1 SciPost Report 1 on Part II from Lorenz Eberhardt

The authors make a brave attempt at one of string theory's most notorious problems: to give a general direct derivation of Hawking's black hole entropy formula $S_{\mathrm{BH}} = \frac{A}{4G}$. The paper does not contain major new ideas, but instead clarifies several existing approaches and puts them into perspective, as well as discusses their strength and weaknesses. The literature on this subject is filled with vague and partially contradictory statements and the authors do a great job of informing the reader about their validity from a modern point of view.

The main approach that the authors pursue is a stringy uplift of the derivations of black hole entropy from the gravitational path integral. This entails computing the leading contribution of order $\frac{1}{G}$ to the string partition function, which is captured by the sphere in perturbative string theory. Computing the sphere partition function of a string worldsheet theory is a subtle problem and the authors use the technology developed in their previous paper which in turn is based on old papers of Tseytlin. In those papers, it is proposed to allow off-shell string backgrounds, i.e. string backgrounds that do not satisfy the equations of motion and which are not represented by CFTs on the worldsheet. This requires the introduction of a UV-cutoff ε on the worldsheet and the effect of dividing by the volume of the Moebius group is roughly realized in the off-shell approach by taking a derivative with respect to $\log \varepsilon$. The authors use this prescription and demonstrate that one can derive the full off-shell spacetime effective action order by order in α' from the formalism. They carry this program explicitly out to first order in α' (which already involves 2 -loop computations in the dilaton sector).

I greatly appreciate that the authors attack such a difficult problem in string theory. Even though I would argue that their main contribution is to flesh out and compare various partially existing proposals and computations in the literature, I think that this paper is a very valuable resource for the community. It is a bit anti-climactic that the computation is essentially reduced from a string theory computation to a gravity computation and is thus not inherently stringy'. In particular these methods will presumably not give new insight into the nature of black hole microstates in quantum gravity. I also thought that although one of the main goals of the paper was to clarify Susskind's \& Uglum's open string picture, they did not add much more to it. I think that this paper is suitable for publication in SciPost.

I have some mostly minor remarks:

1. Eq. (8): I don't understand the last step, why is it still z-dependent? Is this some sort of zero mode of $\log G(X(z))$?

   This seems to be a typo which we have corrected.

2. The first term in eq. (10) should presumably read $\frac{1}{4\pi\epsilon^2}$.

   Yes, the ϵ should have been ϵ^2, and there was a similar error in a couple other places, which we have now fixed.

3. Page 9, last paragraph of section III: Please provide a reference for the mentioned renormalization theorem (the same appears again in the discussion section VI.B without reference).

   Please see the last subsection of 12.6 Polchinski vol II, and Martinec 1986 as cited therein. We have added these refs to the paper.

4. Please also provide a reference for eq. (45). In what sense are the expectation value brackets around $\frac{A}{4G}$ to be interpreted?

   We have added some citations for this (starting with Bekenstein and Hawking). The reasoning behind the expectation values is explained in Sorkin and Sudarksy as well as a couple of papers by one of us (Aron Wall).

5. Eq. (53): $t \to \tau$

   Thanks for spotting this typo.

6. Paragraph after eq. (60): The claim that RG flow on the worldsheet corresponds to a Ricci flow in the target manifold should be explained better. I would have expected a generalized version of a Ricci flow, since there is also a dilaton field that flows (the authors put the B-field to zero by assumption). It should also be mentioned that this statement is only true to leading order in α', there would be corrections at higher orders. It should perhaps also be explained that this statement is true in the string frame, not the Einstein frame. This distinction is never made in the article.

   We agree that it should have been better explained that the RG flow has additional corrections, beyond the usual notion of Ricci flow, at higher order in α'. We have made some changes to make this clear.

   At leading order in α' it happens to be the case that there are no R terms in the dilaton beta function; hence at this order, it is actually consistent to ignore the dilaton when starting with a NLSM for which it is constant. As a result, the usual Ricci flow is valid at least when working at linear order in the angle deficit $\beta - 2\pi$. This is the case that matters near Eq. (60).

   It may however be the case that in other scenarios mentioned in our paper (e.g. the hypothesis that tachyon condensation is equivalent to some smoothed-cone NLSM background), a finite sized dilaton pulse is produced, in which case the RG flow would involve a dilaton field as well.

