Strengths

1. This is one of the first one-loop calculations at dimension 8 in the SMEFT, and an important step towards improving the accuracy of SMEFT predictions.

2. The calculation is well-done and nicely documented. The authors keep track of several ingredients, from the full loop kinematics to the dependence on the lepton Yukawas.

3. The EFT analysis through the Dalitz plots is also important, especially in view of potential measurements of $h \to Z\gamma$ in the future.

Weaknesses

It seems a bit an overkill, to have such refined results with so many details and free parameters, for the most poorly measured Higgs process. The academic exercise of the calculation is probably more worthy at the moment.

Report

The paper reports on the calculation of the Higgs to $\ell\ell\gamma$ decay in the Standard Model Effective Field Theory, including subleading terms in the loop and SMEFT expansions, and allowing for off-shell photons and Z's mediating the fermion current.

This calculation is certainly a nice result, that marks a significant step in higher order SMEFT calculations, and it can meet the requirements for publication in Scipost. Before making this recommendation, I would like the authors to address the issues below. The most significant concern is about the consistency of the truncation of the final result (q. 13).

Requested changes

1. the corrections in C_{HX} to $\bar{g}_2$, $\bar{g}_Z$, $\bar{s}_W$ have to cancel off in observables, so I find eqs (3)-(5) confusing.

2. It is not clear where the input parameter dependence enters in the expressions for the results.

3. the $h\bar{f}f$ Yukawa coupling should receive a correction in δG_F because it depends explicitly on v. This is not mentioned around Eq (9) nor included in the Δ^R expressions in sec 4.2. Was this dependence accounted for?

4. Related to the above, the parameter c_{ll} in Eq (40) should be defined. I also do not understand the statement just below, that δG_F only appears multiplying dimension 6 coefficients. δG_F should also enter at LO in the SMEFT through input corrections to the SM parameters. Is it just a phrasing issue?

5. p5. can the authors be more specific as to when $\bar{m}$ coincides with the measured pole mass $\hat{m}$ and when it doesn't? And in the latter case, could the expression in terms of Wilson coefficients be specified or linked?

   For instance, the results in sec 4.1 are written as a function of $\bar{m}_\ell$. Does this stand for the expression in eq (9) or for the numerical value of the lepton mass? An analogous question can be asked about $\bar{g}_2, \bar{g}_Z, \bar{e}, s_{\bar{W}}$.

6. p5. The authors specify that retaining the lepton mass dependence results in sizeable corrections for the tree level SM decay. I guess they mean retaining lepton masses *in the integration of phase space*, as setting $m_\ell \to 0$ overall would just remove the decay. Is this correct?

7. p8. the nomenclature "box diagrams" in the text can be confused with box loop topologies. it would be best to specify in a few words what is meant.

8. from the discussion in sec 3.2 I understand all lepton masses and yukawas were neglected in the loop calculation. However, Eq (17) presents 3 different results for the 3 flavors. How does that happen?

9. In the definition of the fermionic operators (sec 4), the flavor structure is not specified. I assume the authors are simply taking it to be diagonal and universal, but without resorting to the insertion of spurion Yukawas?

10. What about dipole operators? They contribute for sure to Zll vertices, and probably also γll at dimension 8. If the yukawas are not inserted as flavor symmetry spurions, they should give unsuppressed contributions e.g. in the dim-6 amplitude squared.

11. p13. I don't see $\mathcal{M}_{C6}$ defined anywhere. I guess the numeration was changed and this is now $\mathcal{M}_{C5}$?

12. Do A_{11}, B_{11} enter any of the amplitudes? I see them defined in appendix B but then only A'_{11}, B'_{11} are indicated as contributing to $\mathcal{M}_{C3}$.

