

The HARP WhiteWash

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Abstract

In their persistent efforts to whitewash themselves, HARP Management loudly claim that us, the members of the CERN–Dubna–Protvino Analysis Group in HARP, refuse to adhere to ‘HARP rules’. Yet they keep thundering silence on the reasons why this happens. Here, we present cogent documentary evidence of the boycott and defamation campaign by HARP Management against us that they systematically pursued from their first day in office, and that led to our complete alienation. Since this paper’s topic fits a cabaret rather than CERN premises, we recommend reading the paper while enjoying a Viennese *Melange mit Sachertorte*.

‘...This [timely physics results of high quality] can only be achieved by ... allowing for open scientific discussion, respecting opposing views...’

J. Panman, email of 16 June 2003 to the HARP Collaboration

‘...The collaboration reserves the right to apply a “light” refereeing procedure, to make sure that the content is correct (e.g. comparisons with other results are correct), and proper credit and reference is given to work of others...’

J. Panman, email of 18 March 2004 to the HARP Collaboration

‘...scientific arguments and criticism were silently cannibalized by the same people, and appeared again as their own intellectual property in a successive note...’

HARP Collaboration, Memorandum CERN-SPSC-2004-022 of 2 July 2004

‘...it is not usual to include lower-quality work in status reports...plagiarised copy...bluff...superficial analysis...’

J. Panman, email of 7 July 2004 to the HARP Collaboration

‘...The CB votes positively (with one negative vote and one abstention) on the quality of the present [large-angle] calibration, available to the collaboration, being good enough to continue detector characterisation and physics analysis...’

Minutes of the meeting of the HARP Collaboration Board of 10 March 2005, chaired by C. Goessling.

‘...dash ih nam ili net ? My ne budem nigde ssylat’sja na nih...(English translation: do you give them [the RPC calibration constants] to us or not ? We will nowhere make mention of it)’
Email of 19 March 2005 from a member of the ‘official’ HARP analysis team to a member of our group

‘...According to the dictionary the meaning [of plagiarism] is “the use of another person’s idea or a part of their work and pretend that it is your own”...The wording “bluff”, in the sense “to impress, deter, or intimidate by a false display of confidence” had been used... The assessment of the facts remains unchanged...’

J. Panman, email of 11 July 2005 to the HARP Collaboration

1 Preamble

With a view to objective documentation, this note discloses selected HARP-internal documents. We do so because in case of conflict, values like scientific truth and personal dignity must take precedence over other considerations.

2 Introduction

HARP was born from the realization that secondary hadron production by low-momentum protons of typically 1–10 GeV/ c was uncertain by factors of two to three. The result was the design of a hadron production experiment with nearly 4π acceptance.

On 17 February 2000, HARP was approved in the CERN Research Board, on the prospect of delivering **the** Book of differential cross sections of hadron production in thin and thick targets ranging from hydrogen to lead. One condition of approval¹ was the “...*understanding that their running must be completed by the end of 2001 [because the experiment’s space in the East Hall was needed in 2002 by LHCb]*’.

Despite this next to impossible schedule that was imposed by CERN Management – which left absolutely no time for feedback from tests into design and construction – HARP was successful with completing detector design, construction and commissioning within a record time of 17 months, and taking a wealth of data within 14 months.

Since LHCb was delayed, HARP successfully negotiated with the SPSC and CERN Management to take data also in 2002, until its apparatus had to be dismantled during the winter shutdown 2002/03 with a view to liberating hall space for LHCb².

3 What were the often referred-to hardware faults?

With a view to separating facts from fiction, here is the detailed list of hardware faults that were overlooked during construction or could not be corrected:

1. the use of a non-flammable gas mixture in the NOMAD driftchambers, imposed by CERN safety rules, which led to a low average single-hit efficiency of $\approx 85\%$;
2. electronic crosstalk in the multi-layer motherboard of the TPC’s pad plane, caused by inter-connection pins that were too long and led to unwanted capacitive coupling;

¹Minutes of the meeting of the CERN Research Board held on 17 February 2000.

