From: **Chris Elliott** celliott@ihes.fr
Subject: Re: Twisted gauge theory paper
Date: February 21, 2019 at 11:46 AM
To: Brian Williams brianwilliams.math@gmail.com, Pavel Safronov psafronov@gmail.com

CE

Hi Pavel and Brian,

First of all, for meeting, how about 9am PST on Wednesday, which is 6pm in Europe?

Thanks for your detailed response Pavel, maybe I can try to explain what I was thinking again, sorry for not being very clear. We would like to say, given a super Yang-Mills theory and a Lorentz x R-symmetry orbit Q in the space of square zero supercharges, there is a unique thing called the Q-twisted theory. The obstacle we're running into is that the supertranslation action typically isn't defined off-shell without introducing auxiliary fields, and for different Q's in the same orbit we might need to choose different choices for the auxiliary fields.

So I guess what you're talking about in your e-mail is trying to quantify how bad this problem is, is that right? So let me think about that. As you point out, in dimension 9 and below, in the maximally SUSY case, we can always use Berkovits's auxiliary fields, so we have a uniform choice for all such twists. You asked whether the SU(4) action was the same whether you come via Spin(7) or SU(5) -- but aren't the embeddings in SO(10) isomorphic? The embedding in SO(10) is a choice of 10d rep of SU(4), and I think in both cases we're defining the rep V (x)_R C + C^2 where V is the 4d complex fundamental rep of SU(4)?

So let's think about dimension 10, this is what I was thinking about. There's only one orbit of square zero supercharges. We expect the twist by Q to look like holomorphic Chern-Simons in whatever setup we choose. There's a Lie map from L_Bau --> L_Ber, if it's Q-equivariant it should define a map of twisted theories L_Bau^Q --> L_Ber^Q. We know L_Bau^Q = the BV complex for holomorphic Chern-Simons. For a fixed choice of Q (one of the 9 after restricting to Spin(7) x SO(2)), my guess was that Q would mix L_min and one of the auxiliary fields, and not the other 6, so that the twist L_Ber^Q looked like holomorphic Chern-Simons + something boring, and the map from L_Bau^Q was an equivalence onto the first factor.

The upshot would be that, up to some trivial factors, there was a unique thing we could identify as *the* twist of 10d SYM, i.e. no ambiguity where the twist depended on which auxiliary fields we chose. Then we could identify the dimensional reductions using our calculation that gave holomorphic Chern-Simons theory. Per the comment on the two embeddings of SU(4) I think being the same, I think the twisted theories by reducing either 10d theory are not only the same as theories, but as G-modules for whichever G action we're choosing.

By the way we had some trivial factor corresponding to the extra auxiliary fields G_2, .., G_7 along for the ride. I expect we'll need to keep those to have the further twist of 8d realised off-shell, and to see the Spin(7) action there.

Let me think about your comment (*) too -- I'm confused about the point you're making. So the concern is that if you take an 8d square zero supercharge it might not be in the 8+1 of Spin(7). I think 8+7+1 splits up as 6+4+4+1+1 of SU(4), and so the square-zero rank (1,0) supercharges should be linear combinations of those two trivial factors, with only the topological one being Spin(7) invariant, i.e. lying in just that 1 for Spin(7) summand. The holomorphic one won't ever be in the 8+1 of Spin(7). So what does that mean? Does that mean that the Berkovits theory doesn't ever give the off-shell action of a holomorphic supercharge on the 8d theory? But how can that be: choose any Q acting on 10d Berkovits theory off-shell and reduce to 8d: you get *some* 8d SYM theory with auxiliary fields and an action of a supergroup Pi C, and so you can twist. Surely that twist should be holomorphic? I'm confused here.

All the best,
Chris

On 21/02/2019 17:38, Brian Williams wrote:

