Reply to reports

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We thank the referees for their careful reading of the manuscript, and useful comments and suggestions for improvements. Below is a reply to the two reports, with a list of the changes made to the manuscript.

1 Anonymous Report 1

1. It is correct that the flat limit process differs slightly in our manuscript and in [67,69]. This difference is due to a corner term. Let us explain where this term comes from.

   In our computation of the symplectic potential, we have started form the onset in the Bondi gauge, and obtained in the case of the BS gauge the generic expression (3.4). By construction, this symplectic potential admits a flat limit with no divergencies in Λ^{-1}. In this sense, there are therefore no hypothesis in our derivation of the symplectic structure and of its flat limit. We have simply evaluated the Einstein–Hilbert symplectic potential on-shell to obtain (3.4), which has a well-defined flat limit. When going to AdS, we have to rewrite the shear in terms of $\partial_u q_{AB}$, which introduces factors of Λ^{-1}, and we have found that (3.4) can be rewritten identically as (3.8). Starting from (3.8), one can therefore work our way back to (3.4), and the flat limit is well-defined.

   Now, in order to understand the difference with [67,69], one should note that in these references the starting point is e.g. (4.4) of [69]. From there, the authors work their way back to (4.7), which contains a divergent corner term in the flat limit. This corner term is precisely that contained in (3.8) of our manuscript. Up to the change of notations, (4.4) of [69] is therefore only the first term in (3.8) of our manuscript (appart from the corner term the other terms are irrelevant for the present discussion). Since we have shown that our (3.8) is identically equal to (3.4), which admits a flat limit, it is normal to observe that (4.4) of [69], which differs from our (3.8) by a corner term, does not lead to a well-defined flat limit. This is the origin of the extra corner term introduced by the authors of [67,69].

   In summary, this slight difference appears because the starting point of [67,69] is the Fefferman–Graham symplectic potential, which is then mapped to Bondi gauge via the diffeomorphism between the two gauges. Both procedures are however correct.

We have added a comment below (3.8) of our manuscript to point out this slight difference of treatment of the flat limit.

2. It is correct that (2.68) is not the Bondi mass loss formula in its usual form, since here it is written in terms of the covariant mass $\mathcal{M}$ instead of the Bondi mass M. The former has the property of being covariant under the action of the BMSW asymptotic symmetry group, while the latter satisfies the "true" mass-loss formula with a negative semi-definite news squared term on the rhs. We have added a paragraph below (2.68) to point this out and to explain the difference between the two formulas. It seems to us that, to some authors, the proper definition of the mass is still debated, precisely because of these two different properties.

2 Anonymous Report 2

1. It is correct that the Weyl rescalings in NU gauge were already revealed and studied in the work [127] by Barnich and Lambert. We had already mentioned this point in the first version of the manuscript at the beginning of section 4.1. But as the referee correctly points out, our phrasing about the relationship between Weyl and β_0 was a bit ambiguous/confusing in the introduction of the manuscript. The correct statement is that the Weyl rescalings are indeed always present in NU gauge (since in this gauge the determinant condition is dropped). However, in the usual analysis where $\beta = 0$ (i.e. with the algebraic NU gauge), the Weyl transformations h are time-independent and related to the time translations f as $h = -\partial_u f$. Since in this work we aim at relaxing completely the boundary metric and its time-dependency, we are looking for symmetry parameters which are arbitrary functions of u and x^A. What we initially meant to say is that the differential NU gauge is indeed necessary in order to access the *time-dependent* Weyl transformations. This can indeed be seen on equation (2.83b) or on (4.4), which show that setting β_0, or equivalently going to the algebraic NU gauge, does not allow for an independent symmetry generator h, but instead relates this latter to $\partial_u f$.

   We have added remarks and precisions are various locations in the manuscript to clarify this point, and to emphasize once again that, indeed, the Weyl transformations in NU gauge were revealed already in [127]. These are the locations of the changes:

   - end of the 1st paragraph of the part "Gauge choices" in section 1.2.
   - caption of table 1.
   - beginning of section 4.1.

2. We thank the referee for the references and interesting comments regarding structure at spatial infinity. We have added a final bullet point in section 5 "Perspectives" to discuss these important directions for future work.