Referee Report

Supersymmetry bicomplex of pure spinor AdS background

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Summary

The manuscript studies the representation-theoretic structure of infinitesimal deformations of the pure-spinor description of the $\mathrm{AdS}_5 \times S^5$ background. The authors organize the BRST complex and the Lie superalgebra cohomology of $\mathfrak{psu}(2,2|4)$ into a bicomplex, and compare the two associated spectral sequences. One spectral sequence first takes the pure-spinor BRST cohomology and then computes Lie superalgebra cohomology, giving groups of the form $\mathrm{Ext}^p(\mathcal{R}, H^q_Q)$. The other first computes the Lie cohomology on the off-shell vertex complex and is argued to be more tractable through Shapiro's lemma and the covariant subcomplex. The central idea is that the equality of the limiting pages imposes nontrivial constraints on the less accessible Ext groups.

The paper gives a useful review of the relevant pure-spinor vertex cohomology in $\mathrm{AdS}_5 \times S^5$, including ghost-number-one symmetry vertices, ghost-number-two physical and nonphysical deformations, and the special status of ghost-number-three vertices. A main theme is the treatment of the constant dilaton and axion zero modes at ghost number three, and their relation to the failure of certain representatives to be covariant under the full supersymmetry algebra. The authors then compute low-degree Lie superalgebra cohomology of $\mathfrak{psu}(2,2|4)$ in the trivial and adjoint representations, use these computations to populate the second pages of the two spectral sequences for $\mathcal{R} = \mathbb{C}$, and present an additional discussion for $\mathcal{R} = \mathrm{ad}$.

The manuscript contains several attractive observations. The degree-two comparison between the two spectral sequences is particularly clear: the difference $\lambda^2_{\mathrm{L}} - \lambda^2_{\mathrm{R}}$ is related to $H^1_{\mathfrak{g}}(\mathrm{ad})$, while the sum $\lambda^2_{\mathrm{L}} + \lambda^2_{\mathrm{R}}$ is related to the central extension class $c_2 \in H^2_{\mathfrak{g}}(\mathbb{C})$. The discussion of the Chern-Simons descent leading to the nonzero $\widetilde{d}_4$ on $\mathrm{Tr}(e^{-\omega} de^{\omega})^3$ is also conceptually illuminating. The work is potentially valuable for researchers working on the pure-spinor formulation of strings in AdS backgrounds, superalgebra cohomology, and the organization of non-normalizable deformations in AdS/CFT.

Evaluation

The topic is suitable for a high-energy theory journal. The paper addresses a specialized but meaningful problem, and the method of comparing the two spectral sequences is a promising way to extract information about the representation structure of the BRST cohomology. The relation to earlier work on pure-spinor vertices, beta-deformations, and the symmetry bicomplex is sufficiently clear at the level of motivation, and the low-degree computations, if fully substantiated, would constitute a useful contribution.

At the same time, the present manuscript is not yet in a form where the main mathematical claims can be checked reliably by a reader. Several statements that are central to the final dimension counts are explicitly conjectural: the vanishing of $H^1(\mathfrak{g}_{\mathrm{even}}; H^2_Q)$, the vanishing of the relevant $E_1^{0,2}(H^2_Q)$ term, the assertion that two displayed classes generate $H^2(\mathfrak{g}; H^2_Q)$, and the proposed action of higher differentials for total degree greater than two. Other important steps are described as direct computations without enough detail to reproduce them, for example the rank-one transgression in the Hochschild-Serre computation of $H^3(\mathfrak{g}; \mathbb{C})$ and the relative cohomology computations in the adjoint representation. These gaps do not necessarily invalidate the results, but they make the manuscript read partly as a research program or conjectural framework rather than as a completed derivation.

I therefore recommend publication only after major revision. The paper should be acceptable if the authors either prove the conjectural entries needed for the principal claims, or else explicitly delimit the manuscript as providing conjectures and consistency checks rather than full computations of the second and limiting pages.

