



# HARP Collaboration

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## Third Addendum to the HARP WhiteBook

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V. Ammosov, A. Bolshakova, I. Boyko, G. Chelkov, D. Dedovitch, A. De Min, F. Dydak,  
A. Elagin, V. Gapienko, M. Gostkin, A. Guskov, V. Koreshev, Z. Kroumchtein, L. Linszen,  
Yu. Nefedov, K. Nikolaev, A. Semak, Yu. Sviridov, E. Usenko, J. Wotschack, V. Zaets,  
A. Zhemchugov

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### Abstract

In this ‘Third Addendum to the HARP WhiteBook’, we present a detailed rebuttal to the 27 arguments against our TPC calibration work that were put forward by the ‘HARP Collaboration’ in their ‘Comments on “The Harp Time Projection Chamber: characteristics and physics performance” by V. Ammosov et al.’, and show 26 of them to be wrong, or inappropriate, or without substance.

# 1 Preamble

Our paper ‘The HARP Time Projection Chamber: characteristics and physics performance’ [1] (henceforth referred to as ‘TPC paper’) was written in response to the HARP detector paper [2]. We, also members of the HARP Collaboration, had felt unable to sign the latter paper because we consider its discussion of the HARP TPC and RPCs erroneous and misleading. Hence we felt obliged to address the characteristics of the HARP TPC and RPCs ourselves, to analyse correctly the origin of problems, discuss the underlying detector physics, and describe the corrections that allow reliable and precise hadron production cross-sections to be extracted from HARP data.

The larger part of the HARP Collaboration that signed the HARP detector paper (henceforth referred to as ‘authors’) submitted ‘Comments’ on our paper for publication in Nucl. Instrum. and Meth. Phys. Res. A.

The ‘Comments’ demonstrate the authors’ continued determination to ignore scientific arguments, apparently in the hope that stubborn insistence will prevail. This attitude is all the more questionable as we showed in Ref. [3] and references cited therein that their argumentation in favour of the correctness of their analysis work rests on embarrassing mistakes, contradicts logics, is at variance with common detector physics knowledge, and is numerically inconsistent.

We hold that

1. their TPC  $p_T$  scale is systematically biased by  $\Delta(1/p_T) \sim 0.3 \text{ (GeV}/c)^{-1}$ ;
2. their TPC  $p_T$  resolution is by a factor of two worse than claimed; and
3. their discovery of the ‘500 ps effect’ in the HARP multi-gap RPCs is false.

As a consequence, cross-sections [4]–[6] based on the TPC and RPC calibrations reported by the authors are wrong by factors of up to two.

Our response to the authors’ ‘Comments’ is two-pronged. In Nucl. Instrum. and Meth. Phys. Res. A, we publish a short ‘Rebuttal’ alongside our TPC paper and the ‘Comments’. In this ‘Third Addendum to the HARP WhiteBook’ [7], we address all 27 arguments made in the ‘Comments’, one by one, and show 26 of them (the exception is comment No. 21) to be wrong, or inappropriate, or without substance.

Finally, a technical remark is in order: all authors’ references (text in italics) refer to the bibliography in their ‘Comments’ paper; all references in our replies (bold text) refer to the bibliography of this HARP memo.

## 2 Detailed rebuttal comments

1. *The paper “The Harp Time Projection Chamber: characteristics and physics performance” by V. Ammosov et al. describes a calibration of the HARP TPC system by*

*a group of physicists, formerly belonging to the HARP collaboration, and different of that already achieved and published by the collaboration [1-3], and used to obtain some other physics results [4-7].*

Our ‘HARP–CDP group’ (CDP stands for CERN–Dubna–Protvino) is part of the HARP Collaboration.

In October 2006 the HARP Collaboration Board declared, in a letter addressed to CERN Management, some of us ‘expelled from the HARP Collaboration’. This ‘expulsion’ had no basis in the HARP constitution, was neither accepted by any HARP funding agency, nor by CERN Management, nor by the CERN SPS and PS Programme Committee (SPSC), nor by us.

We disagree with the analysis methods applied, and physics results published, by the ‘HARP Collaboration’.

The ‘Comments’ are signed by the ‘HARP Collaboration’. Why do the authors hide behind this label and not take personal scientific responsibility for their ‘Comments’ by signing with their names?

