Ms. Ref. No.: NPB-D-14-00058R1
Title: Measurement of azimuthal hadron asymmetries in semi-inclusive deep inelastic scattering off unpolarised nucleons
Nuclear Physics B

We thank again the reviewer for the comments and the interesting discussion.

We agree with most of the comments and propose corresponding changes with the exception of the considerations concerning "previous pint 2)". As explained in the answer to that specific point, we agree with some of his/her considerations in case of a hypothetical experiment, but not in the case of COMPASS and of this specific analysis.

Answers to reviewer's comments, May 21, 2014

Reviewer #1: *The authors have addressed most of the referee's previous concerns. There are only a few points left from the first round of comments for which some additional clarification seems in place. In addition I list a few more corrections that were missed before.*

starting from the last (and "easy") part

Some more details to the manuscript:
a) page 4, 3rd last line of section 2: insert "to", i.e., "has led to the final results"
Done
b) Figure 8: is the figure for positive or for negative hadrons (not yet mentioned in the caption)?
Done:
Both figures 6 and 8 refer to positive hadrons for consistency. Of course the results for negative hadrons are similar.
We added the information in the figure captions.
c) page 13, first and second line of main text: (as the authors like to refrain from using c=1), better to use c also in the units for the CEBAF and HERA beams
Done
d) page 15 before conclusion: is it just a visual effect or are most of the h+h- differences seen in the 1d projection are mainly present in the high-pT region while almost gone in the remaining phase space? If so, mention it here as well?
If we exclude the 4 bins with p_T > 0.64 GeV/c, z>0.55 and x<0.02, we cannot see a strong p_T dependence. We would say that there is an indication for smaller differences at the lowest p_T and z values, but this is clear in fig. 11 too.
No change
e) Conclusions: first line — add "polarized" before "160 GeV/c muons"?
Done
f) Conclusions: Why is nothing said about the sine moment. It was measured as well and mentioned in the abstract.
Done:
The third sentence now is replaced by the 2 following sentences:
"The amplitude of the sin(phi_h) modulation is positive and small with respect to the statistical uncertainties. The amplitudes of the cos(phi_h) and cos(2phi_h) clearly show non-zero values and

their dependencies over the kinematical variables turn out to be very strong and not easy to be described in the present phenomenological framework.”

g) Figs. 13-15: There appears to be a missing blank between the first and the second sentence in the caption.

Done

About the other points:

previous point 1) *from the answers I am not sure the referee's point got conveyed properly — I still believe that it is hard to judge whether (including systematics in the discussion) some of the observations like hadron-charge dependence of the asymmetries are justified. However, one can probably live with presenting all uncertainties in the tables only, though preferentially one would show them in the figure as well.*

No change

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previous point 2) *I am not yet fully satisfied with the answer given. Clearly, there is a large acceptance effect on the azimuthal distribution of hadrons, even larger than the physics ones in some regions of phase space (cf. Fig. 6 middle panel). The authors argue in the answer file that*
"The comparison of the projections of the 3d asymmetries with the 1d results is a further positive check. No cross-section model was used to evaluate the projections: we just performed the weighted (according to the measured number of events) average of the 3d asymmetries."
This weighted average results in an _internal_ consistency check. It does not tell so much about the effect of acceptance on the integrated results. Consider following simple example for illustration:
- the asymmetry exhibits only a dependence on the variable integrated over, and let's just take three bins with following physics asymmetries:

$A(x1) = 0$
$A(x2) = 0.5$
$A(x3) = 0$

- the acceptance in x is also strongly varying, e.g.,

$a(x1) = 0.1$
$a(x2) = 1.0$
$a(x3) = 0.1$

- the unpolarized cross section is flat in x

*First of all in the 3d extraction the correct asymmetries are reproduced, however, when averaged over the measured unpolarized cross section ("measured number of events" as described by the authors) the result [(0*0.1 + 0.5*1.0 + 0*0.1)/(0.1+1.0+0.1) = 0.5/1.2] will differ from the physics average of 0.5/3.0, e.g., a much larger average (i.e.,1d result) is measured than the physics would give, because in the experimental average the varying sensitivities to the various corners of phase space was not taken into account. To get the physics 1d projection, the 3d asymmetries have to be weighted with the physics cross section and not the measured yields (which are the cross section times acceptance). And the COMPASS acceptance in the kinematic variables is not flat as one can conclude from the shape of the kinematic distributions in Fig. 2.*

