Referee Report

Gravitational Effective Theories with Maximal Supersymmetry and a Peculiar Parity

Summary and overall assessment

The manuscript studies four-dimensional tree-level effective theories of $\mathcal{N} = 8$ supergravity with SU(4) $\times$ SU(4) R-symmetry. The authors construct the relevant four-, five-, and six-point superamplitudes, use factorization to constrain the six-point NMHV amplitude, and impose a restricted "peculiar parity" condition on a six-scalar amplitude in the $(\mathbf{6}, \mathbf{6})$ sector. Their principal finite-order result is the set of nonlinear relations among four-point Wilson coefficients displayed in equation (5.3), beginning with

$$\kappa^2 g_3 = \frac{1}{2} g_0^2, \qquad \kappa^2 g_5 = g_0 g_2,$$

and continuing through $\mathcal{O}(s^{13})$. The authors then combine these relations with dispersive positivity. Numerically, they find a non-convex allowed region with distinguished Virasoro–Shapiro and infinite-spin-tower corners. Analytically, they observe that the checked relations reorganize the amplitude into the exponential form (6.9); after assuming this form to all orders, they derive the spectral exponential (6.12), a meromorphic product (6.13), and a proposed uniqueness argument for the Virasoro–Shapiro spectrum.

The finite-order six-point result is original, technically substantial, and potentially important. In particular, deriving nonlinear four-point information from six-point supersymmetry, factorization, and a restricted parity property is a meaningful gravitational extension of the mechanism previously found in maximally supersymmetric Yang–Mills theory. The displayed relations are internally consistent with the exponential ansatz: expanding (6.9), using the Newton identities for $s + t + u = 0$, reproduces all the relations in (5.3), including the two independent relations at order g_9. The Virasoro–Shapiro and tuned IST examples also pass these finite-order checks.

However, several claims that support the headline conclusion are not correct or not proved as written. Most importantly, the all-orders exponentiation is assumed rather than derived; the proposed noninteger-N family has its poles and zeros reversed in the text and develops unphysical negative-axis poles; and the finite-spin argument following (6.14) omits a necessary use of partial-wave positivity. In addition, an asymptotic bound used in Section 6 is not obeyed by several amplitudes treated as allowed, while the central computer-assisted calculations and numerical bounds cannot presently be reproduced from the information supplied.

Recommendation

I recommend **major revision**. I would be willing to reconsider a revised manuscript because the finite-order six-point constraints alone appear to be a strong and publishable result. Publication requires a clear separation between established finite-order statements, numerical evidence, and conditional all-orders conclusions, together with correction of the concrete analytic errors below and enough supporting material to audit the computations.

Major comments

1. **The advertised uniqueness result is conditional on an unproved all-orders extrapolation.**

   Section 6.4 states explicitly that the exponential form (6.9) has been verified only through $\mathcal{O}(s^{13})$, and then says that the subsequent analysis proceeds "upon assuming" that this form holds to all orders. Equations (6.12)–(6.15), including the product representation and the Virasoro–Shapiro uniqueness claim, all depend on this assumption. A finite check, even a highly nontrivial one through $\mathcal{O}(s^{13})$, does not establish the infinite sequence of nonlinear relations.

This conditional status is not reflected in the abstract, the final paragraphs of the Introduction, the Note added, Section 6.3, or the Conclusions. Those parts say that the stated physical assumptions analytically isolate the closed-string amplitude, and the Note added contrasts this work with the contemporaneous paper by Xu by saying that exponentiation is deduced here. As the manuscript stands, what is deduced bottom-up is the finite set of relations (5.3); all-order exponentiation is a well-motivated conjectural continuation of those relations.

The authors should either prove that six-point supersymmetry, factorization, and peculiar parity imply (6.9) to every EFT order, or formulate a precise conditional theorem. The latter should state separately: (i) the verified relations through $\mathcal{O}(s^{13})$; (ii) the assumption that their exponential pattern holds to all orders; and (iii) the additional analyticity, convergence, Regge, and positivity hypotheses used after (6.9). The abstract and conclusions should then describe the analytic result as conditional, while presenting the finite-order derivation as the primary unconditional result.