²A HARP member found the following words appropriate, in an email sent on 10 April 2003 to the then spokesperson, F. Dydak: ‘...*the experiment has been hastily executed and precociously ended, with the consequence that the data quality is very poor if not, hopefully, unusable. This was absolutely not necessary, and, as it appears clear, was largely motivated by your own personnel agenda. If you wish to be called responsible, first take that responsibility on your chest, and let the people who care about doing good work finish it in peace. Please stop glorifying yourself for having “completed the experiment in time”...*’

3. the inner fieldcage potential should have been -8623 V rather than -8457 V;
4. the potential of the first fieldcage ring should have been $+96$ V rather than at ground;
5. the gating grid's offset potential should have been -57 V rather than -67 V;
6. the gating grid's bias voltage should have been ± 45 V rather than ± 35 V;
7. the cathode wires should have been at ground potential rather than floating.

Yes, these hardware faults existed. Not good.

On the positive side, there was a newly-built TPC that had not a single high-voltage breakdown and could be read out with a major newly-built novel electronics system, that permits the reconstruction of tracks with good accuracy (if properly calibrated, though); there were newly-built RPCs which outperform expectations by a large margin (if properly calibrated, though); there was a newly-built TOF wall that worked according to expectations; there was a newly-built Cherenkov counter that worked (after the repair of gross vacuum leaks, though), according to expectations.

All in all, given the time constraints imposed by CERN on HARP, we consider it fair to say that the experiment did pretty well.

It is instructive to note that the persistent claims of 'bad hardware performance' come from HARP members who contributed little or nothing to hardware design and construction although they could well have done so.

4 The hostile takeover

The root of the conflict in HARP is a disagreement that arose after the completion of data taking, in early 2003, between the then spokesperson (F. Dydak) and the coordinators for software (S. Giani) and data analysis (J.J. Gomez Cadenas) on priorities, method and efficiency of analysis work. The latter's uncompromising determination to impose their style of work led to a bitter dispute and eventually to a divisive 55 : 53 election of a new HARP Management, in July 2003.

In the first communication of the newly elected spokesperson to HARP [1], not a single word was found for the 3 1/2 years of work for HARP by the outgoing spokesperson.

5 The 2003 Status Report to the SPSC

Two weeks (!) after the hostile takeover, the 2003 Status Report to the SPSC [2] was submitted. This report ignores entirely our groundbreaking work on TPC distortions [3, 4] and TPC crosstalk [5] which had been properly presented to the Collaboration and had been carefully documented in three (then still) 'official' HARP memos, published on 6 June 2003, 30 June 2003 and 6 July 2003, respectively.

There was not even a formal justification of ignoring our results in the Status Report to the SPSC, let alone a scientific justification (as later developments have convincingly shown).

For anybody who had at least a rudimentary understanding of the issues of TPC distortions and crosstalk, there was no doubt that the new HARP Management were solely intent on continuing their unjustified war against the former Management through boycott and disregard of our work, at no matter what cost³.

The complete disregard of our work in the 2003 Status Report to the SPSC has not been forgotten.

6 The first year after the hostile takeover

After the new HARP Management had taken office in this – for us – very discouraging way, we nevertheless saw a window of opportunity in working on the calibrations of the TPC and the RPCs which we thought – erroneously, as we were soon to realize – would be of interest for HARP at large.

Yet the new HARP Management embarked on a crusade to discourage and make impossible the presentation of our results in presentations to the Collaboration and in HARP memos, although they claimed, and keep claiming, that it is our group who refused to participate in, and present our algorithms and results at, ‘official’ HARP meetings.

The following detailed account hopefully helps elucidating the truth.

1. **12 February 2004 (Analysis meeting):** M. Gostkin and A. Zhemchugov presented an update of our 1.5 GeV/ c beam TOF study.
2. **17 February 2004 (Collaboration meeting):** M. Gostkin presented our analysis of the beam timing data and reported our breakthrough to a 70 ps resolution of the time of arrival at the target (while the ‘official’ resolution was about 150–200 ps), supplemented by a detailed HARP memo dated 20 May 2004.

On 30 June 2004, D. Schmitz published a HARP memo on the same subject, also reporting a 70 ps resolution for the time of arrival at the target. There was no reference to our earlier work.

