary_ gerbes", so i need to try to understand what that's about ....

....
[Quoted text hidden]

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Mar 28, 2022 at 9:11 AM
To: John Baez <baez@math.ucr.edu>

me: "hmm, so our discussion inspired me to search on "hodge conjecture for abelian varieties", which seems to give interesting results ...."

my original guess was something like that the "hodge conjecture for abelian varieties" would turn out to be an easy special case of the hodge conjecture, so i was surprised when the search results seemed to suggest more or less the opposite; that is that the "hodge conjecture for abelian varieties" is expected to be about as difficult as the hodge conjecture. if i didn't misread what they said too badly ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Mon, Mar 28, 2022 at 10:31 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 guess i'm hoping that that "descent from the universal cover" idea applying to holomorphic line bundles over

> abelian varieties generalizes nicely to holomorphic n-gerbes, and that this will help clarify how "integral" elements in "abstract dolbeault cohomology groups of abstract hodge structures" relate to the classification of holomorphic n-gerbes .... or something like that ....

Having read a paper on the classification of holomorphic gerbes on complex tori, I'm getting pretty close to guessing how this works for n-gerbes. So I could try to tell you about that.

> but is there a name for "the group of integral elements in dolbeault^{j,k}(X)" in some generality? it seems like there ought to be a standard name for that if i'm on the right track .... i could easily be over-generalizing or mis-generalizing or mis-understanding though ....

There should be some name for it, but I don't know it. The concept makes sense for any Hodge structure, so maybe the Hodge structure experts have a name for it.

> i also noticed that hitchin says something about "_unitary_ gerbes", so i need to try to understand what that's about ....

Back when I did higher gauge theory I was mainly interested in U(1) gerbes, where we replace the group C* by U(1). Maybe those are the same thing? The bigshots like to talk about gerbes for any *sheaf* of groups, and they call that the "band" of the gerbe.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Mon, Mar 28, 2022 at 7:06 PM
To: John Baez <baez@math.ucr.edu>

you: "This helps me understand what's the big deal about Hodge structures, which at first sound like rather bland entities."

even if we haven't mentioned it outloud very recently one of the important themes that seems to be lurking here is that idea about thinking of hodge structures (and/or some aspect of them) as representations of a certain group ....

....

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JAMES DOLAN <james.dolan1@students.mq.edu.au> Tue, Mar 29, 2022 at 2:07 PM
To: John Baez <baez@math.ucr.edu>

me:

"my original guess was something like that the "hodge conjecture for abelian varieties" would turn out to be an easy special case of the hodge conjecture, so i was surprised when the search results seemed to suggest more or less the opposite; that is that the "hodge conjecture for abelian varieties" is expected to be about as difficult as the hodge conjecture. if i didn't misread what they said too badly ...."

here's a vague guess as to what might be going on here:

presumably there's a class of complex projective varieties where "everything" is concentrated in the middle column of the hodge diamond, and more or less as a result of that the hodge conjecture is easy for that special class of varieties; if i worked hard at i might even be able to remember some standard name for that class of varieties.

and maybe abelian varieties are regarded as somewhat opposite in flavor to that, spread out over the full width of the hodge diamond instead of being concentrated in the middle column; and in some simpleminded way maybe this is related to alleged claims that "the hodge conjecture for abelian varieties is about as difficult as the general case of the hodge conjecture".

or something like that ....

....
[Quoted text hidden]

John Baez <john.baez@ucr.edu> Tue, Mar 29, 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 -

> presumably there's a class of complex projective varieties where "everything" is concentrated in the middle column of the hodge diamond, and more or less as a result of that the hodge conjecture is easy for that special class of varieties; if i worked hard at i might even be able to remember some standard name for that class of varieties.

I'd like to know that name!

> and maybe abelian varieties are regarded as somewhat opposite in flavor to that, spread out over the full width of the hodge diamond instead of being concentrated in the middle column; and in some simpleminded way maybe this is related to alleged claims that "the hodge conjecture for abelian varieties is about as difficult as the general case of the hodge conjecture".
>
> or something like that ....

A while ago I bumped into a paper by Milne that made some partial progress:

> In two earlier articles, we proved that, if the Hodge conjecture is true for ALL CM abelian varieties over the complex numbers, then both the Tate conjecture and the standard conjectures are true for abelian varieties over finite fields. Here we rework the proofs so that they apply to a single abelian variety. As a consequence, we prove (unconditionally) that the Tate and standard conjectures are true for many abelian varieties over finite fields, including abelian varieties for which the algebra of Tate classes is not generated by divisor classes.

But I didn't look at it, because it sounded above my pay grade.

Best,
jb

JAMES DOLAN <james.dolan1@students.mq.edu.au> Sat, Apr 2, 2022 at 6:25 AM
To: John Baez <baez@math.ucr.edu>

me:

my original guess was something like that the "hodge conjecture for abelian varieties" would turn out to be an easy special case of the hodge conjecture, so i was surprised when the search results seemed to suggest more or less the opposite; that is that the "hodge conjecture for abelian varieties" is expected to be about as difficult as the hodge conjecture. if i didn't misread what they said too badly ....

hmm, maybe it's not so much that i misread it as that i just didn't read quite far enough; if i'd read just a bit further i'd have seen that milne themself entered the discussion and said (in 2010):

"In fact, abelian varieties should be an "easy" case. For example, it is known that for abelian varieties (but not other varieties), the variational Hodge conjecture implies the Hodge conjecture. It is disconcerting that we can't prove the Hodge conjecture even for abelian varieties, even for abelian varieties of CM-type, and we can't even prove that the Hodge classes Weil described are algebraic. So if the Hodge conjecture was proved in one interesting case, e.g., abelian varieties, that would be a big boost.

Added: As follow up to Matt Emerton's answer, a proof that the Hodge conjecture for abelian varieties implies the Hodge conjecture for all varieties would (surely) also show that Deligne's theorem (that Hodge classes on abelian varieties are absolutely Hodge) implies the same statement for all varieties. But no such result is known (and would be extremely interesting)."

....
[Quoted text hidden]