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State of HARP

F. Dydak / 24th March, 2003

HARP was born in September 1999. It went through the stages of design, construction, commissioning and data taking in only three years. HARP lived through several difficult and even critical moments, but on a whole the experiment has been a success of which the Collaboration can be proud. Yet the remaining and ultimately most important job, namely the analysis of the data and their publication, is still ahead of us.

In August 2002, a reorganization of the Software and Analysis Project took place. The Analysis Project, headed by the Analysis Convenor, was separated from the Software Project, led by the Software Manager. A plan was presented and adopted by the Collaboration which had as Phase 1: subdetector calibration, Phase 2: comparison of data with Monte Carlo, and Phase 3: data analysis. The plan was generic and had no schedule, no milestones and no resources attached. At the time this was not considered to present a problem since it was everyone's tacit assumption that the projects would evolve correctly and proceed with reasonable speed.

In this address, I will present my analysis of where we stand today. First of all, I wish to acknowledge that in the software and analysis domains a lot of good work has been done by quite a number of people, groundbreaking work as well as support for specific tasks as they arose.

Yet from the point of view of results relevant for physics analysis, I will argue that the track reconstruction is late by more than half a year. Therefore, I consider that the Analysis Project is in difficulty and corrective action is needed. I will present my analysis of why the project got into this state. Finally, I will present my proposal on how to overcome the difficulty.

At this point, a short reminder on the Spokesperson's rôle may be in order. Among other duties, he is responsible to the Collaboration Board for what is going on. In normal circumstances, this responsibility is hidden. However, in a situation of difficulty the Spokesperson is not only expected but has a duty to bring any developments to the attention of the Collaboration Board which he considers worrying.

The track reconstruction is late by more than half a year

The appropriate yardstick for measuring progress towards physics analysis is readiness for the mass reconstruction of data. One could reasonably expect that by now, 2 1/2 years after the launch of the Software and Analysis Project, seven months after its reorganization, and five months after the end of data taking, we should be on the verge of starting the first mass reconstruction of data.

Where are we in reality? Let us recall that prior to mass reconstruction of data, the following must be accomplished:

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- selection of good data
- identification of dead channels and their handling in the
  reconstruction program
- subdetector calibration
- correction of TPC Xtalk
- efficient track reconstruction in MWPCs, TPC and NDCs
- particle identification in RPCs, Cherenkov, TOF wall and
  beam-muon identifier
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Final results are available for the first item only. The identification of bad channels and their handling in the reconstruction program has not yet started.

As for subdetector calibration, we still have no mass production of Calibration Ntuples, let alone calibrated subdetectors. I do not share the Analysis Convenor's opinion on the accumulated delay [1].

The most important item is efficient TPC and NDC track reconstruction. We keep being assured that we have successfully tested high-quality reconstruction algorithms which permit the selection of 'golden tracks'.

I believe that nobody will contradict when I state that a reconstruction program only 'works' when it reconstructs data with high and certified performance. 'Golden tracks' may arguably have some rôle in detector calibration and in public relations, but don't suffice for physics analysis.

Let me concentrate on the NDC track reconstruction which has been under the personal attention of the Analysis Convenor since the beginning of HARP.

I should like to recall that already eleven months ago we had three concepts: the official one pursued by the Analysis Convenor himself, a second one from our Dubna/Paris colleagues, and a third one from our Trieste colleagues. I insisted on a fair comparison of the three concepts by running them on the same set of data. The conclusion was then that all three concepts had inadequate performance and further work was needed. In this context, it is also worth remembering that the average 80–85% single-hit efficiency of the NDCs, which is the driving consideration for any pattern recognition, was already well-known in early 2002.

Now we are suddenly told that *'Alternative algorithms, such as the one from Pietro Chimenti, are being actively investigated. In fact, during the past two weeks there has been a major coordinate effort (Pietro, Malcolm, Anselmo) to include Pietro's work in our software framework'* [2].

I consider that the importance of efficient NDC track reconstruction has not received the priority it deserves, and that the sudden announcement of the inclusion of another algorithm constitutes a tacit admission of inadequate performance of the official algorithm.

I would have expected an open discussion of these problems, with a thorough understanding and assessment of the pro's and con's of existing algorithms, and implementation of the best procedure, in continuous confrontation with the reality of data. What is overdue now could have already been done eleven months ago.

The following two questions

1. `How often is a physical track correctly reconstructed in the NDCs?`
2. `How often does the reconstruction in the NDCs find a track although there is no physical track?`

are still without answer. I fear that our NDC reconstruction has has an inacceptably low efficiency. I would be more than pleased to see hard numbers which prove the contrary.