> Hi Chris and Pavel,
>
> Will take a closer look at this over the weekend. I am free in the morning EST on Wednesday. Let's plan to talk then!
>
> Brian
>
>> On Feb 21, 2019, at 6:47 AM, Pavel Safronov <psafronov@gmail.com> wrote:
>>
>> In equation (2) on the LHS he just means that his supersymmetries depend on epsilon and v_i (epsilon, v_1, …, v_7 are spinors). I guess the RHS should be clear.
>>
>> I don't know what to say about the action of \tilde{S} since it's nonlinear. You can instead say that you have some Lie superalgebra whose odd part is S_+ \otimes O (S_+ are 10d spinors and O are the octonions) and whose even part is V + something. This Lie superalgebra has a projection to the ordinary supertranslation algebra where on the odd part you extract the real part of the octonion. You have an action of that Lie superalgebra on the theory, but it is on-shell only. Given an odd element Q in that Lie superalgebra, you can ask that its square is a translation (i.e. the "something" part is zero); I believe that's the condition that the parameters lie on a quadric. Bekovits then shows that the commutator of Q with a Lorentz transformation applied to Q is also a pure translation. He asserts that the SO(10)-orbit of any such Q is at most nine-dimensional.
>>
>>> On Feb 21, 2019, at 12:44 PM, Chris Elliott <celliott@ihes.fr> wrote:
>>>
>>> My thought here is that I want to use the calculations we've already done. If we check this compatibility then, unless I'm

> > > My thought here is that I want to use the calculations we've already done. If we check this compatibility then, unless I'm misunderstanding, we know the appropriate subgroup of SO(n) acts on all the dimensional reductions of the twisted 10d theory that we already calculated.
>
> > Can you clarify what you mean here?
> >
> > > Finally, do we think we should stop worrying too much about the pure spinor formalism, other than an aside in the paper? It does seem like a potentially nice story if we could show that the twist in that language really was "tautologically" holomorphic Chern-Simons, but that only seems valuable for our purposes aesthetically rather than practically given this other point of view.
> >
> > Indeed, we just needed some formulation of 10d SYM which had the necessary supersymmetry action off-shell. Pure spinors seemed like a natural choice, but since it's totally unclear how to do the computation of the twist there (we need to compute that crazy zero-mode cohomology), it seemed to me like a dead end. So, I was looking at some other formulations of 10d SYM with an off-shell supersymmetry and Berkovits and Baulieu are the only reasonable candidates.
> >
> > ---
> >
> > We know that one can break SO(10) to some subgroup G such that one can express v in terms of epsilon for a subspace of epsilons. And we just want to ensure that in whatever twist we're computing we have the expected structure group contained inside G and the twist we care about is contained in that subspace. It would be of course nice to have a uniform formulation in all dimensions (what I was hoping to achieve with pure spinors), but that seems out of reach. The next reasonable thing to ask is that we have a single notion of SYM theory in some dimension and then we perform twists of that theory. I'm not 100% sure we can get it (see the comment marked (*) below). The worst-case scenario is that in dimensions 7 and 8 for different twists we would compute twists of different theories (namely, the Baulieu one and the Berkovits one). Maybe it's not too bad.
> >
> > In https://arxiv.org/abs/0705.2002 it is shown that one can take G = Spin(7) x SO(2) so that S_+ splits as 1 + 7 + 8. And the claim is that for epsilon in 1 + 8 one can express v in terms of epsilon. So, in any maximally supersymmetric Yang—Mills you can have 9 supersymmetries realized off-shell. This helps with any twist obtained by compactifying the 8d topological twist.
> >
> > I strongly suspect the following to be true. Let's take G = SU(5) in SO(10). Then S_+ splits as 1 + 10 + 5 (1 is the trivial rep, 5 is the defining rep or possibly its dual). I expect that you can express v in terms of epsilon if epsilon is in 1 + 5; so, there should be 6 supersymmetries realized off-shell. This should help with any twist obtained by compactifying the 10d holomorphic twist. Even if this doesn't work, we can use Baulieu's auxiliary field to take care of the holomorphic twist in 10d and hence of its compactifications.
> >
> > In some dimensions it might happen that a given supercharge lies in both 1 + 8 with respect to Spin(7) x SO(2) and 1 + 5 with respect to SU(5). If you look at the chart of twists, this definitely happens for the rank (2, 2) topological twist of 4d N=4 SYM, but it might happen even in higher dimensions. So, it would be nice to know how the two theories are related.
> >
> > One possible connection might be the following. There's an inclusion SU(4) < SU(5) < SO(10) and an inclusion SU(4) < Spin(7) < SO(8) < SO(10). Are these the same SU(4)? Then we can ask that some subspaces of supercharges act in the same way on the theory (note that the theory for different G is the same, it just has a different action of supersymmetry). Comment (*): note that the holomorphic twist in 8d is SU(4)-structured. But it might not lie in 1 + 8 with respect to some Spin(7) containing that SU(4).
> >
> > The not-so-bad scenario would be that we always twist Berkovits's theory in different dimensions, but the supersymmetry action is different (even the number of supersymmetries that act is different) depending on whether we choose G = SU(5) or G = Spin(7) x SO(2).
> >
> > ---
> >
> > I think these are all problems for Yang—Mills theories with maximal supersymmetries. My understanding is that e.g. in the case of 8 supercharges you can add 3 auxiliary fields to 6d N=(1, 0) SYM so that you realize all supersymmetries off-shell. In the case of 4 supercharges you can presumably add one auxiliary field to 4d N=1 SYM so that you realize all supersymmetries off-shell. And in 3d N=1 SYM everything is automatically off-shell. And there is no need to break the Lorentz group to have any of those actions.
> >
> > ---
> >
> > I should also mention that even when we have off-shell supersymmetry, it only preserves the action modulo gauge transformations. In other words, if S is the action in the BRST formulation, d is the BRST differential and Q is your supercharge, you have Q S = d S_1. I haven't worked it out, but I think to do the twist we need higher closure data here as well, i.e. we need to find S_2, … such that Q S_1 = d S_2, Q S_2 = d S_3, … But we can worry about it when we do the computation.
> >
> > > > On 21/02/2019 09:58, Pavel Safronov wrote:
> > > > Yes, I should be free all afternoon on Wednesday. Let's see what Brian's plans are.
> > > >
> > > > > On Feb 21, 2019, at 10:49 AM, Chris Elliott <chris.elliott@cantab.net> wrote:
> > > > >
> > > > > Next Wednesday would work, if you're free then?
> > > > >
> > > > > > On 21/02/2019 09:19, Pavel Safronov wrote:
> > > > > > Unfortunately, I'll be away tomorrow. Would next week work for you as well? Maybe it's going to be easier for Brian as well since he's at a conference now.
> > > > > >
> > > > > > > On Feb 20, 2019, at 11:07 PM, Chris Elliott <chris.elliott@cantab.net> wrote:

