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Second Addendum to the HARP WhiteBook

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Abstract

This 'Second Addendum to the HARP WhiteBook' was prompted by the serious flaws in the first physics paper on 'large-angle' pion production that has been published by 'official' HARP, in defiance of strong criticism at various levels. The overall conclusion is that the cross-sections reported in this paper cannot be trusted.

1 Preamble

In December 2006, ‘official’ HARP (henceforth referred to as ‘authors’) circulated a draft of their first paper on ‘large-angle’ particle production for comment. In defiance of strong criticism, the paper was submitted for publication in February 2007. A revised version of this paper (henceforth referred to as ‘article’) has been accepted for publication in the European Physical Journal C [1]. The article reports double-differential cross-sections of π^+ and π^- in the momentum range 100 – 800 MeV/ c , produced by 3, 5, 8 and 12 GeV/ c protons on a 5% λ_{abs} Ta target.

Our ‘HARP Whitebook’ [2] was the reaction on the ‘official’ HARP Technical Paper [3] and held that the reported performances of the HARP large-angle detectors resulted from wrong analysis concepts and were unacceptably poor and/or wrong. Although instructed by CERN Management to refrain from publishing the HARP Technical Paper before completion of the work of the then inaugurated ‘Review Board for HARP’, ‘official’ HARP pressed forward with printing which, in turn, led to the publication of respective ‘Comments’ from our group [4].

Our ‘Addendum to the HARP WhiteBook’ [5] was the reaction on the ‘official’ HARP paper on RPC performance [6] and held that the content of this paper is plain wrong. Nevertheless, ‘official’ HARP pressed forward with printing which, in turn, led to the publication of respective ‘Comments’ from our group [7].

This ‘Second Addendum to the HARP WhiteBook’ complements a third ‘Comments’ paper that we submitted to the European Physical Journal C for publication [8], in response to the publication of the article.

While we focus in our third ‘Comments’ paper on the three dominant flaws of the article, namely a biased momentum scale, a momentum resolution that is much worse than claimed, and poor particle identification, we discuss here many further flaws that we identified in the article and in Refs. [3, 6, 9, 10] that underlie the article.

This paper also features an Appendix that shows that the authors’ Rebuttal [10] of our ‘Comments’ [7] on the ‘official’ HARP paper on RPC performance [6] lacks any substance. The same applies to the arguments made in the authors’ Rebuttal [9] of our ‘Comments’ [4] on the ‘official’ HARP Technical Paper [3].

2 Flaws in the TPC cluster position

2.1 Crosstalk

The crosstalk problem has been amply discussed already in the WhiteBook [2]. The authors claimed repeatedly that crosstalk was reproduced by Monte Carlo and eliminated by a correction algorithm. Our experience with crosstalk is that this claim cannot possibly be correct, and anyway proof of the correctness of the claim has never been given. We note that the article nowhere quotes the $r \cdot \phi$ resolution of TPC clusters which is for crosstalk correction the most sensitive measure of success.

We note that, accidentally, the two horizontal TPC sectors 2 and 5 are considerably more affected by crosstalk than the other sectors 1, 3, 4, and 6. The authors remain silent about this problem. Yet their claim of a correct momentum scale stems from elastic scattering on hydrogen where the scattered proton preferentially populates the two bad sectors 2 and 5 as explained in their Section 5.2.

The z position of clusters is calculated from the leading edge of clusters (Section 3: ‘*The reference time [of a cluster] is defined on the rising edge of the signal when the first pulse in a cluster goes over threshold*’). This is conceptually wrong¹. This concept causes a strong polar-angle dependence of the z position of clusters which typically comprise 5 to 15 samples of 100 ns length which is 25 to 75 mm in space (the charge of clusters are collected on pads that are radially 15 mm long, and therefore are collected over a longer time for smaller polar angles). The result is a systematic track shift along z that depends on the polar angle θ .