- 2. In its biggest part the work described by Ammosov et al. is not original. We have published several papers [13] where all issues concerning the HARP TPC calibration and its physics performance have been addressed and properly understood. The results and techniques described in these earlier papers appear to have been used extensively by Ammosov et al., e.g. the track finding algorithm and the circular fit [1,8], the  $dE/dx$  calculation algorithm [2], etc.*

This comment attacks our professional ethics and it would have been better had it not been made.

We categorically reject this accusation which revives the earlier ‘plagiarism’ and ‘cannibalism’ offences committed by HARP Management. Our paper states clearly how our track finding and fitting works and it comprises all pertinent citations. Our respective algorithms have nothing, we repeat, nothing in common with algorithms used by the authors. The same holds for  $dE/dx$  and indeed for any other algorithm in our analysis work.

We had concluded already some three years ago that it was less time-consuming and more efficient to develop and code our own analysis algorithms from scratch, rather than tracing and eliminating the plethora of mistakes in the reconstruction and simulation algorithms of the authors such as documented in the HARP WhiteBook [7] and two of its Addenda [8, 9].

On a perhaps more humorous note: did the authors realize what they claim? That we copied their work and yet managed, according to them, to get everything wrong.

- 3. The rest of the calibration is based on a variety of theoretical and ad hoc models, often not well justified.*

We disagree with this assessment. We have gone a long way to understand the HARP TPC and RPCs in terms of established detector physics. For a

correct data analysis, it would have been sufficient to apply phenomenological corrections as long as they can be proven adequate in an independent way. We did not find this approach satisfactory and preferred to go beyond this. We developed physical models for our corrections which had two benefits: first, the parameters of the models were not *ad hoc*, had a physical meaning, and their numerical values had to be consistent with established detector physics; second: our physicists' curiosity was satisfied.

4. *A serious flaw of the paper as a whole is the lack of external constraints and cross checks: the same data are often used both to derive corrections and then to demonstrate how well the corrections have worked. In the approach of Ammosov et al. the HARP RPC system is used as an external detector to calibrate the TPC. However, first, the RPC system itself needed calibrating, and the TPC has already been used for this calibration [9].*

This assessment of our calibration algorithms suggests that the authors content themselves with repeating prejudices against our work, irrespective of what is clearly stated in our paper as the reader can easily verify.

Yes, we use the geometrical position of the RPCs, fixed once and for all time, to provide an external coordinate reference with a view to measuring objectively and effectively TPC track distortions. Specifically, we use the geometrical position of the so-called RPC overlaps. No prior TPC calibration is needed for this.

Yes, we use fully relativistic pions (i.e. their velocity is independent of the momentum) to calibrate the 'time zero' of the RPCs.

Yes, we make use of the theorem that the momentum of positive and negative pions with the same time of flight is the same.

These procedures are perfectly legitimate. We insist that there is nowhere in our calibration work any 'circularity' in the sense that, e.g., a track momentum would be used to determine the RPC timing, and then the RPC timing would be used to determine the track momentum. We insist that our corrections have been worked out entirely independently on the basis of external constraints, with no feedback whatsoever from results on hadron production cross-sections.

5. *Second, the effect of the dependence of the RPC timing measurement on the ionization energy losses, demonstrated in [10], has not been taken into account.*

We refer to our RPC publication [10] which discusses in exhaustive and quantitative detail the timing of protons and pions in the HARP RPCs. The authors keep insisting that they 'demonstrated' that protons have in the HARP RPC a time advance of 500 ps with respect to pions. Their claim contradicts our pertinent measurements, is inconsistent with the established understanding of RPC signal formation, and is inconsistent with the proton timing that is observed when not the proton momentum measured in the TPC but the proton momentum predicted from the kinematics of elastic scattering on free target protons is used. The detailed arguments are published in Refs. [3] and [11].

The ‘500 ps effect’ is a mere artefact stemming from the authors’ wrong momentum scale and wrong momentum resolution. While pions are nearly relativistic and hence their time of flight is insensitive to wrong momentum scale and resolution, protons are non-relativistic and hence their time of flight is sensitive to wrong momentum scale and resolution.

As for the dependence of the RPC timing measurement on the ionization energy loss: this subtle point is quantitatively discussed in our RPC publication [10]. There is no new detector physics involved. The proton signal is advanced with respect to the pion signal by

$$\Delta t = 45 \text{ ps} \cdot \ln \frac{dE/dx_{\text{proton}}}{dE/dx_{\text{pion}}},$$

not more, not less.