Hi Pavel and Brian,

Sorry for not being able to reply properly yesterday or today, I was visiting Birmingham and was pretty busy (and am too tired now). I'll try to reply properly tomorrow.

In the mean time, are you still interested in talking on Skype on Friday as we previously suggested? I'm in the UK, and would be free until about 6, I expect. If you're interested is there a time that works?

All the best,

Chris

> On 20/02/2019 18:28, Pavel Safronov wrote:
> I wouldn't say that Berkovits is resolving the SUSY algebra. The rough picture is the following. There is a surjective map \tilde{S} -> S, where S is the space of 10d spinors and \tilde{S} is a certain quadric (does Berkovits know any other equations except for quadratic ones?). Here S parametrizes epsilon and \tilde{S} parametrizes epsilon and v_i. There's a supertranslation Lie algebra, but it doesn't act on 10d SYM (because the SUSY only closes on-shell). What Berkovits shows is that one can add 7 auxiliary fields to 10d SYM (in a really silly way, the action is S_{SYM} + sum (G_i)^2, where G_I are the auxiliary fields), so that given a point in \tilde{S} you do have an off-shell action.
>
> Since \tilde{S} -> S is surjective, it admits a splitting, but there is no Lorentz-invariant section. What some people show is that you can break SO(10) to Spin(7) x SO(2) which gives you a 9d subspace S' in S. Then there is a Spin(7) x SO(2)-invariant section of \tilde{S} -> S over S'. To conclude, Berkovits adds 7 auxiliary fields in a Spin(7) x SO(2)-invariant way, so that you still have 9 supersymmetries and everything closes off-shell.
>
> Baulieu instead considers SU(5) in SO(10) which gives a scalar supercharge Q in S. Baulieu shows that you can add a single auxiliary field (it couples to the curvature as opposed to Berkovits's action where the auxiliary fields don't couple to anything) so that Q preserves the action off-shell.
>
> I have also realized that SU(4) sits inside of Spin(7), so in fact if we care about any example except for 10d SYM, we can just use Berkovits's action with auxiliary fields to do the twist.
>
>> On Feb 20, 2019, at 6:42 AM, brian williams <brianwilliams.math@gmail.com> wrote:
>>
>> Sorry for not responding. I've been keeping up with the email exchange, but have not had time to respond due to this conference.
>>
>> I'm still a little confused about Berkovits approach. You are saying he is resolving the ordinary SUSY algebra by some equivalent Lie algebra. However the equivalence between these two algebras is not Lorentz invariant. But the equivalence does preserve certain decompositions of Lorentz.
>>
>> Does that sound right? If we can say something like this, then it might be possible to relate Baulieu's approach to Berkovits' auxiliary fields.
>>
>> I'll have more time to carefully look at these emails and papers next week.
>>
>> b
>>
>>> On Feb 19, 2019, at 9:30 AM, Pavel Safronov <psafronov@gmail.com> wrote:
>>>
>>> Ah, ok, I think I understand it. Berkovits has nonstandard supersymmetries parametrized by a spinor epsilon and 7 spinors v_i. If they satisfy some equations (namely, equation 3 in that paper), then these new supersymmetries close into the usual translations. But solving for v_i's in terms of epsilon is what breaks the Lorentz symmetry.
>>>
>>> Suppose you consider SO(7) x SO(2) Lorentz symmetry in 10d (i.e. you're looking at a product of a 7-manifold, a 2-manifold and a 1-manifold). Then the 10d spinor representation breaks into a sum of two 7d spinor representations. And you can have 8 supersymmetries to close on-shell here. If you further break SO(7) to G_2, you can have 9 supersymmetries to close on-shell. You can also do better and instead break SO(10) to Spin(7) x SO(2) (product of a Spin(7) 8-manifold and a 2-manifold), then you still have nine supercharges which close off-shell (see https://arxiv.org/abs/0705.2002).
>>>
>>> So, this problem with closure of supersymmetries occurs only in SYM with 16 supercharges. When you have 8 supercharges, you instead just add 3 auxiliary fields and apparently there's no need to break Lorentz invariance.
>>>
>>> As I mentioned, the reason we want to have Lorentz invariance is to preserve all possible symmetry after the twist. Here is what the symmetry groups are in high dimensions:
>>>
>>> 1) 10d. Here we care about SU(5) in SO(10). Baulieu adds an auxiliary field to the action so that the action

