Review: "Extremal stringy black holes in equilibrium at first order in α'"

by

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In this paper, the leading higher-derivative corrections to multi-center black holes are studied in the supergravity theory describing the low-energy effective field theory of the heterotic string. It is argued that these are still in equilibrium including those corrections both for supersymmetric and non-supersymmetric (extremal) cases.
The work situates itself in the context of several publications where subsets of the authors and their collaborators study various aspects of such higher-derivative (α'-)corrections in this theory with an emphasis on black hole solutions, their charges, their entropy and their thermodynamics more generally. The current work distinguishes itself specifically from [11][1] by considering also non-supersymmetric solutions while it is different from [9][2] due to the consideration of multiple centers. A novel feature that can therefore be discussed by the authors in this setup is black hole fragmentation in both the supersymmetric and the extremal (non-supersymmetric) cases. That discussion is unfortunately limited in the present manuscript. Nevertheless, in my opinion, the paper is well-written and could be sufficiently relevant and independent within this systematic study of higher-derivative corrections to the low-energy effective field theory of the heterotic string to merit publication upon addressing the following questions:

1. The starting point for the discussion of new results is the claim that, for the 10d fields, (3.1a) and (3.1b) represent the entire first order in α' modification. Some intermediate results towards deriving this are presented in the appendix. Yet it was outside the scope of this review to go through this key calculation in full detail. Given that the final argument presented in Appendix D seems heavily based on the result of [11], could the authors confirm that they have checked that the final solution satisfies the equations of motion independently of intermediate results in [11] from the point onwards where the sign difference may have been included, i.e. from where the specific ansatz for $\hat{H}$ was used?

   Moreover, I believe that the relation between the vanishing of $T^{(2)}_{++}$ and $\mathcal{Z}_+$ could be made more clear in appendix D. In [11], it is noted that this is established due to the simple form of the ++ component of the Einstein equations in terms of $\mathcal{Z}_+$. One can presumably easily argue based on the quadratic form of H in those Einstein equations as well as the vanishing of cross-terms (due to the relative directions of support) between the term that changing signs, as is done for the non-supersymmetric case, doesn't modify this relation. Adding such an explanation, or an equivalent one would, in my opinion, give a more complete argument.

2. The continued absence of singularities and struts in the corrected solution is an important claim of the paper, yet I believe it could be made more clear how substantial the arguments for this are in the present form of the manuscript. It seems that the argument for the absence of singularities is in the words of the authors "... if they appear in the corrected solutions, it must be in a regime of charge parameters where the perturbative expansion is no longer justified ..." as stated on p23 in the beginning of section 3.1.1. The necessary work is then to carve out more explicitly the regime of validity and some aspects of this are addressed in the manuscript. However, in this discussion I missed how in general the regime of validity depends on the interplay between the size of the charges and their relative distance as compared to the scale of the corrections. For instance, to what extent can I take the limit of two centers approaching each other, say in light of the concluding statement at the end of 3.1.1 on p24 that "... even if we consider a complicated configuration, approaching a center the corrections to $\mathcal{Z}_{+,0}$ reduce to those of a single black hole"? It seems to me that such

[1] In the manuscript, here [1].
[2] In the manuscript, here [2].

questions would be important to more confidently address the more detailed aspects of black hole fragmentation.

In addition, as the authors discuss in the introduction (and stress in Figure 1 and Figure 2), the absence of struts is important for the black holes to be in equilibrium by themselves, i.e. in absence of external forces as provided by say such struts. In footnote 26, in 3.1.1 on p 23, it seems to be suggested that previous regime of validity argument is not valid for the struts. Instead, if I understand correctly, the argument here rests solely on a seeming absence of interaction energies suggested by the relation between the total mass and the putative single center masses. Is this correct and would the authors agree that, even though suggestive, this does not rule out their possible presence?

Finally, I would like to make the following minor questions/remarks/typos which the authors may wish to consider

1. In the introduction, it is noted in a remark (p, last paragraph before point 3. "It is worth mentioning ...") that the α'-corrected T-duality rules discussed from [17], [18][3], investigated in [11] for the supersymmetric family, also stay within the multicenter family for the non-supersymmetric case. It seems, naively due to the vanishing of both the $\mathcal{Z}_+$ and $\mathcal{Z}_-$ α'-correction that the extremal case in fact seems more symmetric with respect to such a transformation, than the supersymmetric case discussed in [11]. Is this true and, if so and the authors have given this some thought, is there some reason for this?

2. First sentence of the final paragraph on p5, is the use of 'non-extremal' correct? It seems as if the setup of the paragraph was to argue for considering 'extremal' ones first. Moreover, the last sentence, 'it will be long before ...' may also not quite be as the authors intended as the paper [16] seems to have been published as [5] in the meantime.

3. A comparison with a more microscopic perspective on the entropy is mentioned in the introduction as a motivation with reference to the seminal work [2] ([6] here). While there seem to be matches for non-supersymmetric cases, the supersymmetry was, as far as I'm aware, important to confidently argue for a protected state degeneracy, or an entropy that can be reliably computed in both regimes. Is this still correct and if so, doesn't this affect the program suggested by the authors?

4. On page 6, just above the start of the last paragraph on that page, it is mentioned that: "the only true tensor field in the Standard Model is the metric". Depending on what is meant with this statement, although perhaps semantic, I would not consider the metric a part of the Standard Model. More interestingly, in the context of their theory, would the authors say the metric is a true tensor field?

5. On page 6, in the last sentence "In [35], we applied ..." 'we' may not be very appropriate given that only a subset of the authors wrote that paper and some authors on that paper are not authors of the manuscript.

6. Section 2 may benefit from some further references. Even if supposedly "well-known".

[3] That is [3] and [4]

7. The authors may which to also cite and relate to other approaches to the study of higher-derivative corrections within string theory, for instance, in the context of precision holography, e.g. [7, 8, 9] and related work.

8. The notation for the curvature of the torsionful connection introduced in the appendix (A.2) is used in the discussion without introduction I believe.

References

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