6. *In the paper by Ammosov et al. there is no justification of the magnitude of many of the systematic errors, which are simply quoted.*

**We disagree with this assessment. We believe that we motivate all quotations of errors of decisive quantities in such a way that our calibration work and its systematic errors can be appreciated by the reader.**

7. *Various corrections, e.g. dynamic distortions, depend on many data-taking parameters including the total ionization deposited in the chamber, i.e. on track multiplicity. All the results are shown for a low Z target (beryllium) while high Z (such as lead) data taken at high beam momentum (e.g. 12 GeV/c) are even more affected by the distortions. No evidence is shown that the advocated correction methods work also for them.*

**We disagree with this assessment. Our pertinent studies, described in comprehensive detail in four HARP memos [12]–[15], showed that track multiplicity is only one out of many ingredients that determine the charge density of Ar<sup>+</sup> ions in the TPC volume. Other, quantitatively more important, ingredients are the beam intensity (both absolute and relative, i.e. as a function of the time in the spill), beam muons, beam scraping, the amount of photon conversions, and spiralling low-momentum electrons. All this has been studied and properly taken into account, as clearly stated in our TPC paper. Needless to say many beam energies and targets were studied before we had sufficient confidence in our understanding of distortion effects and correction algorithms. What we present in our paper is, in our opinion, the right compromise between detail and the allowed length of the paper.**

8. *The paper does not even describe all the problems in operating the HARP TPC and their possible impact on physical performance.*

**We describe all problems with the operation of the HARP TPC correctly and exhaustively. We do not know which other problems the authors refer to.**

9. *The only really original content is the method of correcting for the dynamical distortions. It differs significantly from ours, described briefly in [3], and we have serious objections to it (see below).*

**We insist once more that all, we repeat, all work in our TPC paper is original unless explicitly quoted otherwise, not only the dynamic track distortion corrections. Specifically for the dynamic distortion corrections: we refer to our assessment of the respective work of the authors that we published in Ref. [3] and leave it to the reader to judge whether he considers our work correct or the work of the authors.**

10. *The track finding efficiency is evaluated by “extensive eye-scanning” with claimed precision of 1% ! Other experiments have shown how notoriously subjective scanning events by eye can be, and such a process nearly always introduces systematic biases. Moreover, the track finding efficiency depends on the azimuthal angle because of the particular geometry of the TPC pad plane. All this is measured with objective methods by us and extensively discussed in our paper [2].*

**This comment attacks our professional ethics and it would have been better had it not been made.**

**We quoted our reconstruction efficiency from extensive eye-scanning as  $(95 \pm 1)\%$  and hence state a 20% relative error of the inefficiency, while the authors’ comment purports that the relative error of the inefficiency is 1%.**

**One can argue forever about the virtues of eye scanning that is subjective, versus, e.g., a simulation that never describes data perfectly. We made a choice that we feel is appropriate. Our eye scan was independently done by four persons, the results agree with each other. The results of the eye-scan were of course binned in the variables  $p_T$  and polar angle  $\theta$ , the quotation of  $(95 \pm 1)\%$  merely gives the average size of the reconstruction efficiency. On top of that, we made several checks to convince ourselves that the result of the eye-scan makes sense.**

**If the reader could see displays of a hundred HARP events he would quickly get cured from the notion that, e.g., a ‘full GEANT simulation’ would adequately describe reality.**

11. *Maybe the most important comment regards the method of correcting for various distortions when evaluating track momentum and angle. The procedure is described in sections 4.6 and 5.2. It makes use of the RPC point, the “virtual” beam point (without specifying whether energy loss in the material in the center of the chamber was considered), or the closest point of approach to the TPC axis when exploiting through-going cosmic-muon tracks, and the curvature from the fit. Solving the circle equation gives the 3 parameters describing the circle. The difference between the new track and the original points gives the correction needed.*

**We can only conclude that the authors have not read our TPC paper. We refer to its Section 4.6 where the concept of the ‘virtual beam point’ is exhaustively and quantitatively discussed. We refer to Section 5.2 where the differences between the determination of static distortions and of dynamic**

distortions, the use of the closest point of approach to the TPC axis of through-going cosmic muons for static distortions, and the use of the beam point for dynamic distortions, as unbiased small-radius reference points, are discussed in detail.