I take responsibility that, as Spokesperson, I did not realize earlier that we were heading for difficulties, and I did not take a strong stance early enough to get things back on track. I accept this criticism.

The Analysis Project is sub-optimally organized

The Analysis Convenor insists that the weekly Analysis Meeting is the only place where everything is to presented. In his perspective, *'topics not presented in the Analysis Meeting do not exist'* [3].

By contrast, I am of the opinion that the Analysis Meeting is a good opportunity to discuss agreed concepts, review agreed manpower assignments, present results, and generally keep people informed on the progress of work. However, the Analysis Meeting is NOT the place to hold in-depth technical discussions (which should be held among experts), or to fight over controversial concepts, or to take decisions.

The first issue here is decision taking. The Analysis Convenor himself states: *'The Forum for discussion and information is the Analysis Meeting. The mechanism to take decisions, as far as I know, is the software management board whose members are Jaap Panman, Simone Giani, Christopher Wiebusch (secretary) and myself'* and *'In any case, it is indeed true that decisions are not taken in the analysis meetings. They are taking at the SMB'* [4].

I had been unaware of the existence of a Software Management Board. When I asked S. Giani I was told that the Software Management Board had not met since August 2002.

Therefore, I consider that HARP has no satisfactory mechanism for decision-taking in analysis matters.

The second issue is the Analysis Convenor's non-cooperative attitude vis-à-vis colleagues. Everyone is invited to judge for himself from a telling sequence of events in the TPC Xtalk saga [5].

The third issue is the Analysis Convenor's strategy towards physics analysis. In a situation where track reconstruction is presumably far from being adequate for physics analysis, it is not wise to deploy our scarce resources on developing an object-oriented Data Summary Tape (iDST) which is estimated to take one man-year of expert's work.

Instead, we should focus our meager resources on achieving a performant reconstruction program. Thereafter, we could use the acquired experience with Calibration Ntuples to write Analysis Ntuples, for physics analysis in all HARP institutes.

The fourth issue is management. We need smooth and steady progress, not months of stand-still followed, under pressure, by one month of fireworks. We need a broad analysis organization where everyone interested finds a place according to his talent, rather than analysis work too long confined to too small a circle of experts.

The Spokesperson's policy

I consider that the goal of achieving a preliminary physics analysis in the summer of 2003 is a suitable vehicle to get the Analysis Project out of difficulty.

A preliminary physics analysis has nothing to do with 'lack of care' or with 'quick and dirty'. A personal remark may be in order here. I believe I have an established record as scientist that I would never go along with the publication of results where there is doubt whether they are correct within the quoted errors.

However, a preliminary result with larger than final errors is customary in particle physics and perfectly defensible. There is nothing wrong with giving preliminary results with perhaps a 10% error, rather than with 2% error which is finally aimed for.

I believe that the outside world expects first results from us, and questions will be asked why HARP results are not forthcoming. A preliminary physics result would solve this problem.

However, a preliminary physics result is primarily useful for HARP's internal well-being. Such goal will force us to focus on the important things and will enable a better assessment of our real problems. This, in turn, will help us to deploy our resources in an optimal way in the second, high-precision round of analysis. And: nothing will bind the Collaboration better together than the common achievement of a physics result.

After we had already to decline an invitation to present early results at the Moriond 2003 conference, the next occasions for the presentation of preliminary results are:

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- the NuFact'03 Workshop in the first week of June (invitation pending)
- the EPS Conference in July
- the ICRC 2003 Conference in August (generic abstract submitted)
- the (likely) progress report to the SPSC at the end of August
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For NuFact'03, the only conceivable physics result could come from the DeepWater electron/positron analysis [6]. For later occasions, further physics-motivated candidates for early analyses are the π^+/π^- ratio in ThinWater data, and (p_{L}, p_{T}) distributions from MiniBooNE data.

Out of the occasions listed above, the importance of the progress report to the SPSC sticks out. 15 months after our last progress report, we can hardly tell them again, *'that* *we are working hard and sometime in the future we will have results.'*

By contrast, the Analysis Convenor sees no urgency for preliminary physics results and *'could not care less'* how HARP will be seen from the outside world [7].