Major Comments

1. **The status of the main results should be made precise.**

   The abstract says that the paper clarifies the structure of ghost-number-three zero modes, and the introduction emphasizes constraints on the representation structure from matching the two spectral sequences. However, by Section 10 the manuscript states that for $p + q > 2$ the higher differentials have not actually been computed and that the proposed picture is conjectural. Section 11 similarly says that the description of E_2 in the preceding sections is conjectural because several dimensions have not been rigorously proven.

   This distinction is important. The reader needs a concise statement, preferably near the end of the introduction, separating proven results from conjectures. For example, the degree-two matching appears to be a genuine derivation, while the degree-three and degree-four dimension matching for $\mathcal{R} = \mathbb{C}$ depends on conjectural vanishings and on inferred higher differentials. The final conclusions should use language consistent with this status. If the authors intend the paper to establish the degree-three zero-mode structure as a theorem, they need to supply the missing proofs; otherwise the abstract and conclusion should say that the paper proposes and tests a spectral-sequence picture for those zero modes.

2. **The conjectural cohomology vanishings involving H^2_Q need proof or stronger evidence.**

   Section 8 is crucial for the later tables. The argument for $H^1(\mathfrak{g}_{\text{even}}; H^2_Q) = 0$ is based on flat-space intuition about linear axion and dilaton profiles and their image under d_1. The manuscript then conjectures $E_1^{0,2}(H^2_Q) = 0$ and conjectures that $(\Lambda \circ \mu_1)^2$ and $c_2 \operatorname{Str}(\lambda_{\text{L}}\lambda_{\text{R}})$ generate the relevant second cohomology.

   These are plausible statements, but they are too central to remain at the level of intuition if the final dimension tables are to be used as results. The authors should either provide a real computation of these groups, perhaps in an appendix using the Hochschild-Serre machinery and the known decomposition of H^2_Q, or state explicitly that the subsequent E_2 tables are conditional. In particular, the treatment of physical versus nonphysical beta-deformations should be spelled out carefully enough that the reduction from the two classes in $H^2_{\mathfrak{g}}(\beta)$ to the single class in all of H^2_Q can be independently checked.

3. **Several "direct computations" should be expanded.**

   The computation of $H^\bullet(\mathfrak{g}; \mathbb{C})$ and $H^\bullet(\mathfrak{g}; \text{ad})$ is one of the technical pillars of the paper. The use of the Hochschild-Serre spectral sequence for $\mathfrak{g}_{\text{even}} \subset \mathfrak{g}$ is appropriate, and the bonus $\mathfrak{u}(1)_Y$ grading is a useful organizing principle. Still, some key claims are stated too tersely. In Section 7, the claim that a direct computation makes the d_4 transgression rank one on the two-dimensional $Y = 0$ quartic subspace should be shown. Likewise, the relative cohomology differential in the adjoint case is summarized by its output, but the action of d_1 on the invariant generators is not displayed.

For a journal article, the reader should be able to verify these low-degree cohomology computations without reconstructing them from scratch. I suggest adding an appendix listing the invariant generators, the differential matrix in the chosen basis, and the resulting kernels and images. This would also help fix normalizations and remove any ambiguity in the definitions of c_2, c_3, μ_1, and $\mu_3^{\uparrow,\downarrow}$.

4. **The discussion of ghost-number-three zero modes needs a more explicit argument.**

   Section 3 explains that ghost-number-three vertices are almost in one-to-one correspondence with linearized supergravity solutions, except for the missing constant RR five-form flux and the two-dimensional kernel generated by the dilaton and axion zero modes. Section 4 then studies Lorentz-invariant expressions of type $\lambda^3\theta^5$ and argues that the parity-odd axion zero-mode representative can be completed in $\mathrm{AdS}_5 \times S^5$ inside a finite-dimensional representation, while no analogous argument is available for the parity-even dilaton zero mode.

   This is an interesting result, but the present argument is compressed. The absence of the listed $\lambda^4\theta^6$ obstructions, the uniqueness of the $\lambda^4\theta^8$ obstruction, and its parity assignment rely on nontrivial gamma-matrix and representation-theoretic facts. These should be given as explicit decompositions or identities. The statement that the parity-even expression should also complete to a representative invariant up to Q-exact terms, despite the lack of a finite-dimensional representative, should be clearly labeled as a consequence of earlier general arguments rather than as a completed construction.