2. **The noninteger-N continuation in equation (5.18) has an incorrect pole interpretation.**

   For one channel, set $x = s/m^2$. Combining the Virasoro–Shapiro factor with the gamma-function ratio in (5.18) gives

$$R_N(x) = \frac{\Gamma(1-x)\,\Gamma(1+x+N)}{\Gamma(1+x)\,\Gamma(1-x+N)}.$$

   For noninteger real $N > 1$, this function has poles at

$$x = 1, 2, 3, \ldots, \qquad \text{and} \qquad x = -(N+1), (-N-2), \ldots.$$

   By contrast, $x = N+1, N+2, \ldots$ are zeros, not positive-mass poles. The unwanted negative-axis poles cancel against zeros of the Virasoro–Shapiro factor only when N is a positive integer, in which case (5.18) reduces to the finite product (5.17). Thus the paragraph following (5.18) reverses the additional poles and zeros, and the claimed real-N spectrum does not have the stated analytic structure.

   This is not merely a terminology problem. The negative-axis singularities are not ordinary t- or u-channel pole surfaces and conflict with the fixed-transfer analyticity assumed in Section 6.1. The corresponding analytic continuation of the coefficients is

$$H_N^{(p)} = \zeta(p) - \zeta(p, N+1) = \sum_{n\geq 1} \frac{1}{n^p} - \sum_{q\geq 0} \frac{1}{(N+1+q)^p},$$

   which is a signed, rather than positive, atomic moment representation for noninteger N. Hence the continuous purple curve in Figures 1 and 2 is not exhibited by (5.18) as a family of unitary positive amplitudes. Equation (5.20), written as a finite sum ending at noninteger N, is also undefined; the Hurwitz-zeta continuation must be stated if it is used.

   The authors should restrict the physical product family to positive integer N, correct the pole discussion, and reassess every claim that the purple real-N curve is a continuous positive interpolation between the IST and the string. If a noninteger continuation is retained only as a formal interpolation of Wilson coefficients, it must not be described as a physical spectrum or a unitary amplitude without an independent positive spectral construction.

3. **Polynomiality of the lowest residue does not by itself prove the exact linear spectrum in (6.15).**

   The argument following equation (6.14) correctly shows that canceling the pole at $t = m_1^2$ forces a state at $m_2^2 = 2m_1^2$, which in turn forces the integer sequence $m_n^2 = nm_1^2$. It does not show that this sequence exhausts the spectrum. An explicit counterexample to that inference can be given in units $m_1^2 = 1$: consider

$$\{m_n^2\} = \{1, 2, 3, \ldots\} \cup \{r, r+1, r+2, \ldots\}, \qquad r > 1, \quad r \notin \mathbb{Z}.$$

   The shifted chain contributes to the lowest-level residue the telescoping product

$$\prod_{k=0}^{\infty} \frac{(r+k+1)(r+k+t)(r+k-1-t)}{(r+k-1)(r+k-t)(r+k+1+t)} = \frac{(r+t)(r-1-t)}{r(r-1)}.$$

This is a polynomial. Therefore, the residue still contains only finitely many spins even though the spectrum is not the single linear tower asserted in (6.15). Pole cancellation forces the integer subsequence, but extra poles organize into additional shifted unit-spaced chains rather than being excluded.

Full partial-wave positivity appears capable of repairing the argument, but it must actually be used. Writing $z = 1 + 2t$ at the lowest level, the factor from one shifted chain is

$$1 + \frac{1 - z^2}{4r(r-1)},$$

whose Legendre expansion has a strictly negative $P_2(z)$ coefficient. More generally, products of extra shifted chains give positive combinations of $(1-z^2)^q$ with $q > 0$, each having negative P_2 projection. This suggests a concise positivity proof excluding the extra chains. The current statement that "inspection" of (6.14) alone forces the exact spectrum is nevertheless incomplete. The authors should supply the classification of possible extra chains and the positivity argument, and then state precisely which residue-level unitarity assumption completes the theorem.

4. **The main assumptions omit discrete symmetries that are used structurally in the six-point construction.**

   The abstract and conclusion list maximal supersymmetry, SU(4) $\times$ SU(4) R-symmetry, factorization, and peculiar parity. Section 4, however, additionally assumes exact CP and the symmetry S interchanging the two SU(4) factors. The S symmetry is used already in equation (4.2) to reduce the NMHV basis, and the parity-odd contact structures listed in (4.11) are discarded immediately afterward using CP and S. These restrictions are therefore not merely cosmetic choices of notation.

   The manuscript argues that these symmetries can affect only linear constraints on five- and six-point local terms, but it does not demonstrate that the additional parity-odd contact terms allowed without them cannot absorb or modify the parity condition on $\mathbb{Z}_{1,1}$. Since the nonlinear four-point relations are obtained after eliminating precisely such higher-point contact freedom, this independence needs proof. The authors should either include the exact $\mathbb{Z}_2 \times \mathbb{Z}_2$ symmetry among the hypotheses of all principal results, or repeat the elimination with the full contact-term basis and show explicitly that (5.3) is unchanged. The claimed enhancement from the one condition on $\mathbb{Z}_{1,1}$ to $O(6) \times O(6)$ should likewise be supported by explicit component Ward identities rather than asserted from the double-copy labeling.