The Analysis Convenor insisted that the only way the disagreements of opinion could be resolved, was to bring them to the Collaboration Board: *I am the elected Analysis Convenor, I have the confidence of all people, I know exactly what to do, I take decisions and I want the Spokesperson to stay out of them; the only rôle of the Spokesperson is to provide the necessary resources for analysis work; if the Spokesperson does not agree, the Collaboration Board must decide between my views and the Spokesperson's views'* [10].

The Analysis Convenor twice refused to respond to calls for meetings to seek a compromise [8, 9].

HARP's data analysis cannot remain stalled in a situation where the Analysis Convenor insists on working not only without but against the Spokesperson.

It is tempting to take a statesman's posture and call for 'unity' and 'cooperation'. While such an attitude always sounds good, it is not the appropriate response to our difficulties. First, the Analysis Convenor's lack of results towards physics analysis, and then his uncompromising and uncooperative behaviour, has undermined hopes for reconciliation.

For the Spokesperson it is mandatory that he can identify himself with the policies pursued within the collaboration. It is inconceivable that the Spokesperson has to stay out of analysis matters.

And needless to say that the Spokesperson needs the Collaboration's confidence in his work.

In view of all the above difficulties, the Spokesperson suggests that the Collaboration Board

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- appoints, upon proposal by the Spokesperson, a new Analysis Convenor
  ad interim;
- appoints, upon proposal by the Spokesperson, an Analysis Board
  ad interim, comprising three physicists actively involved in
  physics analysis; the Analysis Convenor, the Software Manager
  and the Spokesperson are ex officio members; the Analysis Board will
  assess the situation and establish realistic goals for summer
  2003; the Analysis Convenor, in cooperation with the Software Manager
  and the Spokesperson, will organize the execution of the work;
- reviews the situation at its next meeting in July 2003.
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References

[1] J.J. Gómez Cadenas, email of 16.02.2003 to F. Dydak:
'...The effort to produce the calibration NTUPLES given the slow response of the collaboration and the lack of man power, has been conducted in a record time...'

[2] J.J. Gómez Cadenas, email of 10.03.2003 to the HARP Collaboration.

[3] J.J. Gómez Cadenas, statement made on 03.02.2003 to F. Dydak on the topic of the 'DeepWater Analysis', which had been proposed by F. Dydak and G. Mills, twice presented by F. Dydak in HARP meetings, and documented in http://cern.ch/dydak/LSNDriddle.ps. The statement's wording may not be exact but is correct in its spirit.

[4] J.J. Gómez Cadenas, email of 15.02.2003 to F. Dydak

[5] 1. On 3 February 2003, J.J. Gómez Cadenas agrees in a discussion with F. Dydak on the importance of getting the TPC Xtalk problem solved. F. Dydak declares himself ready to supervise the pertinent work of J.J. Gómez Cadenas' PhD student J. Burguet Castell. J.J. Gómez Cadenas enthusiastically agrees that J. Burguet Castell is exclusively at F. Dydak's disposal for this work, but only until 24 March. F. Dydak agrees and commits himself to obtaining results until 24 March.

2. F. Dydak presents at J.J. Gómez Cadenas' request in the Analysis Meeting of 11 February 2003 'LA analysis issues: what, when, who' (http://cern.ch/dydak/LAanalysis.ps), in which also the work on Xtalk is discussed.

3. J.J. Gómez Cadenas requests from F. Dydak for the Analysis Meeting one week later again a talk on TPC Xtalk. F. Dydak declines:

From dydak@mail.cern.ch Sun Mar 16 17:01:44 2003
Date: Sat, 15 Feb 2003 14:36:25 +0100 (CET)
From: Friedrich DYDAK ¡dydak@mail.cern.ch¿
To: Juan Jose GOMEZ CADENAS ¡gomez@ific.uv.es¿
Cc: Jordi BURGUET ¡jordi.burguet.castell@cern.ch¿, Alberto DE MIN ¡demin@padova.infn.it¿, Gersende PRIOR ¡gersende.prior@cern.ch¿, Lara HOWLETT-SCOTCHMER ¡lara.scotchmer@cern.ch¿, Valeri SERDIOUK ¡valeri.serdiouk@cern.ch¿
Subject: Provisional agenda

Dear JJ:

You proposed that I make a presentation of 'X-talk correction campaign, status, prospects' on Tuesday next.

I do not think this is useful. I just presented the status and prospects in the last analysis meeting, including a time-table with explicit milestones of 28 February and 24 March. I have nothing new to add. The overall strategy has been presented several times already, and is available as write-up, again nothing new to be added.