2.2 Drift velocity

The authors determined the drift velocity in the most unsafe region that can be found in the HARP TPC. They failed to take into account the variation of the drift velocity with the electric field which is strongly varying between the target and the stesalit endcap because of the accidental high-voltage misalignment. Their failure to correct for this invalidates their claim of an error of ‘*better than 0.5%*’ (Section 2.2). Rather, their drift velocity is biased by 1%–2% with respect to the major part of the TPC volume. As a result, the polar angle θ and with it the momentum is systematically biased.

2.3 Static and dynamic TPC track distortions

We note that the authors still ignore the *durchgriff* effect [11] which is an important element of static TPC track distortions, let alone the local field anomalies in the inner and outer field cages [12].

Eventually, the authors admit that dynamic distortions stem from ‘*the build-up of ion-charge density*’ (Section 2.2), and they admit—indirectly though—the very existence of the ‘margaritka effect’, all effects that our group claimed since long². The ‘margaritka effect’ is clearly visible in Fig. 4 through the non-convergence of positive and negative tracks in their d'_0 variable at the start of the spill in the 8 and 12 GeV/ c data. The reason for this feature is that they enforced by ‘*dedicated corrections*’³ in the 3 and 5 GeV/ c data an approximate convergence, however they faced later the fact that different data sets would need different ‘*dedicated corrections*’. This is phrased as ‘*The small mismatch extrapolated to $N_{\text{evt}} = 0$ visible in the 8 GeV/ c and 12 GeV/ c data are due to residual static distortions.*’, without explanation why static distortions should be different in different data sets.

‘*It was verified with the data that the value of [the polar angle] θ is not modified by the dynamic distortions*’ (Section 5.2). This statement is wrong. Both static and dynamic

¹This concept had been introduced to mitigate effects of crosstalk.

²We note the missing reference to our original work after having removed our names from the author list.

³M.G. Catanesi, statement in the closed SPSC meeting of 5 July 2005.

distortions introduce necessarily also a change of the radial position of clusters and hence change the polar angle θ and with it the momentum.

3 Flaws in the track fit

It has been discussed already in the WhiteBook [2] that the weight of TPC clusters in track fits tend to infinity when the azimuthal angle of clusters is close to 45, 135, 225 and 315 degrees. This mistake renders the p_T resolution worse and will bias the momentum measurement and charge assignment.

Also, it has been discussed already in the WhiteBook [2] that no p_T correction for the inhomogeneities of the solenoidal magnetic field in the forward direction is applied, which changes the p_T of forward tracks by up to 10%.

The authors state correctly that particles, especially slow protons, lose energy in materials before they enter the TPC, and therefore the beam point is no longer on the helical trajectory that is reconstructed in the TPC. Therefore, a fit using the beam point is necessarily biased. The bias is generally large for protons and small for pions. Although the authors' claim of a correct momentum scale is based on the fit of proton tracks, the authors use a fit with a beam point that is systematically shifted (their fit algorithm is based on a circle fit in the transverse projection which has no provision to fit a deviation from a circle). Therefore, fits of proton tracks with the beam point included are biased.

The first and the 20th pad rows suffer from systematic uncertainties that are much larger than the ones in the remaining pad rows. The authors remain silent about this problem which contributes to biases in track fits.

4 Flaws in the analysis concept

It has been discussed already in the WhiteBook [2] that the authors' pulseheight calibration of the TPC pads with 'super-events' has a bias that is specific for each data set.

The determination of the reconstruction efficiency is not trustworthy, because it is solely based on Monte Carlo simulation. Only trivial geometrical and absorption effects are considered. It is silently assumed that the pattern recognition program finds every track correctly and with full efficiency. Anybody who scanned a few hundred events knows that Monte Carlo simulation cannot reproduce adequately the pattern recognition efficiency.

The article's Fig. 10 (left panel) is perhaps typical for the actual disagreement between Monte Carlo and data. It demonstrates that the 'spokes' region is not understood which is not surprising in view of the the authors' lack of understanding track distortions. Rather than discussing possible causes of the disagreement and assessing them quantitatively, the authors' rationale is to ignore the problem.

