Dear authors,

Thank you for the very interesting paper draft.

I find the results intriguing and very worthwhile publishing, but I list several concerns and remarks below.

With kind regards.

General comments:

In many places, I found the language somewhat vague and colloquial, bordering jargon. (As an example p9 3rd paragraph. "allowed shift ... at no cost in the likelihood"; first sentence Sec. 4.1, etc./)
Another example is the text in the outlook (Sec. 5), where the overall quality of the text is somewhat inhomogeneous and, in the last two paragraphs, would profit from a careful read.
A revised version would profit from a leaner and more crisp language.

I found the description of the construction of the likelihood (p9) too vague.
The general formula is given but there is insufficient detail on the concrete implementations of, e.g., the theory uncertainties.
For example, the range of i in Eq. 8 should be defined. The term "width measure" is not defined in the following text.
Lists of all the ingredients for each of the presented results are needed, for both, the top sector fit and the combination.

More detailed comments
1)
Table 2: There are several potentially important measurements not included.
If it is not too difficult, the results would probably benefit from the following.

(very new) total+differential tt/tt+jets in lepton+jets ATLAS
https://arxiv.org/abs/2406.19701

total+differential ttW ATLAS
https://arxiv.org/abs/2401.05299

total+differential ttW CMS
https://arxiv.org/abs/2208.06485

tt+gamma 13 TeV ATLAS
https://arxiv.org/abs/1812.01697
https://arxiv.org/abs/2403.09452

tt+gamma 13 TeV CMS
https://arxiv.org/abs/2107.01508
https://arxiv.org/abs/2201.07301

The ttbar spin correlation measurement provides tt(2l) unfolded cross sections that could constrain ctG (your Fig 10) better than the combination. This input, in particular, has the potential to significantly improve the global results.
https://arxiv.org/pdf/1907.03729

2)
p7/8, Fig. 1. I found the provided examples of the SMEFT sensitivity relatively short. It is not possible to show all possible variations, but a few more examples, possibly with other operators would be very interesting. Why not put all the inclusive cross-sections in a figure, one in each bin, and show how these change with SMEFT by overlaying a few selected parameter points?

3)
You emphasize correctly in the Appendix that the Monte-Carlo replica method has shortcomings, related, in particular, to the quadratic SMEFT terms. But in Eq. 12 you use the asymptotic Gaussian assumption for the Markov Chain. This seems to imply Wilk/Wald which could be violated by quadratic terms as discussed in arxiv:2207.01350. The Gaussian approximation could be invalid in this case. For example, Fig. 1 shows positive modifications for positive and negative values for CQd8, indicating that quadratics are dominant at the 3sigma level. It should be checked (or explained) why/how/if the quadratic terms invalidate the statistical theorems.
(I believe the tests in, e.g., Sec. 3.1 are linear in the POI and would are not conclusive in this regard.)

4)
I think Section 2.3 can be improved in terms of the quality of the text and the level of detail in the construction of the likelihood. In particular, p9 and the paragraph starting with "By ansatz..." can be significantly improved. It is not quite clear how theory uncertainties are correlated. Even though the treatment is heuristic, they should be correlated across bins/measurements for each process separately. There is too little

detail provided on how the theory uncertainties are obtained (which generator scale choice?).

5) (Section 3)
I have no substantial concerns with the studies of the public likelihood. However, the nuisance parameters appearing in the 2D distributions and the naming of the uncertainties
in the impact plots should be harmonized. The uncertainties should also be explained briefly in the text.

6) (Section 4.4)
This section should be improved and better integrated in the paper. Several operators appear only in the appendix. It emphasizes OtG, but the most sensitive measurements (see above) are not included. It appears the Higgs combination is a late addition to the draft and stands rather alone. As for the fit in the top sector, lists of operators, uncertainties, etc. should be added.

Minor comments
p2, 2nd paragraph: SMEFT is renormalizable, but not in the sense of the SM which requires a fixed and unchanged number of counter terms to all orders. Please clarify.

p2, 3rd paragraph. Many more references than [45] (HighTEA, with 3 references itself) would be needed or the sentence should be changed so that it actually refers to what is done in [45].

p3, Eq. 1: It looks odd to use the same index k (without bounds) to count two different sets of operators.
Moreover, the Weinberg operator (dim5) is not mentioned at all - it should be said that it is not relevant.

"Because the underlying symmetry structure is an input to the EFT construction" ... Do you want to say that SMEFT operators have well-defined CP properties and therefore you can remove the CP violating sector?

Is the "symmetry structure" the discrete SM symmetries? This paragraph should be made clearer in this regard.

Table 1, Eq.2 What about 2-quark-two-lepton operators? Those affect tt+multilepton final states and are not mentioned.

Eq. 6 \mp -> \pm should be more appropriate

Sec. 2.3 3rd paragraph: I do not understand the 2nd sentence. You do truncate the Lagrangian but also the LHC rate predictions at the quadratic level.

p9 3rd paragraph. "allowed shift ... at no cost in the likelihood" is unclear. I think you refer to an (additive or multiplicative?) nuisance parameter whose impact is inexpensive to compute.

Sec. 4.1 There are several unnecessary forward references in the text, e.g., in the first sentence.
The last two paragraphs (in particular the last sentence) in the outlook do not integrate well with the narrative of the rest of the text.