becomes off-shell supersymmetric with respect to the scalar supercharge.

2) 9d. Here we care about SU(4) in SO(9). Use Baulieu.

3) 8d. We have three twists:
a) pure rank (1,0). Its stabilizer is SU(4) in SO(8). Use Baulieu.
b) impure rank (1,0). Its stabilizer is Spin(7) in SO(8). You can use Berkovits’s auxiliary fields.
c) rank (1, 1). Its stabilizer is a further subgroup of SU(4) (tt’s the intersection of the mirabolic subgroup in SL(4, C) with SO(8, R), not sure exactly what it is, could be SU(3)). Use Baulieu.

4) 7d. We have three twists:
a) holomorphic rank 1. Its stabilizer is SU(3) in SO(7). Use Baulieu.
b) topological rank 1. Its stabilizer is G_2 in SO(7). Use Berkovits.
c) rank 2. Its stabilizer is a subgroup of SU(3). Use Baulieu.

It would be nice how the two formulations (add a single auxiliary field in 10d or add 7 auxiliary fields) relate. I imagine Baulieu’s formulation is a subset of Berkovits’s, but I haven’t found the precise statement.

So, to summarize, I believe we don’t need to understand the pure spinor formalism after all. Instead, we can use the 7 auxiliary fields of Berkovits to take care of all cases with all symmetry groups.

