
General:

1) You have many figures that include color. Unless you want to pay a lot of money to have them included with color in the printed version, you should include Figure XX (color online) for each Figure that includes color.

Ok, added color online everywhere a color appears.

2) Throughout the paper different use of units is made. For example, Q2, t, W is often used with c=1, while in other places Q2, t have units (GeV/c)2 . Make it consistent throughout.

We fixed it adding all the "c" factors needed.

3) If the current authors list should become the final "lead authorship" group, this would establish a 4-tier ranking. If I include the ranking within the Orsay authors, it is even a 5-tier ranking. Unfortunately, the previous PRL has already started this, which broke with the tradition from ALL previous DVCS paper to place the primary analyzer(s) first, and the 2nd tier authors are the core discussion group listed in alphabetic order. If anyone believes one is getting more credit or recognition for being in place "#3" compared to place "#5" they are mistaken.

Please refer to Silvia Niccolai's email sent on Dec. 20.

Detailed comments:

Eqn.(1): Instead of defining only Q2, it would be better to define all kinematics quantities (Q2, ν, W, xB , ξ, t, p, p') in one block of equations before describing the hard scattering requirements. It becomes difficult to read if most of the definitions are squeezed into the text. It also avoids using quantities such as "t" before they are defined (eqn.(2)), also Fig.1 is referred to before quantities used in the figure are defined ξ, t. Also, in the notation in Eqn.(1) Q2 will be <0, but in the figures later it is quoted as Q2>0. I suggest to use the usual definition q2 = (e-e')2 and Q2 = - q2.

We rephrased that paragraph, making some of the definitions more explicit and discussing what the variables appearing in Fig. 1 represent right after introducing the figure. However, we didn't add there the definition of W, because it doesn't play a role in defining the kinematics of the DVCS process, that is the topic we are discussing there. We preferred to leave it when it is used for the first time (Section on exclusivity cuts).

Line 132-135: There is no reason to put down the Shifeng CLAS measurement as being of "limited statistics" and "non-dedicated" while the Hermes data with even less statistics, and came 4 years (!) later, are just referenced without qualification (as it should be). I consider *our 2006 data as pioneering results.*

We reorganized the presentation of the previous measurements.

Line 300: "DVCS events" should be replaced with "epγ events" or "single photon events"."

We disagree on this. In that paragraph, we are explaining how we extract the DVCS events from the general single-photon events, and introduce the pi0 subtraction procedure. We think that N has to be called DVCS events, because it is the number of events after pi0 subtraction. Please note that we sligthly modified the sentence following a suggestion by Dan Carman.

Line 410-413: There we argue that π0 were not included in the simulation to explain the discrepancy with the MC simulation for EC. However the next section starts with the headline "Exclusive π0 simulations". A referee may ask why were they not included in the simulations with the DVCS/BH events.

The aim of that paragraph is to explain how, from the DVCS simulated events, we define the DVCS exclusivity cuts. To that end, we rely on the pure-DVCS MC distributions, that must not include the pi0 contamination, because we use them to understand the shape of the DVCS distributions and to determine our cuts from that. The sentence you mention explains why there is a discrepancy with the EC data, that, as stated later, are highly contaminated from pi0.

Line 436: Why did the cut allow events with more than one photon if we include only single photons in the asymmetries? That seems to increase π0 contamination that then will have to be subtracted later.

The final state was selected requiring the detection of exactly
one good electron and one good proton, and at least one good photon. This choice was made in order not to loose potentially good events in which there was an extra accidental photon. It was tested redoing the full analyses requiring only one photon, and comparing the resulting final asymmetries with the ones obtained with the ``official'' selection: the results, which can be found in

http://www.lnf.infn.it/~pisanos/dvcs/prd/coll_wide_review/exaclty_1_photon/

show compatible asymmetries and a slight increase in the size of the statistical error bars when limiting the number of photons to 1, while the background hasn't decreased that much. Therefore, it was preferred to stick with the requirement to have at least one good photon.

Figure 12: The MM2 distribution clearly peaks closer to the (pion mass)2 (~0.018) than the photon mass (0). This should be mentioned and discussed as it already shows that π0's dominate the "epγ" sample in EC. This is still true after all the exclusivity cuts. [In line 436 it is discussed that event were selected with "at least one photon". The question I have is, do the exclusivity cuts include a single photon only constraint

or not, and if not, why not?]

For the question about the number of photons, please refer to the answer above.
As to the first part of your question, we added in the text a comment about the clear contamination from pi0 that moves the mm2ep distribution towards 0.018.

Line 713 and Fig.17. The carbon (red) data show a shift towards more positive values of ALU, which may indicate a bias in the carbon data. Two data sets to be comparable at the 3sigma level does not indicate good agreement. The more relevant comparison is to compare the leading twist contributions, i.e. sinφ moments. I suggest to fit the two data sets with an offset fit parameter, i.e. AUL= a + bsinφ, and compare the b values for both data sets. My guess is that they would agree better than the 3sigma quoted for the comparison of the unfitted data sets.

Please refer to Silvia Pisano's email sent on Dec. 22. That section has been highly reorganized in the new version of the paper.

Line 863-866 and Table IV: The table compares errors for the asymmetries BSA, TSA, DSA. The quotation of relative errors is not a useful way of showing asymmetry uncertainties as the asymmetry is already a relative quantity. The absolute asymmetry errors should be given, e.g. AUL ± ΔAUL . The consequence of showing relative percentage is demonstrated in lines 863-866, where one has to explain why the quoted (relative) errors are so different for the different asymmetries while the absolute errors are basically the same.

We agree with you and we have decided to change the table presenting absolute systematic errors. Now the table clearly shows the similar TSA and BSA unicertainties and the fact that DSA has a bigger systematic uncertainty. We edited the table and the text accordingly in the new version of the paper.

Line 949, 996, 1049: These definitions should be given in equation mode with an identifying number, not squeezed in between the text. Also, these definitions are repeated in the captions of Fig.18, Fig. 20, Fig. 23. If they are properly defined with numbers, they should be referenced as such.

We reorganized the equations and the captions as you suggested.

Line 1147-1152: The qualitative conclusion about “scaling” is insufficient. If we want to conclude something about scaling, we should make it quantitative. The data should be analyzed with a straight line a + bQ2 at fixed xB. The uncertainty in b would be the relevant quantity to conclude about “scaling” behavior. Since we have only two points in Q2 at fixed values of xB a fit is not required, just simple algebra. I suggest to do this exercise.

See the following wiki page: https://clasweb.jlab.org/rungroups/eg1-dvcs/wiki/index.php/CFF_evolution,_for_PRD

Conclusion section: One should include something about improved analysis once the new cross section and beam asymmetry data are available. Also, the 12GeV DVCS program should be referred to.

We added a paragraph in the end of the conclusion section.