   There is a related foundational point in Section 3.2. The statement that every higher-derivative three-field gravity operator can be removed by field redefinitions is false in general: a Weyl-cubed interaction produces genuine all-plus and all-minus three-graviton amplitudes. Equation (3.9) may well be exhaustive in this theory because maximal supersymmetry forbids the relevant R^3 supervertex, but that is the argument that should be given and cited, since all subsequent factorization channels rely on the assumed three-point input.

5. **The Regge bound (6.2) is not obeyed by several amplitudes treated as allowed.**

   Equation (6.2) asserts that $M_4(zz\bar{z}\bar{z})/s$ is bounded at large $|s|$ and fixed $u < 0$. The one-mass IST and every finite product of the form (5.17) instead have a product ratio tending to a nonzero constant while the kinematic prefactor behaves as $s^4/(stu) \sim s^2$. Consequently $M_4/s \sim s$. For the Virasoro–Shapiro amplitude, Stirling asymptotics at fixed u give

   $$M_4 \sim s^{2+2\alpha' u},$$

   so M_4/s grows for $-1/(2\alpha') < u < 0$. Thus the displayed bound excludes both an amplitude at the proposed IST corner and the target string amplitude in part of the fixed-transfer domain.

   The authors emphasize that their coefficient sum rules use only $k \geq 4$ subtractions. Those relations may remain valid under a weaker gravitational Regge bound, and this issue may therefore be repairable without changing the finite-order positivity analysis. Nevertheless, (6.2) should be replaced by the actual asymptotic hypothesis sufficient for the contours used. More importantly, Section 6.3 invokes (6.2) in arguing that finite spin at the gap implies scalar exchange there. That inference and the spectral ansatz (6.8) must be revisited under a bound that the Virasoro–Shapiro amplitude itself satisfies. The distinction between unsmeared forward relations and the infrared-safe smeared sum rules mentioned at the end of Section 6.1 should also be incorporated into the hypotheses rather than left as an expectation.

6. **The central computer-assisted amplitude calculation and the numerical bootstrap are not reproducible.**

   The relations (5.3) and the counts in Tables 1–3 are the core evidence for the paper, yet the manuscript supplies neither the explicit high-order bases nor ancillary code. The six-point construction uses numerical inversion of the 15×15 change-of-basis matrix, random four-dimensional kinematic points to test polynomial independence, a degree-24 spurious-pole cancellation, and a pole analysis centered on $M_6(zzz\bar{z}\bar{z}\bar{z})$. The statement that a "selection" of other amplitudes gives no further constraints is not sufficiently precise to establish completeness.

   The authors should provide machine-readable supporting material containing the change-of-basis data (or a constructive exact representation), the $S_2 \times S_4$-symmetric contact-term basis and its stated 29 generators, the kinematic parameterization, exact or rational reconstruction procedure, sample points and precision, rank and spurious-pole tests, the complete component/channel list, and the solved systems that produce Tables 1–3 and (5.3). At least the first nonlinear relation $\kappa^2 g_3 = g_0^2/2$ should be derived transparently in the paper, showing why no allowed local six-point term can absorb the relevant parity-odd factorization contribution.

   Figures 1 and 2 require a parallel reproducibility upgrade. The paper should state, for each colored region, the null-constraint order, spin truncation or tail treatment, polynomial approximation, SDPB precision and tolerances, scan spacing, normalization, and feasibility criterion. A convergence table or plot should quantify the movement of the string corner and the size of the island as constraints are added. Since Section 6.2 itself says the bounds are not converged, the phrases "sharp corner," "numerically isolate," and "rigorously excluded" should be calibrated to the actual truncation. The axes should also display the dimensionless combinations $\Lambda^4 g_2/g_0$ and $\Lambda^6 g_3/g_0$, or state explicitly that $\Lambda = 1$.