Above argumentation is just the reflection that the measured average is an average over acceptance times physics cross section and that in the averaging the acceptance does not cancel (or that the sum of ratios (e.g., asymmetries) is not the ratio of sums).

Similar arguments hold for the phi acceptance function: the average acceptance function in the 1d extraction involves integration of the detector acceptance over the cross section model in MC. That this dependence is not just an academic point becomes obvious from the fact that varying the cross-section model in the MC gives one of the largest contribution to the systematics uncertainties.

While with the latter study and systematics assignment that part of the referee's concern is more or less addressed, the averaging over the measured cross section does not appear to be discussed and considered at all. Admittedly, in absence of a realistic model for the asymmetry to be used in a Monte Carlo study for studying the acceptance (integration) effects, it is difficult to give a realistic estimate of the systematics attached to the 1d results. It could well be that they are small compared to other systematics but this is far from obvious to the reader. The pragmatic approach might thus be to just state that

"In view of a realistic model for the kinematic dependences of the three azimuthal asymmetries, the impact of the spectrometer acceptance on the partial integration of the asymmetries over phase space in the 1d projections is difficult to assess and would be highly model dependent. In that respect the 3-d asymmetries are preferable not only for the additional insight into correlated kinematic dependences but also because of the lesser impact of the detector acceptance on the asymmetry results."

or something similar. The alternative would be to use some of the models (all not so realistic as stated in the paper: they don't reproduce all the features observed) or construct a model based on the data to estimate the impact on the integrated asymmetries.

We thank the referee for the clarification on the previous point 2) which we misunderstood: our answer concerned the azimuthal acceptance only. On the contrary, we do not agree with the comments on the COMPASS acceptance and the statement proposed as “pragmatic approach”.

To better clarify the point in the paper

a. we have modified the last sentence in the first paragraph of section 7.1 which now reads:
   “As described in the previous section, the systematic point-to-point uncertainties are estimated to be as large as twice the statistical ones when including the uncertainty due to the Monte Carlo generators used to estimate the acceptance.”
b. we have modified the first sentence of section 7.2 as follows: “In order to investigate the observed dependencies on kinematic variables the azimuthal asymmetries have also been extracted binning simultaneously the data in bins of x, z and p h T (3d asymmetries). This would also reduce the possible residual impact of the overall detector acceptance on the 1d results which is included in the systematic uncertainty.”

The reasons of our proposal are the following:

1. all the considerations are based on an extreme example which is far from the real situation in COMPASS (we do not feel this is the appropriate place to start a general discussion on such a point, but in our opinion with a similar acceptance it would be questionable if a meaningful measurement could be performed at all)
2. as we wrote in the answer to previous point 2), the COMPASS acceptance is very good, as verified in several different analysis, thanks to the large number of trackers and to the fact that charged particles are detected down to the beam region which is equipped with many scintillating fiber hodoscopes capable to detect both the beam and the scattered particles. Moreover, for this specific analysis, we have selected a limited kinematic region just to be sure to have a good azimuthal acceptance and we could check that the phi-integrated acceptance has maximum bin-to-bin fluctuations of 15%.
   At this regard, we do not understand the sentence: “And the COMPASS acceptance in the kinematic variables is not flat as one can conclude from the shape of the kinematic

distributions in Fig. 2". Fig. 2 gives evidence of the opposite. The distributions of the kinematical variables shown there have just the shape which one expects from Lepto, as can be seen when comparing Fig. 2 with Fig. 2bis (see below), which shows the same distributions obtained from Lepto with the kinematic cuts given at page 6 (and no acceptance at all).

