CLAS ANALYSIS NOTE REVIEW
DEEPLY VIRTUAL PRODUCTION OF THE ρ MESON ON THE PROTON
A. FRADI, M. GUIDAL AND S. NICOLAI

VALERY KUBAROVSKY

Page 7, Line 4

The neutron detection efficiency of CLAS is less then 10%. The value 50% is unrealistic.

Page 9 The e1-dvcs group has different Cherenkov counter fiducial cuts. It is based on the calculation of the average number of photoelectrons in the Cherenkov counter detector plane (in bins of a 2-dim space). It takes into account all inefficiencies of the detector at the edges as well as in the center line ($\phi = 0$). See the analysis note of I. Bedlinsky and Hyon-Suk Jo.

Looking to the Fig. 2.5 we can see that efficiency near the boundary drops. It means that the cut is too wide. We are not sure that MC program will reproduce this distribution.

There are no explanations how you got the formulas for the fiducial cuts (Eq. 2.3-2.5).

Page 10, last line

Pions are not minimum ionization particles. Pions have 50% chance to interact with the material of the EC calorimeter and create hadronic shower. Hadronic calorimeters work on this principle. We can call MIP only muons.

Page 11, line 2

The ionization loses are usually measures in units MeV/g, not MeV/cm. These loses are approximately equal to 2 MeV/g.

Page 17, Eq. 2.15

You don't need 7 parameters to describe almost independent on momentum function $\mu(p)$. The difference between constant and you function is much less than $\sigma(p)$.

Page 18, Eq. 2.17, 2.18

How do you choose these values?

Page 22, Eq. 2.20, 2.21, 2.22

The latest electron momentum corrections are in

`https://clasweb.jlab.org/rungroups/e1-dvcs/wiki/index.php/Corrections_for_electron_and_proton_from_GSIM`

What corrections do you apply for the electron momentum?

Page 29

The justification of the "side band" subtraction method is very week. Figs. 2.24 and 2.25 contain plots that obviously have nonlinear background. More over the authors presented only integrated distributions. We need to see the distributions in bins (Q2,xB,t), (Q2,xB,ϕ).

Page 29

It is not clear what is the phase space distribution that was used for the simulation of the background. Is it for the reaction $ep \to en\pi^+\pi^0$ or for the reaction $\gamma^* p \to n\pi^+\pi^0$? Do you use your own event generator or "genev" generator for this study?

The phase space distribution for the reaction $ep \to en\pi^+\pi^0$ is not a good approximation for the background description.

Page 36

We don't agree that authors obtain good description of the background under the neutron peak. Take a look at the Fig. 2.32. The black curve and magenta curve are very different near the neutron peak. In our view the contamination under the neutron peak may be significantly more than quoted at this page (6%, 9% and 15%). Take a look fro example to the Fig. 2.27 the first and second columns, Fig. 2.28 the first columns, Fig. 2.29 the first columns. In EC-EC case, $x_B = 0.3$, the background is at the level of 80%.

We don't understand why this background was not subtracted. It is the only exclusivity cut in this study. Any background under the neutron peak will change the cross section. It cannot be completely absorbed in the background under the ρ^+-meson peak. You will get contamination from the reaction $ep \to en\rho^+\pi^0$ or $ep \to ep\rho^+\pi^-$.

The systematic error study of this background subtraction is absent in the analysis note.

Page 36

Misprint $MM(\pi^+\pi^-) \to IM(\pi^+\pi^-)$ in two places.

Page 41

After intensive study of the e1-dvcs beam energy dvcs group decided to use the value of the beam energy $E_{Beam} = 5.75$ GeV. Why did you decide to use $E_B = 5.776$ GeV? Please present new arguments or reference to your study of this important parameter of the experiment.

Page 41, the last bullet

As you pointed out the ρ^+ is at rest in the helicity frame. So you can not use it's momentum as the quantization axis. You wrote that you are using ρ^+ momentum in the center of mass frame of $\gamma^* p$-system. To define the quantization axis you need some momentum in the helicity frame.

Fig. 2.33 and 2.34

$MI(\pi^+\pi^0 \to IM(\pi^+\pi^0)$

Fig. 2.35

The figure captures are unreadable.