For the benefit of the authors we try once more: the concept of the ‘virtual beam point’ (which of course means that the energy loss in materials is taken into account, otherwise the concept would be empty), is applied in every track fit that is used for the determination of cross-sections. The ‘virtual beam point’ algorithm is solely applied to secondary particles stemming from beam interactions in the target. It has nothing to do with cosmic muons.

For the determination of static distortions, the energy loss of cosmic muons in materials is not taken into account. For the determination of dynamic distortions, we state clearly in Section 5.2 that only particles with  $p_T$  larger than 200 MeV/c and with  $dE/dx$  lower than 1.5 m.i.p. are used. We invite the authors to convince themselves with a little calculation that under these circumstances the effect of the energy loss on the determination of static and dynamic distortions is negligible.

12. *For the cosmic tracks, Ammosov et al. assume the fitted curvature (momentum) to be the true one and then correct for the static distortions by moving the points. Indeed, for a cosmic muon track passing close to the TPC axis, the closest point of approach to the TPC axis is not biased (or at least, the bias is small), but all distortions are not canceled, and the reconstructed momentum from the overall fit (the two branches together) is less biased but not perfect.*

**We disagree with this assessment which is purely qualitative. We invite the authors to convince themselves with a little calculation that this comment is numerically irrelevant.**

13. *For physics tracks, they use the same method to correct for dynamic distortions, but state explicitly that the curvature is wrong, and an iterative procedure is used. This procedure is ill-defined, because with the same 3 inputs (2 points and a curvature) there is only one result, so it is not clear what the iterative procedure really is! In other words, one can have only one circle with fixed radius passing through two points. Apparently, the iterations adjust just the energy losses of the track before entering the TPC gas volume and move only the “virtual” beam point and the RPC point within its relatively large tolerance (16 mm, as stated in the paper). At this level, according to the authors, only these two points are used as reference (“external constraint”), while distorted cluster positions undergo corrections. Thus, there is no physical means to adjust the curvature, hence to evaluate the cluster distortions, in such a procedure.*

**We disagree with this assessment and wonder how the authors can draw such conclusions from Section 5.2 in our paper. We try once more. The beam point at small radius and the average RPC overlap point at large radius serve as fixed external reference points which never change during the iterative procedure. We explain in footnote 24 that because of the 16 mm width of the RPC overlap, distortions cannot be determined track-by-track, but only for an ensemble of tracks (which is perfectly adequate**

for the purpose). With the fixed inner and outer reference points, two of the circle parameters are fixed, only the circle radius is not yet determined. The only possible way to estimate the circle radius is from a circle fit of the distorted cluster positions. The initial fit of the circle will be biased as we clearly explain in our paper. But the biased fit is better than a random guess of the circle radius. The biased fit delivers biased dynamic track distortions which are compared with the predictions of the physical model of dynamic track distortions, and used to optimize the physical parameters of the model, which is done iteratively. After convergence, the circle radius has been determined in such a way that the  $r\cdot\phi$  corrections of a track comply with the physical model of the dynamic distortions.

We insist that this method is correct and bias-free. We are well aware that the procedure is correct only when the physical model of the dynamic distortions is perfectly correct; it is for this reason that we need to take into account very precise models of the ‘stalactite’ effect and of the dynamic ‘margaritka’ effect, respectively; approximate models would by far not be good enough.

14. *Moreover, the same method is used both to quantify and correct the distortions, and then to evaluate how well the distortions were corrected. An example is found in figures 15 and 16 where  $r\cdot\phi$  residuals are shown to assess dynamic distortions, but the residuals are computed by comparing to tracks corrected for dynamic distortions, and not to an external reference. It is obvious, that an independent tool is needed to evaluate if the distortions are, or are not, well corrected - as has already been demonstrated by the HARP collaboration using elastic scattering, for example in [3].*

We disagree with this assessment. As explained in item 13, the circle radius is not obtained after a more or less arbitrary correction of the distorted  $r\cdot\phi$  coordinates of tracks. Rather,  $r\cdot\phi$  corrections must comply with the physical model of the track distortions. This compliance breaks the degeneracy that is claimed erroneously by the authors.

We take the occasion to stress that our physical models of track distortions not only predict the  $r\cdot\phi$  corrections but also the radial corrections which are of course fully taken into account in our correction algorithms.