3. the quoted systematic uncertainty foe the 1d asymmetries have been evaluated as conservative upper limit and in our estimate they include the possible systematics due to the small variation of the acceptance in the integration range.

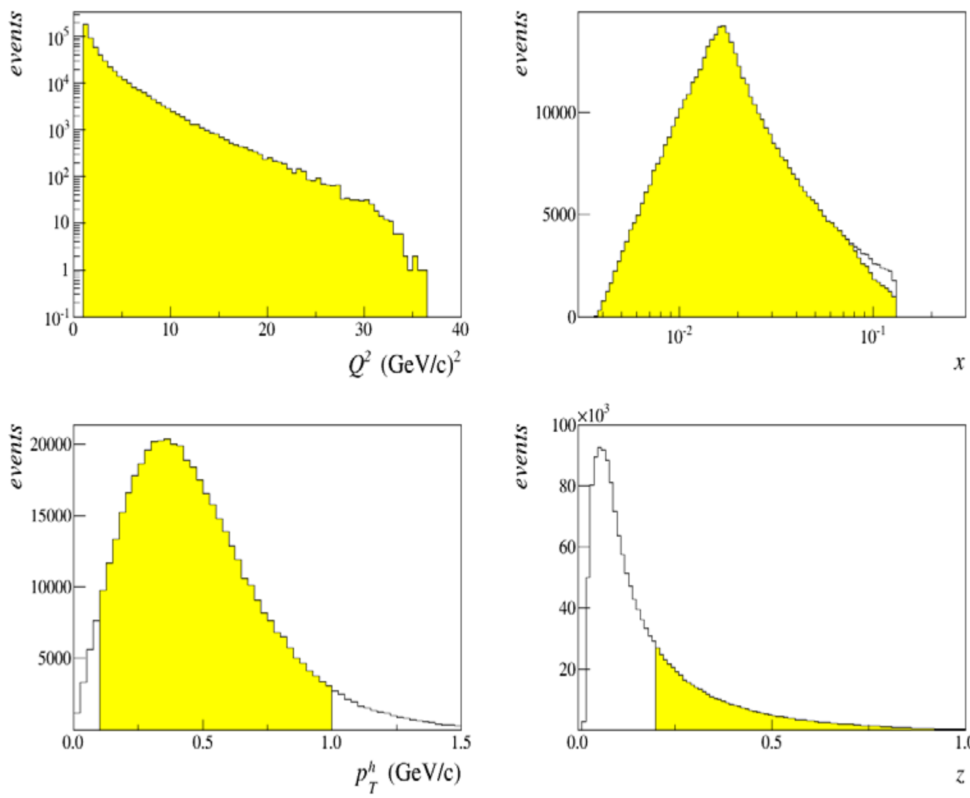

Fig. 2bis: Q^2, x, $p_T{}^h$ and z distributions from Lepto with the kinematic cuts given at page 6 of the paper. To be compared with Fig. 2 of the paper.

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previous point 13) *The fluctuations in a(phi_h) in Fig. 6 are still somewhat bewildering, especially as in the corresponding data those are not seen. The top figure should include the detector acceptance and the presumably smooth physics modulation. If indeed the "irregularities in the acceptance mostly reflect some non-uniformity in the spectrometer" [answer to point 13] then I would expect to see them also in the raw distribution (if those "irregularities" come from a realistic simulation of true detector effects). Is there the possibility that the statistical uncertainties are incorrect, e.g., obtained by "blindly" propagating the ones of the numerator and the denominator in (5), despite the fact that the latter are correlated?*

Apart from this, part of the answer to point 3 rose my attention: "We believe that the jumps are just statistical fluctuations. In fact, looking at the final fig. 11 (see comment above) obtained using the final higher statistics MC, the jumps seem to be reduced."