We turn to the plot of the reconstruction efficiency as a function of the event number in the spill, Fig. 10 (right panel). It is supposed to prove that the 'efficiency' of finding a track

remains constant if the beam point is not used. If the beam point is used, the ‘efficiency’ drops considerably at some point during the spill. The begin of the drop is merely defined by the cut on the quality of the track fit, and hence can be put *ad libitum* anywhere. Apparently, it is put in such a way as to mislead the reader to believe that everything is in order until the 100th event in the spill, i.e., that no momentum bias has accumulated.

The azimuthal-angle position of the barrel RPCs is in the ‘official’ analysis rotated by 1° (7.3 mm in the $r\text{-}\phi$ coordinate) with respect to the HARP coordinate system. This rotation is wrong. When distortions are measured with respect to the TPC-external coordinate system provided by the RPCs, it fakes in one magnet polarity smaller TPC distortions but the effect will be doubled in the other magnet polarity⁴.

5 Contradictions and questionable assertions

5.1 Regarding the momentum scale

The claims of a correct momentum scale and a good momentum resolution stem from the authors’ ‘benchmark’ of elastically scattered protons from a hydrogen target. We pointed out already in Ref. [8] that the different types of dynamic distortions render impossible the application of conclusions from data taken with a hydrogen target to data taken with a Ta target. Here, we note further inconsistencies in the use of hydrogen data.

The momentum scale is defended with data where the proton track in the TPC is fitted without the beam point and where the proton momentum is predicted from the forward scattered track. But this is not the relevant momentum scale. The relevant momentum scale is the one of tracks fitted with the beam point which has in the presence of dynamic distortions completely different biases.

We conjecture that the reason for not stating explicitly the momentum scale from a fit of elastically scattered protons with the beam point included, is that the authors’ fit algorithm only deals with a circle in the transverse direction, i.e., the systematic deviation of the beam point because of energy loss in materials cannot be taken into account. The question then arises how the authors can measure the missing mass of the forward scattered particle from a fit of the proton in the TPC that includes the beam point—and hence is necessarily biased—that agrees with the known masses of the proton and the pion, respectively.

There is, however, an implicit statement on the momentum scale from a fit of elastically scattered protons with the beam point included. Figure 15 of the ‘official’ HARP Technical Paper [3] shows the distribution of the squared ‘missing mass’, i.e., the mass of the forward-going proton in elastic proton–proton scattering. This missing mass is derived from the measurement of the large-angle recoil proton in the TPC, whereby a fit including the beam point is used. No numerical result is given but the authors state: ‘...*the correct position of the missing mass peak are a valuable cross-check of the correctness of the calibration procedure*’. The very same distribution is shown in Fig. 6.2 of the thesis of S. Borghi [14]. There the pertinent text reads: ‘*The value of the mass peak of the proton...is found with excellent*

⁴The correct position of the RPCs were published by our group already in March 2005 [13].

accuracy $m_p^2 = 0.9147 \pm 0.0022 \text{ (GeV}/c)^2$...only the first 40 events in the spill are selected to avoid the effect of dynamic distortions’.

We note that the Particle Data Group value of the squared proton mass is $m_p^2 = 0.8804$, 15.6σ away from the result of ‘official’ HARP’s missing-mass analysis using the first 40 events in the spill only.

5.2 Regarding momentum resolution

We note that the statement on the proton momentum resolution being ‘dominated by the effect of energy loss and multiple scattering’ (Section 5.2) is not understandable. The data shown in the left panel of Fig. 13 refer to the momentum in the TPC gas, measured without the vertex constraint. For a proton with 400 MeV/ c momentum in the TPC gas one reads off a relative momentum resolution of 30%. The proton’s energy loss in the gas is less than 1 MeV. Its energy loss in the material before entering the TPC is less than 20 MeV. This energy loss has a statistical uncertainty of less than 15%, leading to a momentum uncertainty of less than 2%. The contribution from the polar angle variation due to multiple scattering is even smaller. It remains unclear how the momentum resolution without vertex constraint of 20% at 400 MeV/ c is increased to 30%.

