The author now addresses comments raised in my previous report. I think the paper is now suitable for publication. However, I have additional comments that might be helpful in improving the presentation of the paper, but I leave it to the author to decide whether and how to implement these comments. I also think that new additions to the paper, including the equations for the potentials λ, ω, and the relation to Kaluza Klein reduction are very interesting.

1) Regarding the main issue, *i.e.* the ambiguity in the definition of the improvement vector, the author now imposes certain conditions that remove the ambiguity. The imposed conditions (eq.(60) for example) are rather technical and I wonder if one can obtain a better understanding of these conditions by using the Hodge decomposition for manifolds with boundary. Note that there is a natural inner product on the space of k-forms

$$\langle \omega, \eta \rangle \equiv \int \omega \wedge *\eta \tag{1}$$

Using this, one can unambiguously decompose a form into an exact and a co-closed part, *i.e.* (using $\delta \equiv *d*$)

$$\omega = d\phi + \psi \,, \qquad \delta\psi = 0 \,, \tag{2}$$

such that $\langle d\phi, \psi \rangle = 0$ and ψ at the boundary is transverse, *i.e.* the projection on the boundary acts trivially, and $\delta = *d*$. (see appendix C of [1] for a quick review and more details). In this language, it seems to me that the improvement 1-form is built such that it is orthogonal to exact forms w.r.t the product (1). I suggest that the author considers formulating the gauge condition in this language.

2) The author mentions in the abstract that the improved vector is well-defined "under a mild topological condition on the spacetime". This conveys the impression that this is the only assumption in the construction. However, there are additional assumptions, including certain form of asymptotic flatness, which is not guaranteed in the presence of non-localized matter fields. Also, the paper crucially relies on assuming the existence of an ACMC gauge. Therefore, I suggest that the author updates the abstract to highlight the implicit assumptions of the paper.

3) Regarding ref. [11]: My understanding is that the existence of a third set of multipoles in [11] is conjectured for generalized theories of gravity, not for GR plus matter fields. Therefore, I don't see how the analysis of this paper excludes that possibility. Moreover, even in GR plus matter, the existence of an improvement vector such that $\boldsymbol{\omega}_{\text{tot}} = d\omega$ only implies that spin

(or current) multipole moments are well defined. However, the improvement vector itself is constructed out of matter fields and may include information about multipole moments of additional fields in the theory. The opening of section 3.1.2 may be slightly improved.

4) I agree with other changes implemented in the paper.

References

[1] "Strolling along gravitational vacua," JHEP **01**, 184 (2020) doi:10.1007/JHEP01(2020)184 [arXiv:1904.12869 [hep-th]].