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Referee report

The authors have addressed all the points in the previous report, and I appreciate that my suggested changes have been implemented. Nevertheless, there is still disagreement between us that may not be resolved here. Despite my responses to the authors below, there is no point in delaying the publication of the draft, so I recommend publication without further review.

Regarding at what range of z the perturbation theory is still valid, the authors countered my point by arguing that: 1) perturbative corrections remain small ($\lesssim 0.1$) for all range of ν considered; 2) the fitted higher-twist terms are also small; 3) the actual physical scale in the matching kernel is $(1-u)z$, which makes it possible to go to larger value of z without hitting the Landau pole.

The problem of Argument 1) is that the absolute size of $O(\alpha_s)$ correction in the ν or z space is not always a good indicator of perturbative convergence, as it is not obvious how they are translated to the x space. Naively, one would anticipate that the moderate x region is more stable, while the end-point regions are more sensitive to even the slight ~ 0.1 corrections in ν space. Besides, regarding whether resummation is necessary, there is a paradox following the authors' argument. If perturbative corrections are small, then one should expect that the fixed-order and resummed results are not much different. However, if we do a resummation, then soon the perturbation theory will break down as z becomes larger. Note that the resummmed matching formula can explicitly preserve the $\ln z^2$ evolution even with the higher-twist terms included. Why should we ditch it and use fixed-order formula for all the z considered?

One reason of the "smallness" of the perturbative correction, as more easily seen from the OPE, is that there is a cancelation of the MSbar Wilson coefficient C_n and the C_0 in the ratio scheme. Both fixed-order C_n and C_0 grow very fast in z^2, but their ratio is much milder. In the authors' matching formula, the matching kernel is equivalently obtained by doing a perturbative expansion of ratio in α_s. So here comes another question: if both C_n and C_0 deviates far away from 1.0, how can one justify the expansion of the ratio in α_s? If there were a way to justify it to all orders in perturbation theory, then resummation must be included.

In Argument 3), the authors raised an interesting point. In the OPE formula, this corresponds to a resummation scale z/k where $k \gg 1$. While I do not know the answer for the value of k, the bottom line is that i) it can describe the data at all z considered, ii) the resummed pertubative series converges well, iii) the choice of k should be universal, which means that it works for the other matrix elements or hadron states. Apart from these considerations, this point itself needs deeper understanding. The $\ln z^2$ corresponds to DGLAP evolution, whereas the $\ln(1-u)$ which is accompanied by $(1-u)$ in the denominator, is usually from the threshold logarithms. The threshold logarithms satisfy a different RG equation, and their resummation is different from the DGLAP logs. So without a further understanding of the resummations, one cannot use this point as a proof of convergence of perturbation series.

As for Argument 2), clearly the authors' findings are irrefutable. My understanding is that this is a necessary condition to show that perturbation theory still works, but not sufficient. My reason is related to the authors' counter arguments to my point 4 in the last report.

Note that the matrix element considered here is a function in ν and z^2, and the dependence on ν is analytical at $\nu = 0$ when z is finite. Therefore, what the authors are doing here is to describe the z^2 dependence of the matrix elements that distinguish them from a universal "twist-2" curve in ν that can be parameterized with orthonormal polynomial bases. At small z, one may ignore the higher-twist contributions and test the $\ln z^2$ evolution; in the large z region, however, the non-perturbative dependence on z^2 become important. It could well be that, given the precision of the data, either $\ln z^2$ evolution or some other model of z^2 dependence can describe the data points. In the authors' counter example, they compared fits with the same model with and without the $\ln z^2$ terms. This example may not be sufficient, as the $\ln z^2$ terms can be compensated by some novel z^2-dependent or higher-twist terms. It could even be that the data satisfy a model in z^2 that is totally different from the twist-expansion.

Since my previous and current reports are focused on the draft only, I think it is best to leave the discussion on the other method, large momentum effective theory, in a future context.

To summarize, I think the fundamental disagreement between the authors and me is that when applying perturbation theory to data, should we treat perturbation theory as a first-principles input, or, do we use data as an empirical proof for the theory or as a constraint of the uncertainties in the theory? This may sound philosophical, but in other areas I think the answer is unequivocally the former. In perturbation theory, the power corrections are always correlated with the uncertainties in the leading-power coefficient functions. Without an accurate understanding of the leading-power coefficient functions as a priori, the estimates of the power corrections are at best phenomenological. If one wants to make systematic improvement to such calculations, one cannot avoid considering all the mentioned issues.