Response Letter to the Referees

Lisong Chen, Ayres Freitas

April 11, 2023

Preface

First, we thank all referees for their detailed review and critical assessment of our work. In this letter, we respond to all the questions and comments the three referees gave in order.

1 Anonymous Report 1

1.1 Comment 1

- The formulation in Section 2 relies on the literature for the corresponding calculations for LEP 1. The due credit to the original papers should be given.

  Could the referee explain in more detail what literature this comment is referring to? We have cited the original papers for the complex-pole scheme before eq.(6), Refs.[31-34], and the references for the loop corrections at various orders are given in section 4, in the bullet list after "GRIFFIN version 1.0 contains ..."

1.2 Comment 2

- The treatment of logarithms of $\log(1-\frac{s}{s_0})$ in GRIFFIN should be described in some detail.

  We added a new passage below Eq.12 that explains the origin of the $\log(1-\frac{s}{s_0})$.

1.3 Comment 3

- The code contains only the IR-finite part of the NLO corrections. The precise definition of this part should be given, for instance for the vertex form factors.

  We added a more detailed definition of the factors Z_{if} in eq.(16) and the text just before and after this equation.

1.4 Comment 4

- The cancellation of IFI between γZ boxes and the corresponding real radiation mentioned on page 5 is only valid for inclusive quantities. This should be stressed.

  We thank the referee for pointing that out; this should have been stressed. We commented on this in our new manuscript.

1.5 Comment 5

- While it is obvious that Eq. (35) does not lead to double counting close to the resonance, this is not so clear away from the resonance. The authors should comment on this point.

  We have added a discussion of this aspect in footnote 5 of our new manuscript.

1.6 Comment 6

- The "etc." in the first list of items on page 9 should be explained or omitted.

  We have taken it out from our new manuscript.

1.7 Comment 7

- In the comparison between GRIFFIN and DIZET, the largest differences show up for quantities related to bottom quarks. This needs to be commented.

  We have added a comment in the paragraph starting with "Most predictions ..." on page 13.

1.8 Comment 8

- The differences between GRIFFIN and DIZET of up to 2% in the differential cross section away from the resonance region appear to be quite large. Can these really be explained by NNLO effects? Which enhancements of NNLO corrections can cause differences of this size?

  In the regions where the discrepancy between GRIFFIN and DIZET is relatively large (above the resonance for $\cos\theta = -0.8$ and below the resonance for $\cos\theta = +0.8$), there is a strong cancellation between the s-channel photon and Z exchange contributions, which makes the tree-level matrix element small. Thus the *relative* corrections become large. In fact, the NLO corrections reach 20–30% in those regions, so a 1–2% NNLO effect would be perfectly consistent with expectations from the perturbative series. We have added a comment in the text to explain this.

2 Anonymous Report 2

2.1 Comment 1

- The title, abstract, and introduction give the impression that GRIFFIN may be applicable to cross sections, while it only computes IR-subtracted $2 \to 2$ matrix elements. This should be stated in a more explicit way, starting from the abstract. **We thank the referee for pointing it out. We have modified our abstract, introduction and summary accordingly.**

2.2 Comment 2

- The treatment of logarithms of $1-s/s_0$ in GRIFFIN should be described in some details.

  We added a new passage below Eq.12 that explains the origin of the $\log(1-\frac{s}{s_0})$.

2.3 Comment 3

- The implemented IR-factorization prescription should be documented in full detail, i.e. with explicit formulas for all factorized singularities. This information is crucial for interfacing the code to Monte Carlo generators.

  We added a more detailed definition of the factors Z_{if} above eq.(16). Since they are derived from physical processes (within QED+QCD), it is not necessary to document their singularity structure. When these virtual correction factors are combined with the corresponding real emission contributions, the sum is manifestly IR-finite, and thus this combination can be self-consistently computed in any Monte-Carlo program, irrespective of the regularization or IR subtraction scheme used in that program.

2.4 Comment 4

- The IR factorization scheme should also be adapted to dimensional regularisation (with massless photons and massive or massless fermions)

  We kindly ask the referee to refer to the response to comment 2.3.

2.5 Comment 5

- The matching prescription (35) involves off-shell matrix elements with real Z-boson masses. The authors should comment on its applicability to modern off-shell calculations based on the complex-mass scheme.