We note the contradiction between the assertion in Section 2.2: ‘It [the developed method of pad equalization] is used to reduce the fluctuation of response between pads down to a 3% level’, and the claim of a p_T resolution of $\sim 30\%$ at 1 GeV/ c . The 3% precision claimed by the authors would lead to a considerably better p_T resolution and must in reality be worse by one order of magnitude.

5.3 Regarding the loss of data

The authors argue that their 70% loss of data does not matter. This contradicts earlier statements to the SPSC where it was argued that more running time to increase statistics was needed. It contradicts experience since there are always corners in the phase space of hadron production where one wants more statistics. It contradicts common sense since systematic errors can be reduced by learning from larger statistics. They contradict themselves: why have their systematic errors not been kept much smaller by restricting themselves to the first, say, 10% of the spill if statistics are not important? For example, Table 2 lists for the 8 GeV/ c data in the momentum range 100–300 MeV/ c a total systematic error of 11.1% while the statistical error is 1.6%. The largest contribution is from the momentum scale. It would have been only logical to trade off statistics against systematics.

5.4 Regarding the reliability of cross-sections

In view of differences of dynamic distortions in different data settings, and the dominant impact of (uncorrected) dynamic distortions on data quality, we wonder about the assertion in the paper’s abstract that ‘The measurement of these yields within a single experiment elimi-

nates most systematic errors in the comparison between rates at different beam momenta...'. On top of that, the authors contradict their own assertion: in Section 2.2 one reads 'The effect [of dynamic distortions] also increases with beam momentum; this is expected from the track multiplicity increase. Also the beam intensity was higher for higher beam momenta.; in the HARP Technical Paper [3] one reads in Section 5.1.7.6: 'Different runs or settings are affected in different ways, so that the net effect is sometimes large and at other times negligible'.

In view of the uncertainties with the positive and negative p_T scales, we wonder about the assertion in the paper's Abstract that *'The measurement of these yields within a single experiment eliminates most systematic errors in the comparison ... between positive and negative pion production'*.

The simultaneous correction for experimental resolution, energy-loss, trigger and reconstruction efficiency, acceptance, backgrounds, absorption and decays, and tertiary interactions, in a single 'unfolding correction' does not create confidence that things are done right. The method is totally untransparent. It depends on how well Monte Carlo agrees in many details with data. Our experience does not support the authors' optimistic view on the HARP Monte Carlo simulation.

Appendix

On the consistency of the ‘official’ performance of the HARP barrel RPC system

Introduction

The ‘official’ performance of the HARP barrel RPC system was reported in Ref. [6]. This paper was contested in Ref. [7] where it was claimed that the results reported in Ref. [6] contradict each other by a factor of 4.6. In their ‘Rebuttal’ [10], the authors of Ref. [6] insisted that their results are consistent and presented several arguments seemingly in favour of their point of view.

In this note we analyze the arguments of the Rebuttal and prove that they fall short of their aim by two orders of magnitude.

Formulation of the problem

In Ref. [6] the global time-of-flight (TOF) uncertainty $\sigma(t_{\text{TOF}})$ was expressed as

$$\sigma^2(t_{\text{TOF}}) = \sigma^2(t_{\text{RPC}}) + \sigma^2(t_{\text{beam}}) + \sigma^2(t_{\text{TPC}}), \quad (1)$$

where $\sigma(t_{\text{RPC}})$ is the uncertainty of the RPC time measurement, $\sigma(t_{\text{beam}})$ is the uncertainty of the arrival time of the beam particle at the target, and $\sigma(t_{\text{TPC}})$ is the uncertainty introduced by the TPC track reconstruction.