> On Feb 19, 2019, at 3:59 PM, Pavel Safronov <psafronov@gmail.com> wrote:
>
> I was wondering, did you guys understand the paper https://arxiv.org/abs/hep-th/9308128? It claims to add auxiliary fields to 10d SYM such that the theory is still Lorentz-invariant and some (namely, 9) supersymmetries close off-shell. Do you understand how this can happen? The semi-spin representation S_+ is irreducible, so how can it have a distinguished 9-dimensional subspace?
>
>> On Feb 18, 2019, at 8:58 PM, Chris Elliott <chris.elliott@cantab.net> wrote:
>>
>> Ah, ok, thanks, sorry for being dumb. I was hoping there's a nicer resolution than the one in the the appendix of Berkovits' paper but I'll have to think about it some more I guess.
>>
>> Best wishes,
>>
>> Chris
>>
>>> On 18/02/2019 20:47, Pavel Safronov wrote:
>>> The Koszul complex is not a resolution here. You can see it by counting dimensions: if it were, the dimension of the space of pure spinors would be dim(S) - dim(V) = 6 instead of 11. So, there has to be higher cohomology. The Koszul complex is the Chevalley—Eilenberg complex of the supertranslation Lie algebra and its cohomology is computed in a paper of Movshev—Schwarz, I think, where you can see explicitly what the higher cohomology groups are.
>>>
>>>> On Feb 18, 2019, at 5:53 PM, Chris Elliott <chris.elliott@cantab.net> wrote:
>>>>
>>>> Thanks Pavel!
>>>>
>>>> That's really helpful, and I'll give them a closer read.
>>>>
>>>> So I was thinking about this today, and I got confused by a basic thing, maybe one of you can tell me where I'm going wrong.
>>>>
>>>> Let's focus on the zero-mode cohomology (and abelian gauge group). I'm also going to talk about the story *without* twisting. We're supposed to compute the cohomology of O(Pi S x Pi P[-1]) with respect to the zero-mode differential lambda^a d/dtheta^a. Let's resolve the O(P) part: I think we can use a Koszul resolution which looks like O(Pi S[-1] x Pi V) with Koszul differential generated by the map V^* --> Sym^2(S^*) sending v^i to lambda_a gamma^i lambda_a.
>>>>
>>>> So the cohomology we're computing looks like
>>>>
>>>> H^*(H^*(O(Pi S x Pi S[-1] x Pi V), d_Koszul), lambda^a d/dtheta^a).
>>>>
>>>> Because the Koszul resolution is a resolution, if we take the spectral sequence of the double complex with gradings "sym degree in V" and "sym degree in theta" the cohomology of the E_1 page is concentrated in degree 0, so the spectral sequence collapses at the E_2 page and we're equivalently computing
>>>>
>>>> H^*(O(Pi S x Pi S[-1] x Pi V), d_Koszul + lambda^a d/dtheta^a).
>>>>
>>>> On the other hand, if we try to compute the spectral sequence taking the two summands of the

differential in the opposite order, the differential lambda^a d/dtheta^a is turning on a isomorphism Pi S --> Pi S[-1], so the E_2 page just looks like O(Pi V) with zero differential, and with no room for higher differentials. But that's not what we're supposed to get: the zero mode cohomology is supposed to look like the BV fields for super Yang-Mills, so C + V[-1] + S[-1] + V^*[-2] + S^*[-2] + C[-3], which doesn't even have the same Z/2 graded dimension as O(Pi V).

I assume I'm doing something foolish, but I can't see what it is. Any thoughts?

Best wishes,

Chris

> On 18/02/2019 17:26, Pavel Safronov wrote:
> I was writing some notes for myself trying to understand pure spinors, I attach them. Brian, can you add access to sugra on GIT for me (pgsafronov)? I'll add the tex file there.
>
> Regarding the computation that we need to do, it's essentially what Chris was proposing, but instead of adding D to the differential you are supposed to add Q (these are the left- and right-invariant vector fields on the spacetime). The distinction doesn't matter for the zero-mode cohomology, but matters when you compute the E_2 differential (there is a spectral sequence whose E_1 page is the zero-mode cohomology which converges to the full cohomology; its E_2 differential is the BV differential and people say it degener)ates at E_2).
>
> I haven't figured out how to show abstractly that the holomorphic twist of this theory is holomorphic Chern—Simons, but I'll continue thinking about it. By the way, is there a simpler example which we can try to work out first? E.g. there seems to be a pure spinor formulation of 4d N=1 SYM, but in the 4d case the space of square-zero supercharges is very simple: it's a union of two C^2's glued at the origin. Or possibly the holomorphic twist of the 4d chiral superfield is even easier to compute.
>
>> On Feb 14, 2019, at 2:17 PM, Chris Elliott <chris.elliott@cantab.net> wrote:
>>
>> By the way, I forgot to say earlier, but I've got my Skype working properly, so we're free to use that rather than Google Hangouts.
>>
>> Best,
>>
>> Chris
>>
>>> On 13/02/2019 12:05, Chris Elliott wrote:
>>>
>>> Ok, 3-4pm European time tomorrow it is. Talk to you then!
>>>
>>> Chris
>>>
>>>> On 13/02/2019 07:58, Pavel Safronov wrote:
>>>>
>>>> That's 3pm-4pm my time. Works for me!
>>>>
>>>>> On Feb 13, 2019, at 4:46 AM, Brian Williams <beezees222@gmail.com> wrote:
>>>>>
>>>>> I'm pretty busy both Th and F this week, but could meet for an hour from 9 AM — 10 AM EST on Th if that works.
>>>>>
>>>>> Best,
>>>>> Brian
>>>>>
>>>>>> On Feb 12, 2019, at 2:48 PM, Pavel Safronov <psafronov@gmail.com> wrote:
>>>>>>
>>>>>> Brian, do you have any constraints? I'm free all day until 5:30pm Swiss time.
>>>>>>
>>>>>> Pavel