7. **The meromorphy step and the finite-product unitarity claims require sharper proofs.**

   Even conditional on all-order exponentiation, the passage from (6.12) to (6.13) uses several nontrivial assumptions: interchange of the EFT series with the spectral integral, control of the infinite product, real analyticity throughout the massless physical interval, quantization of the logarithmic discontinuity, exclusion of continuous spectral support, exclusion of multiplicities $c_n > 1$, and absence of additional entire factors. The manuscript itself acknowledges that the needed real-analyticity statement is not part of an established massless axiomatic framework. These ingredients should be stated as explicit hypotheses and organized as lemmas or propositions. Positivity of the spin-summed density used in (6.12) must also be kept distinct from positivity of every partial-wave coefficient of every residue.

   The discussion after equation (6.16) illustrates the same issue. Projecting one residue onto spin zero and studying a large-spin condition at the other mass are necessary tests, but they do not by themselves prove that every partial wave at both masses is nonnegative. The conclusion that the two-mass product "is unitary if" the ratio lies in (6.17) therefore requires an all-spin proof or complete numerical evidence. Otherwise it should be weakened to say that the amplitude passes the tests performed. Moreover, the endpoints near 1.25 and 5.63 cannot be described as roots of a cubic polynomial when the displayed expression (6.16) contains $\log(r+1)$; the exact defining equation and numerical procedure should be given.

8. **The novelty claims should distinguish the finite-order advance from antecedent and overlapping work.**

   The strongest novel result is the bottom-up derivation of gravitational nonlinear relations from a six-point parity condition. This should be foregrounded. The numerical non-convexity and bifurcation have a close precedent in the cited hidden-zero analysis; the analytic reasoning after exponentiation follows the strategy of the cited analytic Veneziano bootstrap; and the contemporaneous work by Xu studies a Virasoro-inspired nonlinear ansatz and an analytic island. The Note added should compare assumptions, overlapping coefficient relations, the product argument, and the role of KLT information in concrete terms. It should not claim that this manuscript deduces exponentiation unless the all-orders gap is closed.

Conversely, the manuscript has a genuine point of distinction: no string spectrum, KLT relation, or monodromy identity is assumed in deriving the checked six-point relations. That statement is both accurate and significant. It is more defensible than the broader claim in Section 7 that the assumptions are not string-theoretic in nature, since peculiar parity is explicitly motivated by the genus-zero string worldsheet. The SU(8) result in Section 4.4 should also be positioned more carefully relative to established results on $E_{7(7)}$, scalar soft limits, and the incompatibility of stringy R^4 corrections with unbroken continuous symmetry.

Minor comments

1. The IST spin content is stated inconsistently. The Introduction gives $J = 0, 2, 4, \ldots$, whereas Section 5.3.2 says $J = 0, 1, 2, \ldots$, and the abstract says "every spin." The residue of the stated crossing-symmetric amplitude should be decomposed and the even-spin content reported consistently.
2. Section 5.3.1 says that the five-point string comparison was checked through $\mathcal{O}(s^7)$, while Appendix E says $\mathcal{O}(s^9)$. These statements should agree.
3. In the opening of Section 6.3, "one at the IST and one at the open string" should read "closed string."
4. The claim that $\zeta_{3,3,5}$ is algebraically independent of single zeta values should be qualified. If motivic independence is intended, this should be stated and cited; numerical algebraic independence is not established in the form asserted.
5. The sum in the exponent of equation (6.9) needs an explicit upper limit, presumably $k = \infty$. In the paragraph after (6.10), the forward-limit exponential collapses to unity, not "to a sum."
6. Unitarity requires $\rho_j(s) \geq 0$, rather than strict positivity as stated below (6.4); absent spins have zero spectral weight.
7. The multiline display (5.3) suppresses the number on every populated row and attaches its label to an empty final row. The equation number should be placed visibly on the final nonempty line or the display should use an `equation/aligned` environment.
8. Section 2 begins "The positive and a negative helicity gravitons." This should be "the positive- and negative-helicity gravitons."
9. For positive integer N, the products (5.17) have finitely many distinct mass levels, with an infinite spin tower at each level. The present wording can be read as claiming an infinite set of masses at $1 \leq n \leq N$.
10. The bibliography entry labeled `Stieberger_website` has empty author and title fields. It should be replaced by a complete, stable reference.

Publication assessment

The manuscript addresses a timely question at the intersection of amplitudes, EFT positivity, maximal supersymmetry, and the string bootstrap. Its finite-order six-point analysis is technically ambitious and, if made reproducible, should be of substantial interest to the high-energy theory community. The present version does not yet meet the standard for publication because the strongest theorem is conditional but advertised as unconditional, and because the noninteger product family and the finite-spin proof contain concrete errors or gaps. A revision that corrects these points, states all hypotheses precisely, releases the computational support, and repositions the uniqueness claim would be a strong candidate for publication.