As for the claim of the authors that they calibrated with elastic scattering events: we showed in Ref. [3] in detail that their claims are wrong and explain succinctly what went wrong in their analysis.

15. *Without such an external reference, one should assign defensible systematic errors associated with the comparison, which has not been done.*

We disagree with this assessment. First, the claim of the lack of an external reference is wrong as explained in item 13. Second, we presented in our TPC paper exhaustively, and adequately, the quality of our track distortion corrections.

16. *The apparent agreement between start and end of the spill does not actually mean that both have been adjusted correctly. It may only demonstrate that a procedure has been*



*found to adjust different parts of the spill to match each other, but not necessarily with reality.*

**For once, we agree with this statement. However, we wonder whether this comment of the authors refers rather to their own work than to ours.**

**In our case, the constancy of track momentum with spill time is the result, we repeat, the result of our dynamic track distortion correction. The correction itself is based only on the fixed inner and outer reference points and the time-dependent parameters of the physical model of dynamic track distortions. If everything is done right, constancy of track momentum with time in the spill must result – and it does in our case.**

17. *A further evidence that the distortion correction procedure is ill-defined and even circular is the need to introduce an “apparent constant” azimuthal rotation of the RPCs by 2 mm depending on the magnetic field direction (Section 5.2). The conjecture that this is due to correlations between various static distortions appears unsupported. On equal footing, it could be a distortion introduced by an error in the correction, perhaps with a radial dependence such that it is largest at large radius - hence the apparent shift in the RPC positions. The statement that the “reconstructed physics quantities are unaffected” is not justified at all: a distortion could indeed change curvature and hence momentum.*

**The 2 mm azimuthal shift of the RPCs leads to a shift of the  $r \cdot \phi$  positions of clusters that is proportional to the radius, while the beam point is not affected. Therefore, the trajectories of all tracks are rotated by a small angle in azimuth which obviously leaves the track momentum unchanged.**

**We can assure the authors that no other distortion correction that we were able to think of produces a distortion that is compatible with being linear with radius and that attains 2 mm at the position of the RPCs.**

**The ‘apparent’ 2 mm azimuthal shift of the RPC overlaps is not a physical shift of the RPCs since it changes sign with the magnetic field. It is needed and its precise origin in terms of a physical process is not clear to us. We stress again that this does not matter: the understanding of the TPC distortions in terms of physical processes is a bonus, not an absolute must. What matters at the end of the day is that the effectively applied model of the distortion correction works. It does, as extensively discussed in our TPC paper.**

**Since the authors criticize our 2 mm ‘apparent’ shift of the RPC position: what about the wrong 7.3 mm shift of the RPC positions in the authors’ analysis?**

18. *The next symptom of an ill momentum determination procedure is the necessity of introducing further corrections to the measured momentum. As stated in Section 6.1, “the correct momentum scale was enforced by means of a small correction of the absolute value of the sagitta which was applied with opposite sign for positively and negatively charged particles. This sagitta correction [...] was determined with high precision from the requirement that  $\pi^+$  and  $\pi^-$  with the same time of flight have the same momentum.” We see here again a correction “by hand” with opposite sign for opposite magnetic field.*

*Of course,  $\pi^+$  and  $\pi^-$  with the same momentum and polar angle have the same time of flight. The delicate question here is to know whether one measures the same ToF for both charges. The RPC layout [11,9] introduces a rotational asymmetry, and therefore a potential asymmetry in handling particles of different charges. This is because the RPC pad preamplifiers are located on one side of the pads and the measured ToF depends on the distance between the preamplifier position and the impact point of the track at the RPC pad. Ammosov et al. correct for such time dependence [9]. Nevertheless, their correction does not fully eliminate the ToF dependence on the track charge because the RPC timing is more often given by a hit in the first gap, while the radius at which the distance to the preamplifier used for the correction is calculated corresponds to that of the middle of the stack. Thus, they could introduce a bias in momentum with the above mentioned “by hand” correction.*

**We disagree with this assessment. Even in the limiting case that the timing of the RPC hit is given by the first of the four gas gaps (for which there is no evidence whatsoever), the timing difference is of order 1 ps for typical RPC signals from pions and hence negligible. We invite the authors to convince themselves of this with a little calculation.**

**Since the authors criticize us for an effect that is smaller than 1 ps: what about the 60 ps timing difference between positive and negative pions observed in their RPC calibration [7]?**