Fig. 2.38

What is the enchantment at low $M_{\gamma\gamma}$ in MC distributions. You don't have it in data.

Fig. 2.41-43

We don't understand how do you normalize MC data. It looks like you normalized data and MC by a maximum number of the event in the neutron peak. So you did not take into account the background under the neutron.

Page 55

Did you try to adjust the event generator taking into account the results of your study of the exclusive ρ^+ production? You know σ_{total}, Q^2-dependence, W-dependence, t-dependence of the cross section. You can significantly improve the description of your reaction by MC program (Fig.2.44) that looks not very impressive for a moment.

Fig. 2.45

The figure captures are unreadable.

Page 60, second line after Eq.2.37

Misprint : te, must be the.

Page 60

You don't have cut on the "Hole factor". However you MC is not perfect so you cannot rely on the data where you have small "Hole factor". We think that at least 50% of the quoted cross section have to be based on the measured value of the cross section but not on the calculation from MC program. On page 55 you wrote that you should not expect to reproduce your experimental data with your simple generator. The estimation of the "Hole factor" is one of the examples when you have to have perfect MC. You have very complicated analysis with 7 variables and extremely complicated CLAS acceptance. The average acceptance in your case is well below 10%. What are your estimations of the systematic errors connected with the method to calculate cross section based on the implementation of the "Hole factor".

You have to apply the cut on the minimum value of the "Hole factor". It will reduce the number of points where you measure the cross section but increase the reliability of your results.

Page 64, just the line under Fig. 2.48

Misprint: chhose.

Section 2.3.4

You are using the event generator for the radiative correction calculations. These corrections depend on the reaction under study. You calculated the radiative corrections as a function of Q^2 and x_B only. In fact the corrections depend on other variables as well, for example on ϕ. These corrections will change your ϕ distribution and will affect the extraction of the structure functions. Do you have any idea what are the systematic errors in the calculation of radiative corrections?

Fig. 2.51

The Eq. 2.45 is the approximation. In fact this distribution is not Poissonian. The average number of the photoelectrons is different for the different areas of the Cherenkov counter. So the integrated distribution is the composition of the Poisson distributions with different N_{phe}. And it is not Poisson distribution.

It is not clear why the upper limit for the integration in the Eq. 2.46 is set to 200. You still have events with $N_{phe} > 200$.

Page 68

The CLAS electron trigger includes EC calorimeter in coincidence with CC counter. It means that you cannot estimate EC electron efficiency using Eq. 2.47. The trigger system has already preselected events with the EC calorimeter. So your estimation is biased by the selection of the events by the CLAS trigger.

Page 70. Section Bin volume correction

There is another method to calculate the bin volume correction. You don't need the sub-bins (100 in each bin of variables Q^2, x_B and t). You can play random MC events in your (Q^2,x_B,t) bin and calculate the probability to get events inside your cuts.

Page 73. 2.3.7 Total cross section

The background under the neutron peak was not taken into account in the calculation of the total cross section $\gamma^* p \to n\pi^+\pi^0$. These corrections are at least at the level of 10% or more depending on the kinematics.

Fig. 2.56

There is very suspicious point at this figure at Q^2=1.3 GeV^2 (top right panel). What is the reason for the cross section to go down at this point? Why is it so different from the neighbor x_b bins?

Taking a look to the Fig. 2.37 I realized that the average $< Q^2 >$ value is around 1.6 GeV not 1.3 GeV as it is quoted in Fig.2.62. The fiducial volume in $Q^2 - x_B$ plane is so small for this bin that it is very easy to get systematic error in the evaluation of the bin volume correction or bin migration effects. You have to change the $Q62 - x_B$ binning or exclude this point from the consideration in your analysis.

How do you calculate the average kinematics for your bins?

The same strange behavior of the cross section is observed in the Fig. 2.62 for the ρ^+.

Page 73, line 9 from the bottom

Do you use "genev" for the MC simulation? You call it non resonant 2-pion production...

Page 73, Section Background subtraction

You did not subtract the background under the neutron peak.

Page 75, line 1

The agreement between data and MC is reasonable but not perfect. The shape of the background directly affect the ρ^+signal extraction. The careful systematic error study has to be performed to understand the influence of the simplification of the MC program to the final result.