  We thank the referee for this suggestion. We have added a discussion in footnote 5 in section 3. Also, we would like to point out that the Z-boson mass used in the off-resonant matrix elements is the real part of the complex pole s_0 instead of PDG mass, so this prescription is compatible with the complex-mass scheme up to the given order of perturbation theory.

2.6 Comment 6

- The terminology "cross section matrix elements" at the beginning of Sect. 4 is confusing and should be clarified.

  We thank the referee for pointing this out. We have corrected it in the manuscript.

2.7 Comment 7

- Table 1: the authors should clearly indicate which parameters play the role of user-provided input parameters and which ones are derived from other input parameters. For instance, gauge-boson widths are not independent input parameters: how are they treated in GRIFFIN?

In the context of Table 1, all parameters listed here are input parameters. The gauge-boson widths are needed as inputs for the translation of the PDG masses to the complex-pole masses. See Eq.(6).

2.8 Comment 8

- 8) The origin of the percent-level differences in Fig. 1 should be clarified in some detail.

We kindly ask the referee to see the response given for Comment 1.8.

3 Anonymous Report 3

3.1 Comment 1

- first paragraph: Measurements of DY at the LHC and the Tevatron also include initial state b-quarks, whose contributions play a role a the precision provided by the LHC experiments. Of course, their contribution at higher-order EW differs from the other light quarks due to the top, but they should at least be mentioned here.

We thank the referee for pointing this out. We have modified our first paragraph accordingly.

3.2 Comment 2

- first paragraph: Along the examples for QED corrections in MC for e+e- a few more recent advances should be mentioned, in particular 1911.12040 and 2203.10948, possibly referring to 2203.12557 for an overview.

We apologize for missing those relevant references. The original intention was to mention those MC tools linked to DIZET (in comparison with ZFITTER.). We have added a footnote to acknowledge other relevant MC programs properly.

3.3 Comment 3-6

- Elaborations on the notations.

We thank the referee for pointing this out. We have clarified them in the manuscript.

3.4 Comment 7

- eq. (6): Possibly connect eq. (6) to the otherwise well-known conversion between pole and on-shell scheme, which I think are the same just using different terminology.

We understood the referee's suggestion. However, since this passage is not in the context of the discussion of renormalization, we decided not to confuse readers by introducing more terminologies. The fact that M_Z, Γ_Z defined there are the real and imaginary parts of the pole s_0 (instead of the PDG masses) has been clarified in the passage above Eq.6.

3.5 Comment 8

- It is not entirely clear how logarithms of $1 - s/s_0$ are handled in the expansion of eq. (35) and following, please elaborate.

  We added a new passage below Eq.12 that explains the origin of the $\log(1 - \frac{s}{s_0})$.

3.6 Comment 9

- In the matching of the on-shell resummation to the complete off-shell fixed-order calculation, it is not obvious that no artifacts appear away from the on-shell limit as the resummation is never switched off. Please comment.

  We have added a discussion of this aspect in footnote 5 of our new manuscript.

3.7 Comment 10

- Further, also in the limit that the off-shell calculation is in the vicinity of the on-shell production, since the pole is never reached by the off-shell calculation due to the finite width, it is not clear either that no artifacts are introduced as in this limit the expanded resummation and the fixed-order do not (logarithmically) coincide (at least when a modern universal scheme like the complex mass scheme is used).

  Please see our response to the previous comment. We are not sure what logarithmic contributions the referee is referring to.

3.8 Comment 11

- The details on the implementation of the IR subtraction are insuffient, in particular if the code is to be used and interfaced to other tools and event generators. In particular, with the limited information provided a conversion to modern subtraction schemes using dimensional regularisation is not straightforward.

  We kindly ask the referee to see our response to comment 2.3, where the similar concern has been addressed.

3.9 Comment 12

- In the comparison to Dizet, the observed differences are not sufficiently explained in text, in particular in processes with bottom quarks. Please elaborate. In particular since the deviations of up to 2% naively seem too large to be attributed to generic NNLO effects. If they are enhanced by some mechanism, please discuss these.

  We kindly ask the referee to see our response to comments 1.7,1.8, where similar concerns have be addressed. And please see the updates in our new manuscript.