The value of $\sigma(t_{\text{TOF}})$ was reported in Ref. [6] as 305 ps. Below, we examine in detail the three contributions to Eq. (1).

Beam timing precision

The value of $\sigma(t_{\text{beam}})$ was reported in Ref. [6] as 70 ps. The same value was reported in Ref. [3].

RPC timing precision

The uncertainty of the RPC time measurement, $\sigma(t_{\text{RPC}})$, obviously includes the intrinsic RPC time resolution, which was reported in Ref. [6] as 141 ps. Additional contributions to $\sigma(t_{\text{RPC}})$ were quoted in Refs. [6] and [10]:

1. ‘*Residual common temperature variation*’ [6]. The variation of the RPC time with temperature was reported in Ref. [6] to be (49 ± 5) ps/°C. The day-night temperature variations are within $\pm 5^\circ\text{C}$ (see Fig. 10 in Ref. [6]). The 5 ps/°C uncertainty of the temperature slope is equivalent to a 25 ps variation of the RPC time (this is an overestimate: it assumes that the temperature takes only the extreme values). The contribution of residual temperature variation to the global TOF resolution, added in quadrature, changes the value of 305 ps by 0.4%.

2. ‘*Jitter of the TDC start signal*’ [6]. The same signal was used for the TDC start for the beam timing scintillators and for the RPCs. The signal jitter therefore cancels in the TOF measurement. The contribution of ‘jitter of the TDC start signal’ to the global TOF resolution is zero.
3. ‘*Different strip delays and strip transit times*’ [10]. The correction for this effect is described in Ref. [6] (Section III.B) where it is said that the correction was done following the procedure from Ref. [15]. There one reads: ‘*The estimated uncertainty due to a common correction on the impact strip number is $\sigma = 50$ ps*’. This contribution to the global TOF resolution, added in quadrature, changes the value of 305 ps by 1.3%.
4. ‘*t0 constants drift, electronics drift*’ [10]. This contribution is quoted in Ref. [6]: ‘*An estimate of the stability of the RPC calibration can be obtained... The results show ... $\sigma = 35$ ps*’. The 35 ps contribution to the global TOF resolution is small: added in quadrature it changes the value of 305 ps by 0.7%.

Adding in quadrature these contributions and the intrinsic RPC resolution (141 ps) one obtains

$$\sigma(t_{\text{RPC}}) = 156 \text{ ps} \quad (2)$$

Thus, the ‘*number of effects that deteriorate RPC resolution*’ [10] deteriorate the resolution quantitatively by 10%.

Uncertainty from the TPC track reconstruction

The particle TOF measured in the RPCs is compared with the TOF prediction from the measured track momentum. Therefore the uncertainty of the track reconstruction adds a contribution $\sigma(t_{\text{TPC}})$ to the global TOF resolution $\sigma(t_{\text{TOF}})$. The predicted TOF is the ratio of the track length L and the particle velocity v . The latter is calculated from the momentum measured in the TPC. Thus the uncertainty $\sigma(t_{\text{TPC}})$ has two contributions: from the TPC momentum resolution and from the uncertainty of the track length evaluation.

Uncertainty from the track length

The track length is measured between the vertex of the primary interaction and the point where the secondary particle hits the RPC. The contributions to the track length uncertainty, uncertainty of the primary vertex, uncertainty of the RPC impact point, uncertainty of the track curvature, and uncertainty of the track polar angle θ , are analyzed in turn:

1. The primary vertex position is measured by the beam MWPCs. The uncertainty σ_{VTX} of this measurement was reported in Ref. [3]: ‘*[MWPC] measure the beam particle position ... at the target with an accuracy of < 1 mm*’. This corresponds to the following relative uncertainty of the track length:

$$\Delta L/L = \sigma_{\text{VTX}}/R_{\text{RPC}}, \quad (3)$$

where $R_{\text{RPC}} = 428$ mm is the radius of the RPC cylinder. Thus, the uncertainty of the track length is $< 0.23\%$, independent of the track polar angle.