Eq. 2.52

There is no justification of the empirical factor that skews the Breit-Wigner. This factor changes the ρ signal a lot in the region of low W. Usually the ρ BW has a tail to the higher values of $M_{\pi^+\pi^0}$ due to the p-wave of the decay products. It is clear that we have to cut this tail near the kinematical limit (low W). The skew factor suppress the high mass tail (not completely at the kinematical limit btw) and makes the tail to low values of $M\pi^+\pi^-$ (Fig. 2.61).

Like a study of the systematic errors we may suggest to change the ρ shape in such a way that will cut the high mass region according to the 2-body phase space:

$$\frac{dN}{dM_{\pi^+\pi^-}} = BW(M_{\pi^+\pi^-})\frac{p^*(W \to nM_{\pi^+\pi^-})}{p^*(W \to n\rho^+)}$$

where $p^*(m)$ is the *neutron* or $\pi^+\pi^0$ momentum in the W-rest frame (it is just 2-body phase space).

Page 84

The transverse size of the nucleon is connected with the $b_\perp$ slope parameter not with the b=slope parameter:

$$\frac{d\sigma}{dp_\perp} = Ae^{-b_\perp p_\perp}$$

The connection between b and $b_\perp$ is

$$b_\perp = \frac{b}{1 - x_B}$$

Could you please plot $b_\perp$ as a function of W?

We noticed that you have 3 points with $W < 2$ GeV (Fig. 2.64). Do you apply cut on W in your analysis?

Fig. 2.66

The number of points that you are using in this fit is extremely small. If you will take into account that this distribution is symmetric you will end up with only 3 points with 3 parameters in the fit. We think that you have to increase the number of ϕ points for this particular distribution and study the systematic uncertainties connected with ϕ subdivisions.

Could you please provide for us the full covariance matrix of your fit to take a look to the correlation between parameters with so small number of experimental points.

Fig. 2.68

Again the number of points is very small taking into account that the acceptance strongly depends on this variable (Fig. 2.45). We want you to vary the number of points and study the systematic errors connected with binning.

Fig. 2.70-2.71

As you pointed out this distribution has to be symmetric. It is not. Obviously the background is higher than you anticipated. How will your result on the calculation of R be changed if you will use the data with $\theta_{HS} < 180^0$ only? You have to apply the correction factor due to the cut the half of the acceptance. This will give you the estimation of the systematic errors.

Eq. 2.65

There is no explanation why do you use this average number for the systematic error. The usual way to calculate the systematic errors with several uncorrelated sources is

$$(\delta^{syst}_{exclu})^2 = \Sigma(\delta^{syst}_i)^2$$

You made a great job and calculated the correlation between two parameters. In this case you have to take the maximum value among all your systematic errors. Really, let us imagine for a moment that you don't have systematics connected with one of the parameters and systematics is determined by the other parameter only. If we will follow your prescription we will get systematics twice less than it is.

Fig 2.76

The plot is very busy. Could you please plot all distributions at the separate graphs.

Page 96

The calculation of the cross sections (Fig. 2.77) includes the "Hole factor". Did you recalculate this factor for every MC model you tested?

We have the same question for your study of dependence of the cross section on binning (Fig. 2.79).

Page 101

Can we take a look to the reduced cross sections for these 3 binnings?

Page 101, line 7 from the bottom

The calculation of the systematic error due to the formula 2.65 is not proved (see questions about Eq. 2.65). In our view you have to take the maximum value of the calculated systematic uncertainties.

The background subtraction has the biggest systematic error in comparison with other sources. So we suggest to investigate it in more detail and add one more ρ^+ shape suggested in the previous question.

Fig. 2.83

It is not clear how did you include different backgrounds into your fit. Could you please give more detailed explanation of this procedure.

Page 106,Table 2.9

The average systematic uncertainty for the background subtraction looks too small (see Fig. 2.82).

Fig. E.2, E.3, F.1 F.2, F.3, F.4, F.5, G.1, G.2, G.3, G.4, G.5

The ρ^+ signal is almost invisible in the kinematic point ($Q^2 = 2.7\ GeV, x_B = 0.56$). It is hard to believe that we can extract number of ρ^+ mesons for this point. It is more safe to remove this pint from the publication. Or you have to present the extensive systematic error study for this bin.