3. **10 March 2004:** Our group published in a HARP Memo the ‘Generalized Least Squares Fit’ [6], a general algorithm for the fit of a multi-dimensional space of error-prone variables, and its application to the fit of clusters in the HARP TPC. We considered this memo a major and entirely uncontroversial contribution to the HARP large-angle analysis.

We were shocked by a flood of scientifically untenable and personally insulting emails (see e.g. Refs. [7, 8, 9, 10, 11, 12, 13]) that followed the announcement of our memo.

This incident showed that people were not interested in scientific arguments but solely intent on hurting us. This display of lack of scientific culture made a lasting impression.

4. **18 May 2004, TPC Meeting:** A. Zhemchugov presented the performance of our crosstalk correction, that had been detailed in a HARP Memo dated 7 May 2004.

³In a remarkable display of inability to learn and adapt, this boycott and disregard of our work has persisted until today.

5. **18 May 2004, TPC Meeting:** Yu. Nefedov presented the ‘Generalized Least Squares Fit’.

We quote the minutes of this meeting: *...The criticisms already exposed on the note have not been addressed and, overall, it was confirmed that such work is not justified in HARP. The talk was not endorsed.*

This judgement by the chairperson, used thereafter by J. Panman to refuse a presentation of our ‘Generalized Least Squares Fit’ at a HARP Collaboration Meeting, led to our decision that we would never again present results in so-called ‘HARP Analysis Meetings’. We were – and are – not prepared to present our work to people whose judgement deserves no respect. Yet we decided that we would continue offering talks at least at Collaboration Meetings, in the hope to find there a less hostile and aggressive prejudice against our work.

6. **24 May 2004:** G. Chelkov requests five talks on our work in a forthcoming Collaboration Meeting (Generalized Least Squares Fit, TPC cluster reconstruction, Performance of crosstalk correction, 3.5 μ s and 100 ns problems in the water data, and an update on the water data reduction).

With the exception of the talk on crosstalk, J. Panman refused these talks, citing formal reasons. We quote a typical response from an email of 26 May 2004 to G. Chelkov: *‘...As has been communicated many times to you, these things must be presented at the proper meetings first. Since the problem has been studied by others who have reported their findings regularly, I invite you to listen to the talks on this subject...’.*

By contrast, our point of view was that it was rather ‘official’ HARP that could have learned a lot from our presentations, if they had scheduled them and listened to us.

Arguments of such kind contradicted any scientific spirit. It appeared that their sole purpose was to hurt us and to make us feel unwelcome. Apparently, our work was perceived as unwanted competition to ‘official’ work.

The refusals of our proposed talks marked a turning point in the sense that we realized that the excessive use of formalities by the new HARP Management would always take preponderance over scientific content, while for us the reverse was – and is – true.

7. **28 May 2004, Collaboration Meeting:** A. Zhemchugov presented again the performance of our crosstalk correction.

8. **28 May 2004, Collaboration Board Meeting:** G. Chelkov made a desperate last attempt to reverse the situation and convince the HARP institute representatives that useful analysis work could be done in more than one way, and that the Collaboration could profit from our work. We quote from his intervention (available as Appendix of Ref. [14]): *...[1] Scientific work is independent of the method that has been used to obtain it, there is never one and only one method to achieve results; [2] Every physicist must be free to choose the method of work which he thinks is best for his goals; [3] Every physicist must be free to communicate his results to his colleagues in the collaboration, on his sole responsibility, in writing and in oral presentation at collaboration meetings....*

G. Chelkov’s intervention met total rejection, not even a compromise was ever suggested by anybody from ‘official’ HARP.

Reluctantly, we concluded that with this HARP Management we would not get anywhere within HARP, and that we would rather have to seek needed support and acknowledgement outside HARP. The obvious address was the CERN SPSC.

9. **29 June 2004, HARP Workshop:** A. Zhemchugov presented our work on the 3.6 μ s and 100 ns problems, that had been detailed before in a HARP memo dated 27 May 2004. This marked our last presentation to HARP at our own initiative.
10. Up to the end of June 2004, our group had published nine (!) HARP memos in which our work was described in great detail [15]; although we used identification numbers that were markedly different from the ‘official’ numbers, HARP Management chose to ignore entirely the scientific content and limited themselves to numerous complaints about the ‘...*severe and repeated ethical mistakes...such as...abuse of HARP label and numbering scheme for private notes...*’ [16].