2. The azimuthal uncertainty of the track extrapolation to the RPC cylinder does not introduce by itself a track length uncertainty; however, it introduces an uncertainty on the radial position of the impact point (the RPCs form a polygon in the transverse projection rather than a circle). The radial position of an RPC varies by up to 3 mm for different azimuthal angles. Assuming that the track extrapolation to the RPC has an azimuthal resolution of 15 mm (an overestimate) one gets a radial uncertainty of less than 1 mm. The corresponding uncertainty of the track length is less than 0.23%, independent of the track polar angle.
3. The track has the shape of an arc rather than a straight line. The length of the arc is defined by the track radius. Assuming a 10% momentum resolution for $p_T = 200 \text{ MeV}/c$ one gets a 0.18% uncertainty of the arc length.
4. The arc length is determined in the transverse projection. The track length in space is obtained by dividing the arc length by $\sin \theta$. The uncertainty of the polar angle measurement is conveniently expressed in terms of $\Delta\lambda$, the uncertainty of the variable $1/\tan \theta$. The uncertainty of the track length is

$$\Delta L = L_{XY} \cdot \cos \theta \cdot \Delta\lambda \quad (4)$$

where L_{XY} is the track length in the transverse projection. The resolution $\Delta\lambda$ has been reported in Ref. [16] as $\Delta\lambda = 0.009$. The track length uncertainty changes from zero at $\theta = 90^\circ$ to 3.6 mm at $\theta = 20^\circ$, while the relative uncertainty $\Delta L/L$ reaches the maximum value 0.45% at 45° .

Adding in quadrature the contributions listed above, one gets the total uncertainty of the track length evaluation (for $\theta = 45^\circ$):

$$\Delta L/L = 0.58\%. \quad (5)$$

The relative uncertainty of the TOF prediction is numerically the same. The track length contribution to the TOF uncertainty ranges from 7 ps at $\theta = 90^\circ$ to 23 ps at $\theta = 20^\circ$.

Uncertainty from the momentum resolution

As derived in Ref. [5], the relation between the TPC momentum resolution $\sigma(1/p_T)$ and the corresponding contribution to the TOF uncertainty Δt reads as

$$\sigma(1/p_T) = \frac{c}{mR\sqrt{1-\beta^2}}\Delta t. \quad (6)$$

where m is the mass and β the relative velocity of the particle, and R the radius of the RPC cylinder. The time uncertainty depends on p_T rather than on total momentum and that there is no explicit dependence on the polar angle θ . The only simplification made in the formula (6) is the assumption that the tracks are straight lines. At $p_T = 200 \text{ MeV}/c$ this causes the calculations to be wrong by less than 1%.

For $p_T = 200 \text{ MeV}/c$, a momentum resolution of about 9% was reported in Ref. [3]. Using conservatively 10% one obtains the TOF uncertainty for pions as

$$\Delta t = 57 \text{ ps}. \quad (7)$$

Total track reconstruction uncertainty

Adding in quadrature the contributions from the track length and from the momentum resolution one obtains (for the worst case at $\theta = 20^\circ$):

$$\sigma(t_{\text{TPC}}) = 61 \text{ ps.} \quad (8)$$

Summary

Adding in quadrature all the contributions to $\sigma(t_{\text{TOF}})$ discussed above, i.e. $\sigma(t_{\text{TPC}})$, $\sigma(t_{\text{beam}})$ and $\sigma(t_{\text{RPC}})$, one obtains:

$$\sigma(t_{\text{TOF}}) = 181 \text{ ps} \quad (9)$$

This is in severe disagreement with 305 ps reported in Ref. [6].

The 305 ps resolution is reproduced if the momentum resolution is 44% rather than 10% (that is a discrepancy by a factor of 4.4, nearly the same as the factor 4.6 that was estimated in Ref. [7]).

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