13. I am not fully convinced that the result is truncated consistently.

    The calculation follows a loop and a SMEFT expansion. In addition, the m_ℓ^2 suppression in the tree-level SM term is treated on the same level as a loop factor. Order by order the various contributions are classified as

	1	Λ^{-2}	Λ^{-4}
1	–	–	$\lvert A_0^6\rvert^2$
$((4\pi)^{-2}, m_\ell^2)$	–	$\lvert A_0^{SM,\star} A_0^6\rvert + \lvert A_1^{SM,\star} A_0^6\rvert$	$\lvert A_0^{SM,\star} A_0^8\rvert + \lvert A_1^{SM,\star} A_0^8\rvert + \lvert A_0^{6,\star} A_1^6\rvert$
$((4\pi)^{-2}, m_\ell^2)^2$	$\lvert A_0^{SM}\rvert^2 + \lvert A_1^{SM}\rvert^2$	$\lvert A_0^{SM*} A_1^6\rvert + \lvert A_1^{SM,\star} A_1^6\rvert$	$\lvert A_1^6\rvert^2 + \lvert A_1^{SM,\star} A_1^8\rvert + \lvert A_0^{SM*} A_1^8\rvert$

where A_L^d, is an amplitude with L-loops and d-dimensional operators and A_0^8 is understood to include tree-level diagrams with two $d = 6$ operators. $|A_0^{SM*} A_1^{SM}|$ is omitted because, as discussed in the manuscript, the different chirality of the lepton currents would add a further m_ℓ^2 power compared to $|\mathcal{M}_{0,1}^{SM}|^2$.

As far as I understand, the terms in red were not included in the calculation.

One could argue that the final result is not complete to order $((4\pi)^{-2}\Lambda^{-4})$, unless $|A_0^{6,\star} A_1^6|$ is retained alongside the dimension 8 contributions, or alternatively all dimension 8 terms are dropped.

On page 19 the authors justify this neglect by stating that "the contribution of the SMEFT at one loop and $\mathcal{O}(1/\Lambda^2)$ interfering with a tree level SMEFT amplitude would have a negligible effect compared to the tree-level squared SMEFT contributions". While I do agree with this statement ($|A_0^{6,\star} A_1^6|$ belongs one order higher in loops compared to $|A_0^6|^2$), the same reasoning implies that all dimension-8 contributions should be equally dropped. This contradiction does not seem to be resolved in any of the 3 counting scenarios proposed.

14. in eq (33) the term $|\mathcal{M}_{C1} + \mathcal{M}_{C2}|^2$ needs to be truncated at Λ^4 after squaring, correct? If that is the case, it would be worth indicating.

15. The cuts presented in table 3 are meaningful but not fully realistic. I wonder if the introduction of thresholds for the energy/p_T measurements, photon isolation requirements etc could spoil the main conclusions. For instance, the numerical results in eqs (13), (17) seem quite different from those reported in Refs. [8,9] (eg. $\Gamma_{h\to\mu\mu\gamma}$ is 0.47 keV here vs 0.27 in [8], and $\Gamma^{\text{tree}}_{h\to\mu\mu\gamma}$ the comparison is 0.101 vs 0.026), which is most likely due to a large sensitivity to the kinematic selections. Did the authors do any checks in this respect?

16. In order to claim that a certain set of cuts would enhance sensitivity to a certain class of operators, the overall normalizations should be considered as well, not only the Dalitz plots. That is, it can very well be that a certain phase space region contains most of contribution A and only 5% of contribution B, but if the latter is still 100 times larger than the former, one might never actually get to probe A. It would be useful if the authors could provide this information or at least qualitative indications.

17. some typos:

    - p3. derivate → derivative
    - p4. $SU(3)_L \to SU(2)_L$
    - p10. $c_{Hl}^{(8),\epsilon}$ is not defined. is this a typo?
    - p10. bee → be
    - eq (29) in the first line $\mathcal{M}_{C5}^{L,R} \to \mathcal{M}_{C4}^{L,R}$, and also in eq (28) $\mathcal{M}_{C4}^{L,R} \to \mathcal{M}_{C3}^{L,R}$
    - there are a few "the the"