7 Our separate 2004 submission to the SPSC

In anticipation of the complete boycott of our work (although we had tried everything possible for a whole year to make HARP aware of our results, and had faced nothing but repulsion and offences), we wrote a Status Report on the work of our group (dated 22 June 2004) and submitted it with the consent of the SPSC Chairman to the SPSC [17].

‘Official’ HARP’s response was an outrageous email by J. Panman to the Collaboration, full of incorrect accusations and rude offences [18]. This email was complemented by a Memorandum [16] by ‘the HARP Collaboration’ to the SPSC Chairman of which every HARP member who has not distanced himself from the text, should feel ashamed⁴.

The sentence in this Memorandum ‘...*that the work performed by F. Dydak and the Dubna group on the TPC is redundant and unnecessary for the HARP physics goals. Such a (first unsolicited, then discouraged) effort could have been justified only if the algorithms and analysis tools developed by the rest of the collaboration were performing insufficiently: on the contrary, it has been re-stated at the Collaboration Board in May 2004 that those tools are more than adequately performing...*’ is of particular interest: J. Panman confirms explicitly that our claims of boycott and discrimination by HARP Management are well justified; he writes himself that first he did not solicit our work, and then discouraged (!) it.

8 The denunciatory letter to the CERN DG

On 28 June 2004, C. Goessling and J. Panman addressed a letter to the CERN DG in which they asked for sanctions against F. Dydak, on grounds of factually wrong and utterly offensive accusations. This letter, although unmistakably written in the name of the HARP Collaboration, was never made available to the Collaboration Board, let alone to the Collaboration. It seems, the reason is that its primitive denunciatory content tells much more about the authors than about the target of their attack.

We really wonder whether HARP members just follow HARP Management, no question asked, or at least occasionally care about knowing how HARP Management act and write in their name.

⁴The HARP spokesperson never deemed it necessary to inform the Collaboration that this Memorandum ended up as the first ‘official’ HARP communication to be removed from the SPSC archive.

Our submission of the separate report to the SPSC, and C. Goessling's and J. Panman's letter to the DG marked the termination of our willingness to suffer from obsessions of HARP Management with rules and methods that we considered inappropriate and ineffective.

In July 2004, after only one year in office of the new HARP Management, our relations with them were thoroughly broken.

9 From July 2004 to July 2006

On the suggestion of CERN Management, we gave two more presentations in the HARP Collaboration meeting of 9 March 2005. I. Boyko presented the temperature dependence and the resolution of the RPC timing, that were both detailed in two HARP memos dated 9 February 2005 and 9 March 2005, respectively. Yu. Nefedov presented our final results on the static distortion corrections and preliminary results on our dynamic distortion corrections, that were detailed in a HARP memo dated 17 January 2005 and later in our submission to the SPSC [19] dated 21 June 2005.

We offered our complete RPC analysis chain to 'official' HARP but refused to give just our calibration constants, for fear – well justified as it turned out – that they would be used within wrong concepts and we would eventually get blamed for wrong 'official' results.

On the suggestion of CERN Management, we made on 18 July 2005 in our 'Good will gestures' message [20] our complete analysis software publicly available and declared ourselves then – and also several times later – prepared to report on, and discuss, all our results in a 'neutral setting'.

All failed.

HARP Management remained stuck with their way of duplicating our work, remained stuck with producing results that were considerably worse than ours if not wrong, remained stuck with their 'rules', and made a virtue of their total lack of flexibility.

10 Summary and conclusion

From their first day in office, HARP Management did everything possible to discourage our group from working and prevent us from presenting our results to the Collaboration.

Although our work was meticulously written up and publicly available, it was not referred to with a single word in any 'official' HARP document, or in 'official' HARP memos, or in 'official' HARP presentations.

HARP Management resorted publicly to unjustified defamations and rude offences which were never taken back and for which no apology was ever received.

Our conclusion is so obvious that it needs not be formulated in writing.

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