   We have added some discussion of these points in the text following Eq. (60), starting with the phrase "This statement holds" and proceeding until the end of the following complete paragraph.

7. Eq. (63): Please think about using another variable than β, as it can be very easy to mix up with the inverse temperature.

   Unfortunately, the use of beta is quite strongly associated with each of these quantities, we have tried to adapt to this by using a different font for each one. Fortunately most sections of the paper use just one of these in equations.

8. Eq. (65): I think the biggest issue with such a factorization is that once we take back reaction into account, the inner and outer regions cannot be defined anymore since the horizon itself fluctuates. Thus (65) can at best only hold approximately in the limit of weak string coupling.

   Thanks for this comment. While we agree that backreaction would be an issue at strong gravitational coupling, it is not immediately clear that this is a problem for Susskind-Uglum's open string picture, as they want to recover the Bekenstein-Hawking entropy even when the strings are weakly coupled, in which case one might hope that this backreaction can be neglected. Unfortunately, due to the other technical problems we have identified, it is far from clear that the open string calculation can really be completed at weak coupling.

9. The explanation before eq. (68) is in my view incorrect or misleading. The embedding of the worldsheet into Euclidean target space $X : \Sigma \longrightarrow \mathfrak{M}$ is always a continuous map (but not necessarily differentiable). The horizon $H \subset \mathfrak{M}$ is a closed subset in the Euclidean spacetime. Thus by simple topology $X^{-1}(H)$ is closed and hence compact (since Σ is compact). Since Σ intersects H generically in a number of points, $X^{-1}(H)$ consists of a number of points and is thus by compactness finite. Thus the worldsheet intersects the horizon actually only a finite number of times. Consequently, I also think that the following discussion about adding stiffness terms is moot.

   This topological argument is incorrect for 2 reasons. First of all, there are compact sets that are not finite, e.g. a Cantor set.

   More importantly, the initial premise that X is a well-defined map, though it has pedagogical value, is not literally true. This is because the worldsheet is really described by a 2d QFT, and in QFT it is false that the value of the field at a single point is a well-defined operator. Instead it is an operator-valued distribution and only has a well defined spectrum after smearing. Please see the discussion in Haag, Local Quantum Physics, section I.5.2 (p. 45). As the issue arises because of UV fluctuations of the worldsheet fields, it is potentially solvable by adding a UV cutoff such as the stiffness term proposed in our manuscript.

   (Incidentally the inconsistent use of m and n around eqs. (69) and (70) is also confusing to the reader.)

   Oops. We have attempted to replace all of them with n.

10. Section V.A: The argument that the sphere contribution in the on-shell approach to the orbifold replica trick vanishes is unconvincing to me. The authors point to their previous paper, section II.B for this. The argument their is a supergravity argument which shows that for smooth and compact

target spaces, the sphere partition function vanishes since the action is on-shell a total derivative. This argument does not apply to the orbifold, since the orbifold is neither smooth nor compact. As far as I am aware it is not known how to compute the sphere contribution and it is currently unknown whether it is zero or non-zero. The only reliable argument for a vanishing sphere partition function that I am aware of only works at $N = 1$, where at least for the superstring, spacetime supersymmetry requires a vanishing onshell action. I urge the authors to be more honest about the comparison. It is fair to say that the on-shell orbifold approach is not able to produce quantitative results for the sphere contribution at the moment.

Although the orbifold geometry is indeeed not smooth, its sphere level partition function Z (without tachyon condensation) can be quite easily calculated by thinking of the orbifold as implemented by a discrete gauge field. As the sphere is simply connected, the sole effect of this discrete gauge field is to divide the Z of the smooth theory by the order N of the gauge group. Thus, it is manifest that the action vanishes in this case. See footnote 36 of the revised manuscript.

(As in the case of non-orbifold geometries, this argument is valid up to a possible boundary term at infinity, but any such boundary term should be local, and hence linear in beta, and therefore must drop out of the conical entropy calculation.)

We believe that there has been a slight misunderstanding regarding our citation of section II.B of our 1st paper. Our intention was not to invoke Eq. (8), which was indeed derived in the smooth case, but rather the first paragraph of II.B which gives the worldsheet explanation for why the on-shell action vanishes. This argument does not depend on smoothness of the target space manifold.

11. The first sentence of the last paragraph on the left column of page 17 presumably has a typo and I don't understand what the authors want to say.

    This should read "of special significance".