19. *Section 6.2 pretends to demonstrate the correctness of the momentum scale by exploiting a comparison of the measured proton momentum with that derived from the measured ToF in the RPCs. Such a cross check is particularly doubtful. There is a strong correlation in the procedure with the TPC being used to calibrate the RPCs, which then check the TPC. It is assumed that there are no unexpected detector effects for the RPCs, though in fact the HARP collaboration has reported such an effect [10].*

**We disagree with this assessment. Our Fig. 20 is to show our good  $p_T$  resolution and not the momentum scale, as clearly said both in the title of Section 6.2 and in its text.**

**We nowhere say that Fig. 20 demonstrates that our momentum scale is correct. We recall that our momentum scale is defined through the strength of the solenoidal magnetic field, and through the equality of time of flight of positive and negative pions as measured by the RPCs. The small sagitta correction resulting from the latter, which we interpret as a correction for remanent electronics crosstalk, is applied also for protons. Apart from this small sagitta correction, no further proton momentum correction stemming from the time of flight of protons is applied.**

**Returning to the authors’ observation that the average of the entries in Fig. 20 is nearly zero (it is not supposed to be perfectly zero as discussed in our RPC publication [10]): this demonstrates the absence of the ‘500 ps effect’ in the HARP RPCs that is claimed by the authors.**

20. *Protons of 400 MeV/c have a time advance of 200 ps due to their higher energy loss in the RPC. This time shift corresponds to a 7% shift in  $p_T$  (for the angular range considered), much greater than the 1.6% shift shown in fig 20a.*

We disagree with this assessment.

Further to our pertinent discussion in item 5, we note the variation with time of the ‘500 ps effect’ that the authors claim to have discovered. This effect appears in the HARP Technical Paper [2] with the phrase (Section 5.2.3) ‘*The measured time-charge dependence for protons is shifted typically by about 500 ps towards shorter times due to their higher ionization rate, hence steeper rise of the pulse leading edge.*’ The effect is corroborated and at some length discussed in Refs. [17] and [18], though the 500 ps changed to 300 ps in Ref. [17] and to 400 ps in Ref. [18].

In two most recent papers, published almost concurrently, the authors claim that protons with a momentum of 400 MeV/ $c$  in the TPC gas have a time advance of 300 ps, see Fig. 12 (right panel) in Ref. [19], and of 500 ps, see Fig. 6 (right panel) in Ref. [16]. In their ‘Comments’, the very same effect is quoted as 200 ps.

We refer once more to our discussion in Ref. [3] and references cited therein, where it is shown that the ‘500 ps effect’ is an artefact and does not exist.

21. *Figure 21 does not add more confidence. It gives the difference in the predicted momentum of large angle recoil protons in elastic scattering events from a +3 GeV/ $c$  beam hitting a liquid hydrogen target with the same quantity derived from ToF measured by the RPCs. The plot is one-dimensional, no dependence of this difference on the momentum is given. The mean value of the fitted Gaussian can be always adjusted to zero by selection of a proper angular and/or momentum range. In addition, Ammosov et al. have not described so far their analysis of elastic scattering data. And it is very surprising that if such an analysis exists, they do not make a direct comparison of the predicted proton momentum with the one measured in the TPC rather than going through an intermediate comparison with RPC ToF measurement.*

This comment attacks our professional ethics and it would have been better had it not been made. We do not adjust data to obtain a convenient result.

As for Fig. 21 of our TPC paper: there is no momentum dependence of the agreement between measured and predicted inverse transverse momentum of the recoil protons.

However, we apologize for a mistake in the text concerning Fig. 21 in our TPC paper. There we say that  $1/p_T^{\text{meas}}$  is derived from the RPC time of flight under the proton hypothesis and that no reconstruction in the TPC is involved. This is wrong. The truth is that  $1/p_T^{\text{meas}}$  is determined from the reconstruction in the TPC.

Fig. 21 confirms our claim of an absolute momentum error of less than 2%, and of a resolution  $\sigma(1/p_T) \sim 0.20 \text{ (GeV}/c)^{-1}$ . Hence part of the authors’ comment is without substance but in this case the misunderstanding is our fault and not theirs.

