I start by outlining the major points that, in my view, require revision.

1. An analytic computation of the one-loop amplitude for $t\bar{t}j$ has already been presented in ref. [77]. The main novelty in this article is an optimized representation of the master integrals in terms of a set of algebraically independent functions called pentagon functions. As the authors emphasize, this representation is advantageous because it enables the analytic cancellation of the UV/IR ϵ-poles in the amplitude. While I understand that performing this subtraction is not essential to validate the result—since the comparison with OpenLoops provides a more stringent check—omitting it undermines the significance of the proposed representation. In other words, the authors claim that their representation enables the analytic cancellation of poles, but do not prove it. I therefore encourage them to do so. This would also enhance the article's value by collecting universal ingredients required for validating the future two-loop results.

2. One of the stated objectives is "*offering insight into the algebraic complexity of the two-loop scattering amplitudes*". However, the complexity of the finite part of the two-loop amplitude is typically orders of magnitude higher than that of the $\mathcal{O}(\epsilon^2)$ terms at one-loop. Moreover, no information on the algebraic complexity of the obtained results is provided, hence readers gain no insight into this aspect. Nonetheless, one-loop $\mathcal{O}(\epsilon^2)$ results can provide valuable information in view of a two-loop computation. For instance, the authors could comment on the effectiveness of their reconstruction strategy in reducing the complexity (the authors don't even say in what variable they perform the univariate partial fractioning, and does it work equally well for the leading and subleading colour terms?). It would also be informative to compare the complexity of the amplitudes for the two processes, and identify any potential relations between them that might be leveraged at two loops. Therefore, I recommend that the authors include quantitative information on the algebraic complexity of their results and provide at least some insight in view of a future two-loop computation.

3. The numerical evaluation routine presented in this paper appears to have a limitation:

   > This setup can be used for obtaining solutions of the DEs for any target point connected to the boundary point through a path which entirely lies within the s_{34} channel.

   I cannot find any explanation for this restriction in the manuscript. Moreover, the authors provide no information on how serious the limitation is: how much of the s_{34} channel can be reached starting from the four phase-space points where the authors computed the master integrals? If there were sub-regions that cannot be reached this way, how would one access them? These questions are particularly relevant given the stated goal of exploring the complexity of the two-loop computation. In fact, while at one loop a better approach is available (as the authors note at the end of sec. 3), this will most likely be infeasible at two loops.

Additionally, the presentation requires minor clarifications in several points, which I enumerate below.

4. In the abstract, the authors should clarify that the "*pentagon functions*" appearing in the last sentence are the "*algebraically independent components of the ϵ-expanded master integrals*" mentioned above in order for the abstract to be understandable to a broader audience.

5. In the abstract, the authors claim that the method they employed enables "*efficient numerical evaluation*", yet the article contains no information about the evaluation time. The authors must either substantiate this claim in the article or remove it.

6. In the introduction, the authors write that "*Theoretical predictions [...] are available up to next-to-leading order (NLO) accuracy, including both QCD and EW corrections*". In fact, some of

the cited references provide also a description of the off-shell effects. Only in eq. (6) of sec. 2 do the authors clarify that they treat the outgoing top quarks as on-shell. I believe that this approximation should be spelled out already in the introduction and not only in an equation.

7. The caption of fig. 1 states:

   > Black solid lines represent top quarks while black dashed lines represent massless external quarks.

   However, black solid lines are also used for internal top-quark lines, e.g. in the top-left Feynman diagram. This inconsistency should be clarified.

8. In sec. 2.1, the authors keep generic n_f and n_h, but these are set to numbers (presumably $n_f = 5$, $n_h = 1$) in the numerical benchmarks in sec. 4. This should be clarified.

9. In sec. 2.3, does the choice of reference momenta for the polarisation vectors below eq. (25) lead to a simplification? In other words, is this choice motivated or arbitrary? The authors should specify it.

10. The polarisation sum in eq. (29) implies the gauge condition $\varepsilon(p, q) \cdot q = 0$. This gauge choice—and possibly its motivation—should be stated in the text.

11. In sec. 3.1, what does "*standard permutation*" mean? I suppose this means the ordering of the external momenta given in fig. 2, but it should be clearly stated. Moreover, the authors use the notation $\{1, 2, 3, 4, 5\}$ for the standard permutation σ_0 but then switch to parentheses in eq. (43), e.g., $(3, 4, 5)$.

12. The notion of "*transcendental weight*" should be briefly introduced somewhere in sec. 3.1 or 3.2.

13. In sec. 3.1,

    > For constructing the alphabet across all the families and their permutations, we collected all the letters

    the verb "*collected*" appears to indicate that the authors started from a known set of letters. If that is true, it should be stated clearly together with the appropriate reference; otherwise, the authors should clarify how they constructed the letters.

14. In sec. 3.1, the authors write that they "*look for linear relations between them* [the letters] *in order to identify a minimal set.*" Since they define letters as the arguments $W_i(\vec{x})$ of the $\mathrm{d}\log$'s in eq. (45), the relevant linear relations are among the $\mathrm{d}\log$ of the letters rather than the letters themselves.

15. In sec. 3.2, "*a minimal set of independent components*" is ambiguous. The authors should specify whether they mean "algebraically" or "linearly" independent. The same holds below eq. (50).

16. In sec. 3.2, the sentence "*The latter usually leads [...] to the analytic subtraction of ϵ-poles*" is misleading. If the chosen MI components are algebraically independent, the ϵ-poles cancel out analytically against the appropriate UV/IR subtraction. Failure to do so would imply missed relations.