5. **The inference from noncovariance to nonzero higher differentials should be justified.**

   In Section 10 the authors argue that neither the dilaton nor the axion ghost-number-three zero mode survives to E_∞, because these zero modes cannot be chosen covariantly, so some higher differential must kill them. This is a central interpretive step. As written, it is not fully justified: noncovariance of a representative does not by itself make clear which page differential must act nontrivially on the corresponding class, nor does it identify the target class except through parity and dimension matching.

   The paper would be much stronger if the authors computed at least one of these differentials explicitly, even modulo a controlled ansatz. If that is not feasible, the discussion should be reformulated as a conjectural matching argument. The possible targets in $E_2^{2,2}$ and $E_2^{3,1}$ should be listed with their parities, and the unresolved ambiguity should be stated. The sentence that $d_3 W_{\mathrm{axion},0}$ "must be equal" to one of the two parity-odd elements should be weakened unless the dimension and filtration arguments determine it uniquely.

6. **There appears to be a notation error in the matching argument of Section 11.**

   Section 10 first gives $\dim \widetilde{E}_2 = 2$ in total degree three and $\dim \widetilde{E}_2 = 7$ in total degree four, and then argues that after higher differentials the limiting dimensions should be one and six. Section 11 then refers to matching with $\dim \widetilde{E}_2^{p+q=3} = 1$ and $\dim \widetilde{E}_2^{p+q=4} = 6$. These should presumably be $\widetilde{E}_\infty$ dimensions, not $\widetilde{E}_2$ dimensions. This should be corrected, since the distinction between the second page and the limiting page is the main conceptual issue in the paper.

7. **The $\mathcal{R} = \mathrm{ad}$ case is underdeveloped relative to its prominence.**

   The final section states the $\mathcal{R} = \mathrm{ad}$ grids and says that all generators are covariant, hence all higher differentials vanish, and that the total dimensions match. This is a useful check, but the derivation is too short. The cohomology of the covariant pure-spinor complex in the adjoint representation is stated as immediate, including a trivial second cohomology and a six-dimensional third cohomology. These claims should be justified, or else this section should be presented more modestly as a consistency check supported by the tables. As it stands, the reader has to trust the tables without enough explanation of how the entries were obtained.

Minor Comments

1. The manuscript contains several typographical errors that should be fixed before publication: "field strenght", "worlsdsheet", "wordsheet", "Action of of", and "The central charge is of $\mathfrak{g}$ is given by". There is also a subsection title in the degree-four discussion that appears to say "Structure of $\widetilde{E}_2$" where the text is describing E_2.

2. The notation alternates between Str and STr. If these are meant to be the same supertrace, the notation should be made uniform.

3. The manuscript sometimes uses informal language such as "we are not sure" and "we do not have a rigorous proof". It is good to be transparent, but in the final version these phrases should be replaced by precise statements of conjectures, assumptions, or open problems.

4. The definitions of λ_{L}^2 and λ_{R}^2 are given early in the paper, but these symbols play a major role later. It would help to remind the reader of this abbreviation when the degree-two comparison begins.

5. The gray entries in the tables should be explained more explicitly in the captions. Some entries are not computed, some are asserted to vanish, and some are irrelevant to the displayed total-degree comparison. These distinctions should be visually or textually clear.

6. The relation between formal Taylor-series vertices, non-normalizable deformations, and physical supergravity solutions should be summarized more sharply in the introduction. This would help readers understand the precise domain of the representation-theoretic statements.

Recommendation

I recommend major revision. The manuscript addresses a worthwhile and technically sophisticated problem, and the bicomplex perspective is likely to be useful to specialists. The degree-two analysis and the proposed organization of the higher-degree data are interesting enough to merit publication. However, the current version relies on several conjectural vanishings and inferred higher differentials while presenting some of the resulting tables and conclusions with more definiteness than is warranted. A revised version should either supply the missing computations or explicitly state the conditional nature of the higher-degree claims, and it should expand the key low-degree cohomology calculations so that the results are reproducible.