22. *Moving on to Section 6.3 “ $dE/dx$  versus momentum” we mention that no quantitative evidence is shown to support the important claim that  $dE/dx$  confirms the momentum scale within 2%. Superimposing a curve on a twodimensional plot is only suggestive.*

*It appears unlikely that this can be substantiated given the poor momentum resolution and the lack of a minimum ionizing proton band. It is even not stated whether the line on fig. 22 (the “theoretical expectation”) represents mean or most probable value. Since this is the only “absolute” reference for the momentum scale in the paper, it is hard to see how a 2% precision can be quoted for this quantity.*

**This comment attacks our professional ethics and it would have been better had it not been made.**

**We never claimed that  $dE/dx$  is used to confirm the TPC momentum scale. We said that  $dE/dx$  is ‘in agreement’ or ‘consistent’ with the momentum scale, not more, not less. As for the claim of a 2% precision of the absolute momentum scale: we refer to our Section 6.1 where it is explained in detail that the absolute momentum scale rests on the equality of time of flight of positively and negatively charged pions, and on the strength of the solenoidal magnetic field. The confirmation of the momentum scale with elastically scattered protons is a confirmation of the absolute momentum scale, not more, not less.**

23. *The  $dE/dx$  cannot be used in HARP to estimate the momentum scale with such a precision because both the scale and offset calibrations of  $dE/dx$  are free parameters and the resolution in  $dE/dx$ , about 20%, is insufficient to achieve it.*

**Our  $dE/dx$  resolution is rather 16% than 20%. As explained in item 22, our absolute momentum scale was never based on, or defended with,  $dE/dx$ .**

24. *A few slices through fig. 22 for small bands of momentum, including the pion minimum and the proton band, superimposing the expected and measured  $dE/dx$  distributions would demonstrate this. A plot showing the stability versus angle would also be very helpful, as this would check for sagitta biases (as  $dE/dx$  depends on the total momentum, while the measured momentum comes from the  $pT$ ). Unfortunately, Ammosov et al. do not show such plots.*

**Our TPC paper reflects what we consider important and not necessarily what the authors would like to see. One can argue forever which plots should be shown in a paper within a limited length. We made our choice according to our priorities on what messages we want to get across to the reader.**

**Our  $dE/dx$  distribution shows a slight dependence on polar angle for which we corrected in our analysis. We did not include this minor effect in our TPC paper as it is, after the small correction, in practice irrelevant. For details we refer to our pertinent HARP memo [20].**

25. *We end our list of specific comments with a remark about figure 23. It shows the velocity of particles as determined by the time of flight from the RPCs against the momentum measured in the TPC. The authors pretend on “nearly perfect” agreement between theoretical curves and the data. Such an agreement is particularly doubtful for the proton line in view of the effect, demonstrated in our work [10]. As it has been already mentioned above, a strong experimental evidence is presented there that low momentum protons have a time advance of several tens to several hundreds of*

*picoseconds due to their higher energy loss in the RPC. Ammosov et al. do not believe in the existence of such an effect and do not correct for it but nevertheless they get “nearly perfect” agreement.*

**We repeat once more that the ‘500 ps effect’ does not exist, irrespective of how strongly the authors insist that they ‘demonstrated’ its existence.**

**In the absence of the 500 ps effect, one expects agreement and not disagreement between the momentum measured in the TPC and the momentum inferred from RPC time of flight. This is exactly what we observe. For the difference between ‘perfect’ agreement and ‘nearly perfect agreement’, we refer to our pertinent discussion in our RPC paper [10].**

26. *Another suspicious thing is that there are no positrons visible in Fig. 22a, while there are some in Fig. 22c. However, the latter represents a sub-sample of the events plotted in the former. We doubt that the only reason is the logarithmic scale of the latter.*

**This comment attacks our professional ethics and it would have been better had it not been made. We do not manipulate data.**

**Their comment refers to our Fig. 23, not to Fig. 22, and is anyway wrong. The colour legend of Fig. 23c shows that the positrons are lower by one order of magnitude with respect to the maximum in the scatter plot. The colour legend of Fig. 23a shows such entries with white colour, i.e., not at all.**

27. *In these Comments we have expressed serious doubts about the correctness of the method of the distortion corrections of the HARP TPC described in the paper of Ammosov et al. An ill-defined iteration procedure is applied that cannot lead to correct results. Several corrections are introduced “by hand” with the clear aim to shift the distributions to the “right places”. The claimed momentum resolution and absence of a bias in the momentum scale are not confirmed by physical benchmarks and remain unjustified.*