There are, of course, many questions of detail behind the X-talk correction campaign. These are continuously being discussed among experts, but such discussions are not suitable for the analysis meeting because they are time-consuming and primarily useful only to those directly involved.

The next time that X-talk should be on the agenda, is when results(!) can/should be reported (summary of what we learned from the hardware measurements, availability of hardware results to the correction algorithm in a suitable format). The respective milestone date is 28 February.

Sincerely, Friedrich

4. F. Dydak realizes that previous Xtalk correction concepts apply only to a restricted number of cases and works out a more general scheme, with particular attention to easy applicability in a correction algorithm.

5. F. Dydak proposes an expert's discussion between J. Burguet Castell, A. De Min and himself:

From dydak@mail.cern.ch Tue Mar 4 08:19:19 2003
Date: Wed, 26 Feb 2003 15:46:46 +0100 (CET)
From: Friedrich DYDAK ¡dydak@mail.cern.ch¿
To: Jordi BURGUET ¡jordi.burguet.castell@cern.ch¿, Alberto DE MIN ¡demin@padova.infn.it¿
Subject: Meeting

Dear Jordi, dear Alberto:

Unfortunately, I cannot be at CERN from 27.2. until 2.3. inclusive.

I believe, I have cut through the TPC Xtalk problem and wish to expose my conclusions to expert criticism, i.e. yours.

We have no time to lose. Can we meet on Monday next (3.3.) at 09.00 h, in the Conference Room bldg 1/1-025 (where we have a whiteboard)? Be prepared for a thorough discussion which lasts all morning. Please confirm!

Sincerely, Friedrich

6. Information by F. Dydak on a hastily written paper for experts:

From dydak@mail.cern.ch Tue Mar 4 08:18:36 2003
Date: Wed, 26 Feb 2003 20:57:14 +0100 (CET)
From: Friedrich DYDAK ¡dydak@mail.cern.ch¿
To: Jordi BURGUET ¡jordi.burguet.castell@cern.ch¿, Alberto DE MIN ¡demin@padova.infn.it¿

Dear colleagues:

For Monday next, you can get some idea what I propose to discuss with you, from the draft paper

http://cern.ch/dydak/crosstalk2.ps

Please bear with me! I would never expose such a rough and incomplete draft to colleagues of mine, if there were not the pressure of time (I am to leave CERN now, will be back only on Monday morning). The paper is surely very hard to read, I will anyway go slowly through its contents on Monday. But maybe you risk a look already before.

By the way: I hope that through the discussion with you, and with your input, the paper will become eventually mature and can be given to the collaboration. I hope that by this time you will have joined me as authors.

Sincerely, Friedrich

7. J.J. Gómez Cadenas forbids his student to meet F. Dydak.

From Juan.Jose.Gomez.Cadenas@cern.ch Tue Mar 4 08:20:27 2003
Date: Thu, 27 Feb 2003 20:43:56 +0100 (CET)
From: Juan Jose Gomez Cadenas ¡Juan.Jose.Gomez.Cadenas@cern.ch¿
To: friedrich.dydak@cern.ch
Cc: jordi.burguet@cern.ch, jaap.panman@cern.ch, simone.giani@cern.ch
Subject: Fwd: (fwd)

Dear Friedrich,

This is to inform you that Jordi Burguet will discontinue his work on the TPC cross talk correction until the strategy and methodology of this program is clarified (e.g, in the analysis meeting, as of my invitation –declined by you– of two weeks ago). Consequently he will not be available to meet you next Monday.

Best regards, Juan Jose

[6] F. Dydak, A contribution to the LSND puzzle?,
http://cern.ch/dydak/LSNDriddle.ps

[7] J.J. Gómez Cadenas, in an email of 16.02.2003 to F. Dydak:
'...As for EP Management, let me put it a bit bluntly. I couldn't care less. Perhaps EP Managers have nothing better to do, in the face of the enormous delays of LHC than nagging our humble experiment?'

[8] J.J. Gómez Cadenas, in an email of 26.02.2003 to F. Dydak:
'...In particular, if you decide to send an email to the board, please do not submit it first to me for feedback. I believe we have made our respective points of view very clear today, therefore it is not useful to go through any further iteration...'

[9] J.J. Gómez Cadenas, in an email of 02.03.2003 to L. Linssen:
'...I appreciate very much your proposal to discuss. Indeed I am and have always been open to such a discussion. However, I believe that the right forum is the analysis meeting, where we can have a participation as wide as possible...'

[10] Statement made in a meeting with F. Dydak on 26 February 2003. The wording may not be exact but is correct in its spirit.