17. In sec. 3.2, concerning the use of AMFlow, the authors should specify what programs they interfaced it to for IBP generation and solution, and motivate the choice of 70-digit precision.

18. In sec. 3.4, what is the precision of the function values provided in the ancillary files? The same question should be answered for the benchmark values of the amplitude in sec. 4. Moreover, how where the four phase-space points chosen?

19. In sec. 3.2, the iterated integral with $\omega = 0$ starting the iteration in eq. (48) is undefined.

20. In eq. (48), the authors imply that $\gamma : [0, 1] \to \gamma(t)$ is a parametrisation of the path γ.

21. In eq. (53) and the text above, the authors appear to include $i\pi$ and ζ_4 in the list of possible transcendental constants. Firstly, $\zeta_4 \propto \zeta_2^2$, hence including both ζ_2 and ζ_4 in the ansatz would yield redundancies that their approach is meant to eliminate. Secondly, $i\pi$ is typically associated with branch cuts; since the MI-component relations under consideration must hold on any sheet of the functions, it would be surprising if they contained $i\pi$'s. Indeed, ref. [95] does not include them, and the authors themselves write that all MI components are expressed as polynomials in the pentagon functions and $\{\zeta_2, \zeta_3\}$ (ζ_4 should not appear, as I wrote above) above eq. (54). The authors should clarify the role of $i\pi$ in their construction.

22. In sec. 3.2, the sentence "*eq. (53) is set equal to eq. (47) at weight-two*" is unclear. I suppose that the authors mean that the RHSs of those equations should be equated.

23. In sec. 3.2, since the boundary values are known only numerically, the unknown coefficients $\tilde{c}$ and $\tilde{c}^i$ can only be determined numerically. However, from the ancillary files I see that they were rationalized. The authors should state that they rationalized the numerical values of these coefficients, and clarify how they checked that the rationalisation is correct.

24. In eq. (60), the sentence "*the normalisation matrix N is chosen such that the connection matrix $\tilde{A}'(\vec{x})$ is square-root free*" is ambiguous, since there is freedom in how to remove square roots. The authors should clarify.

25. In sec. 3.4, the expression "*The DEs system for the MIs*" in "*The DEs system for the MIs is significantly smaller*" is ambiguous, since several families and sets of MIs appear in this work. E.g., in sec. 3.1 they mention a set of 130 MIs that cover all families, including permutations. Is it the same set here? The authors should clarify what MIs they refer to.

26. The helicity amplitude in eq. (32) depends on the reference momenta n_i. How are they treated in the colour- and helicity-summed interference given in eq. (69)? Do they take explicit values and, if so, how are they chosen?

The following references ought to be revised:

27. Solving DEs for Feynman integrals through generalized series expansions has a long history, dating back to (at least) hep-ph/0505041 by Pozzorini and Remiddi (2006). The given reference [80] concerns only the multivariate extension of this approach.

28. In the introduction of sec. 3 the authors write:

    > We use the method of pentagon functions [37,62–64,68,95]

    The list of citations includes articles proposing new aspects of this method ([62–64,95]) as well as a few applications ([37,95]). On the one hand, this combination may confuse readers seeking to learn the method; on the other, the selection of applications appears arbitrary and may be perceived as unfair to relevant works that were omitted (e.g., 2312.16966, 2307.03098, 2410.19088, 2412.13876, 2502.14952). I encourage the authors to either review the literature or limit the references to studies that specifically develop the method itself.

29. At the end of sec. 3, refs. [63,64] are cited for one-fold integral representations. While these citations advanced this method, the approach was originally proposed by Caron-Huot and Henn in arXiv:1404.2922.

Finally, I spotted a number of potential typos:

1. sec. 1: "it enables us to constraint" → "it enables us to constrain";
2. sec. 1: "the efficient computation [...], has been enabled" → "the efficient computation [...] has been enabled" ;
3. sec. 1: "known in the literature as pentagon-functions" → "known in the literature as pentagon functions";
4. sec. 1: "they can only be used of a calculation" → "they can only be used for calculating";
5. fig. 1: "wave lines" → "wavy lines";
6. sec. 2.1: "adding the tree level amplitude" → "adding the tree-level amplitude";
7. sec. 2.1: "The full UV-renormalised amplitude" → "The fully UV-renormalised amplitude";
8. sec. 2.2: "Similar to the $t\bar{t}j$ amplitudes" → "Similarly to the $t\bar{t}j$ amplitudes";
9. sec. 2.2: "charges of the top-quark" → "charges of the top quark";
10. sec. 2.3: "t' Hooft" → "'t Hooft";
11. sec. 2.3: "while for those involving" → "while those involving";
12. sec. 2.3: "with q being the massless reference" → "with q_i being the massless reference";
13. sec. 3.2: "which consequently involve Laurent expansion" → "which consequently involves Laurent expansion";
14. sec. 3.3: "the momenta are required to be real and thus associated with physical scattering angles and positive energies" → remove "thus" because the positivity of energies does not follow from the reality of the momenta;
15. eq. (50) contains a number of typos: w should be ω, and ω' should equal ω;
16. sec. 4: "interefered" → "interfered".

In conclusion, I believe that the topic addressed in this article is well suited for SciPost, but the treatment has several shortcomings. I will be happy to recommend the publication of a revised version that addresses the above points of criticism.