In general, the size of statistics-driven jumps should be correlated with the size of the statistical uncertainties. That they appear "better" when increasing MC statistics hints at a dependence on the MC statistics. Indeed, the MC simulation involved numbers of events similar to that of data. Have the MC statistical uncertainties been propagated to the final statistical uncertainties on the asymmetries extracted from data?

Stimulated by the comment, we checked how the plots in Fig. 6 were produced. In fact they were produced with one of the Monte Carlo data sets used to estimate systematics. Using the “default” Monte Carlo (the one used to get the final results, with higher statistics) the acceptance in our opinion looks smoother and it is compatible with the old one on the basis of a chi2 test. The comparison between “old” and “new” acceptance is shown in Fig. 6bis. Finally, we did not find any problem in the treatment of the statistical uncertainties.

We have updated Fig. 6.

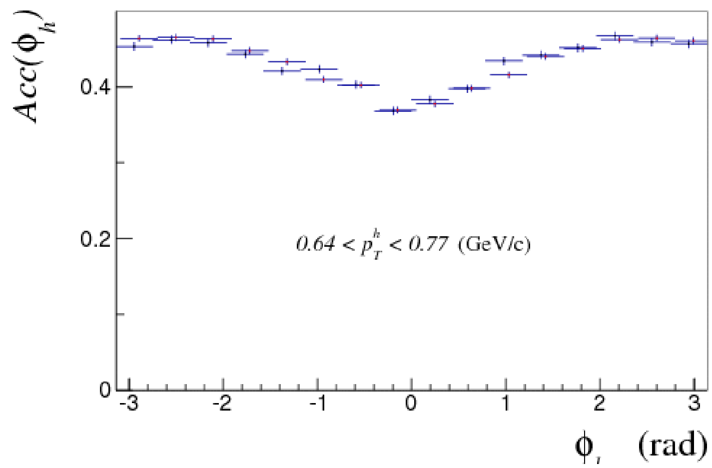

Fig. 6bis: comparison between the acceptance values of the central panel of Fig. 6 in the previous version (black points) and the acceptance values of the central panel of Fig. 6 in the present version (red points, slightly shifted to the right).

previous point 18) *I still don't get the point the authors are trying to make here. The manuscript reads "it is clear that the large negative values at small x in the 1d projection are mostly due to the hadrons with 0.55<z<0.85." The term "large" is subjective and relative. As the scale given for this measurement is the overall size of the asymmetries, the small x points for the cos\phi_h modulation does not appear to be large (it is small compared to the overall size of the cosine modulation, which is more on the order of 0.05). Maybe instead of "large" "clearly nonzero" or "non-vanishing" was meant?*

Furthermore, looking at the corresponding 3d results, yes — the large-z (low-pT) bins show the largest values for this cosine modulation. However, the uncertainties of those points are large(r) and many other z-pT bins have cosine moments of the same size as the one of the low-x point in the 1d projection, e.g., all of the high-pT bins have in the lowest x bin asymmetries of similar size as the one seen in the 1d projection for that x bin, but with much higher precision than the large-z bins. So which of those points dominates the final point in the 1d projection after averaging over z and pT seems not so obvious, and the statement as it stands right now appears more like speculation if not accompanied by more substantial arguments.

It is true though that the largest asymmetries for this modulation are observed in the high-z region, which certainly deserves to be stated.

Correct.

The proposed change did not improve the description of the results. We prefer the new formulation is better:

"From the results shown in Fig. 12 ~~an~~ interesting information on A_UU^cos phi_h can be obtained. Looking at the x dependence in the z and p_T^h bins, it is clear that the largest negative values at small x ~~in the 1d projection~~ are obtained for ~~mostly due to the~~ hadrons with 0.55 < z < 0.85, while for smaller z the asymmetries are either very small (0.1 GeV/c < p_T^h < 0.5 GeV/c) or indicate a different x dependence (p_T^h>0.5 GeV/c). Also, as can be seen in the figure, the absolute values of the asymmetries for z < 0.55 increase ~~somewhat~~ with p_T^h and the large and negative values at large z in the 1d projection are mainly due to the values at small x and p_T."