On Feb 12, 2019, at 5:45 PM, Chris Elliott <chris.elliott@cantab.net> wrote:

Hi Pavel and Brian,

You suggested chatting on Skype on Thursday. Are you still interested? I should be free any time in the afternoon (so morning in Boston).

All the best,

Chris

On 09/02/2019 20:30, Chris Elliott wrote:

Hi Pavel,

No problem, and I see your point about G2 and Spin(7). Brian and I chatted a bit today, let's talk again on Thursday 14th (that day works fine for me, how about you Brian?). In the meantime I'm also working on other things but I'll try to think things through a bit more before we talk. Then we can hopefully make a plan next week.

Thanks,

Chris

PS: I'm sorry if you received this message twice, there's something wrong with the IHÉS SMTP server at the moment and some of my e-mails aren't making it through, so I'm resending with another account.

On 08/02/2019 18:55, Pavel Safronov wrote:

Hi Chris and Brian,

Sorry, I was busy this week trying to fix a mistake in a paper. I think I should be done early next week, then I can think about twists again and I'll reply to your email. Maybe we can skype on Thursday next week? (I.e. Feb 14.)

Quick answer to 4): I was insisting on having a superPoicnare invariant formulation of 10d superYang Mills as I'm hoping it will also give rise to superPoincare formulations of compactified theories. (Which is why I prefer the pure spinor formalism to the introduction of a single auxiliary field as in Baulieu.) I want to recover the results for A-twists of 7d and 8d Yang—Mills which count G2 and Spin(7) instantons I think. Having a not completely superPoincare-invariant formulation of theories, we will only see some subgroups instead.

Thanks for your email,
Pavel

On Feb 3, 2019, at 4:05 PM, Chris Elliott <celliott@ihes.fr> wrote:

Hi Pavel and Brian,

Sorry for not getting in touch sooner, I've been busy with job application stuff this past week. I've been trying to think about the pure spinor story we were talking about for 10d Yang-Mills, and I wanted to talk about what our plan is for progressing. Here are some unstructured thoughts.

1) The pure spinor theory is what we discussed before: it's the Chern-Simons type theory with linearized BV complex (C^oo(R^{10|16} x Pi P[1]) , Q), where P is the space of 10d pure spinors. The differential Q = lambda^a D_a splits up as a term lambda^a d_a involving only fermionic derivatives, plus a term that includes bosonic derivatives. The cohomology with the first summand of the differential is calculated in the appendix of
https://arxiv.org/pdf/hep-th/0105050.pdf
, but I feel like there's a more conceptual way of describing it. This cohomology looks like the BV complex of SYM without its differential.

There's an actual map from the cohomology into the complex. At first I guessed

There's an actual map from the cohomology into the complex. At first I guessed that turning on the SYM differential, and turning on the rest of the differential Q would describe a quasi-isomorphism, but thinking about it a bit more I'm sceptical: the differential from degree 1 to degree 2 is a first order differential operator on the pure spinor side and a second order differential operator on the SYM side.

2) If I understand correctly, the (off-shell) supertranslation action on pure spinor theory is just on superspace, but the Lorentz action is on the pure spinor space P as well.

3) If we just take the pure spinor theory as our definition of 10d SYM, I think that twisting -- like in the supergravity setting, just means turning on a non-trivial value for the pure spinor, i.e in the twisted theory by a pure spinor lambda_0, twisting means deforming the differential Q from lambda^a D_a to (lambda + lambda_0)^a D_a. Do you want to try redoing our calculation of the 10d holomorphic twist, showing this theory is equivalent to 5d holomorphic Chern-Simons, then going from there?

4) More generally, what do you guys envision as the structure of the paper, or even just the part of the paper that calculates the various twists? I think we've done enough calculations that we should start trying to draft some subsections, but I wanted to talk to you both about a plan first.

If you'd like to talk on Skype sometime I'm available any time next week.

All the best,

Chris