**We do not question anybody’s right to have serious doubts. What we question is the stubborn inability of the authors to face the fact that their physics analysis is seriously flawed. Rather than going back to the drawing board and re-think their physics analysis, they find it appropriate to put forward all sorts of arguments that—so they apparently hope—invalidate our work.**

**On the one hand, the authors never succeeded coming forward with a single quantitative argument that would prove a significant systematic error in our work. On the other hand, the authors ignore our many quantitative arguments as to why their own work is seriously flawed.**

**We leave the judgement to the reader.**

**It is sad to see physicists acting the way the authors do. It is sad to see how the authors insist on making sure that the originally good label ‘HARP Collaboration’ will go into history as a synonym for physics results that cannot be trusted.**

## References

- [1] V. Ammosov *et al.*, The HARP Time Projection Chamber: characteristics and physics performance, Preprint CERN-PH-EP/2007-030, accepted for publication in Nucl. Instrum. and Meth. Phys. Res. **A**
- [2] M.G. Catanesi *et al.*, Nucl. Instrum. and Meth. Phys. Res. **A** **571** (2007) 527
- [3] V. Ammosov *et al.*, Comments on TPC and RPC calibrations reported by the HARP Collaboration, Preprint arXiv:0711.2216 [hep-ex]
- [4] M.G. Catanesi *et al.*, Eur. Phys. J. **C51** (2007) 787
- [5] M.G. Catanesi *et al.*, Large-angle production of charged pions by 3 GeV/ $c$ –12 GeV/ $c$  protons on carbon, copper and tin targets, Preprint arXiv:0709.3464 [hep-ex], accepted for publication in Eur. Phys. J. **C**
- [6] M.G. Catanesi *et al.*, Large-angle production of charged pions by 3 GeV/ $c$ –12.9 GeV/ $c$  protons on beryllium, aluminium and lead targets, Preprint arXiv:0709.3458 [hep-ex]
- [7] V. Ammosov *et al.*, The HARP WhiteBook, HARP Memo 06-101 (6 September 2006), <http://cern.ch/dydak/WhiteBook.pdf>
- [8] V. Ammosov *et al.*, Addendum to the HARP WhiteBook, HARP Memo 06-105 (23 October 2006), <http://cern.ch/dydak/WhiteBookAddendum.pdf>
- [9] V. Ammosov *et al.*, Second Addendum to the HARP WhiteBook, HARP Memo 07-101 (10 January 2007), <http://cern.ch/dydak/WhiteBookAddendum2.pdf>
- [10] V. Ammosov *et al.*, Nucl. Instrum. and Meth. Phys. Res. **A** **578** (2007) 119
- [11] V. Ammosov *et al.*, IEEE Trans. Nucl. Sci. **54** (2007) 1454
- [12] F. Dydak, On distortions of TPC coordinates: inhomogeneities of electric and magnetic field, HARP memo 03-001 (6 June 2003), <http://cern.ch/dydak/TPCd distortions.ps>
- [13] F. Dydak, A. Krasnoperov and Yu. Nefedov, TPC track distortions: correction maps for magnetic and static electric inhomogeneities, HARP Memo 03-002 (30 June 2003), <http://cern.ch/dydak/TPCd distortions2.ps>
- [14] I. Boyko *et al.*, TPC track distortions III: *fiat lux*, HARP Memo 05-101 (17 January 2005), <http://cern.ch/dydak/TPCd distortions3.pdf>
- [15] V. Ammosov *et al.*, TPC track distortions IV: *post tenebras lux*, HARP Memo 07-102 (10 August 2007), <http://cern.ch/dydak/TPCd distortions4.pdf>
- [16] A. Artamonov *et al.*, J. Instrum. **2** (2007) P10004
- [17] HARP Collaboration, Nucl. Instrum. and Meth. Phys. Res. **A571** (2007) 564
- [18] M. Bogomilov *et al.*, IEEE Trans. Nucl. Sci. **54** (2007) 342

- [19] HARP Collaboration, Momentum scale in the HARP TPC, Preprint arXiv:0709.2806 [physics.ins-det]
- [20] V. Ammosov *et al.*, Pad equalization and  $dE/dx$  in the HARP TPC, HARP Memo 06-104 (19 October 2006), <http://cern.ch/dydak/dEdx.pdf>