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Some Historical Records on the Work of Friedhelm Brasse in the Years 1964 to 1994

by

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Abstract:

Some facts on the scientific work of Friedhelm Brasse in the years 1964 to 1994 are recollected on the occasion of his retirement from DESY at May 10 , 1994.

to Friedhelm Brasse
and his coworkers in group F21

1 Introduction

Friedhelm Brasse worked on electron and muon scattering at DESY and CERN since the early sixties until today and contributed strongly to this field like very few others. As I went along with him quite some time, I try to record the main steps and circumstances [1]. The effort to avoid personal bias is small.

2 Some early dates

- May 8, 1929, born in Leckingsen
- 1949 to 1957 Studies of Physics at the University of Bonn
- 1954 Diploma [1] at MPI for Iron Research at Düsseldorf.
- 1957 PhD [2], same MPI
- 1957 to January 31, 1959, Assistant at the MPI
- February 1, 1959, to December 31, 1960, employed by Prof. W. Jentschke ("Privat-Dienstvertrag")
- since January 1st, 1961, employed by DESY
- 1959-60 work on electron beam extraction at DESY [3]
- 1961 leave of absence for studies at the Cambridge Electron Accellerator (CEA). (See [4]).
- 1962 work on beam extraction at DESY
- 1963-65 work on electron and proton spectrometers in the DESY synchrotron tunnel.

3 Experiments with Internal Beam in the DESY Synchrotron

The first *ep* experiments at DESY were based on magnetic spectrometers situated inside the DESY synchrotron tunnel [5]. After acceleration of the electrons, the orbit was distorted ("beam bump") and the beam passed some 100 times through a small target (first polyethylen later hydrogen or deuterium) of ≈ 1 cm length. Scattered electrons and recoil protons were recorded by scintillators and Cerenkov counters. Events fulfilling the electronic concidences were just counted. No computers were yet in the game.

[1] also as some reference for the event of his retirement at May 10, 1994.

The first papers are produced by a collaboration of the DESY group F21 with KFK Karlsruhe (G. Galster, G. Hartwig and Herwig Schopper, the latter of which had at the time his first activities at DESY, coming back as director in 1973). The DESY based physicists, besides Friedhelm Brasse, were Hans-Joachim Behrend, Eberhard Ganßauge (Marburg), Hans Hultschig and the PhD student Jochen Engler and somewhat later Werner Flauger. Important technical work was done already at that time by Klaus Thiele (targets, cryogenics), Johannes Koll (Cerenkov detectors, mechanics of all kind) and Helmut Weiss (electronics). K. Thiele and J. Koll worked with Friedhelm up to now, H. Weiss until the times of EMC.

3.1 Heavy Electron Search

Friedhelm reacted often fast on new physics questions appearing on the horizon (see e.g. section 5.2). As a consequence the first published result based on the internal spectrometers was not devoted to proton form factor measurements, but to a search for a heavy electron [6]. The search was triggered by an indication of deviations from QED by measurements of electron pair production [7]. But no trace of a heavy electron was found.

3.2 Elastic *ep* scattering

The determination of elastic proton form factors was the primary experimental goal in the tradition of the experiments of Hofstadter at Stanford and the experiments at Cambrige (CEA) and at Cornell University. Before SLAC came to operation the DESY group explored the until then largest momentum transfers to the proton and found [8] that the magnetic form factor can be described very well by the empirical dipole dependence $1/(1+Q^2/0.71)^2$ up to momentum transfers of $Q^2 \approx 10$ GeV^2, different from other findings.

3.3 Inelastic *ep* scattering

The really unknown attracted Friedhelm certainly more than precision measurements of well defined reactions. This led to early investigations of inelastic *ep* scattering [9].

The energy spectra taken at different fixed electron scattering angles showed beautiful resonance structures for hadronic invariant masses W up to about 2 GeV at momentum transfers Q^2 in the range 0.5 to 2.5 GeV^2.

The results on the Q^2 dependence and their discussion are most interesting, showing that - more and more distinctly with increasing W - the decrease of the cross section with Q^2 was not as strong as naively to be expected from the nucleon form factor. In retrospect this is to be related to the scattering from point like constituents of the nucleon in hard inelastic reactions. The studies were extended later to larger momentum transfers up to $Q^2 \approx 5$ GeV^2 with similar conclusions [10].

Just at the same time Bjorken predicted "scaling" in deep inelastic scattering [11] and preliminary data from SLAC [12] indicated consistency with it. This changed the perspectives considerably. Therefore in the last experiment of the series, it was tried to go well beyond the resonance region (up to $W \approx 3$ GeV) [13]. Indeed approximate Bjorken scaling was observed, but the higher energies available at SLAC allowed for a much better job there.

However the combination of the large angle data of DESY with data from SLAC, led at the electron and photon Conference 1969 at Liverpool to the very important conclusion that the longitudinal virtual photon cross section is small, consistent with spin 1/2 partons [14].

A contemporary discussion of the *ep* data of this time can be found in talks of F. Brasse and J. May [15].

4 Parametrizations of the World *ep* Data

Certainly it is the main strength of Friedhelm to look into the future and to prepare experiments in time, but he worked also on the full exploitation of experimental results. In 1971, with an update in 1976, the Q^2 dependence of all existing data on *ep* total crossections was fitted in steps of 15 MeV in W over the whole resonance region for 2 different ranges of the virtual photon polarization ϵ. The resulting parametrizations [16] clearly showed that the longitudinal cross section σ_l is small everywhere in $1.1 < W < 2$ GeV.

Also the deep inelastic *ep* data were parametrized in collaboration with V. Rittenberg and H. R. Rubinstein who had proposed a generalization of the Bjorken scaling variable $\omega = 1/x$, ω_W (W for Weizman) which allowed for a parametrization of deep inelastic data including small Q^2 and even photoproduction [17].

5 Experiments with the External Electron Beam

5.1 The First Pion-Nucleon Resonances $\Delta(1232)$

Meanwhile the preparations for another series of experiments were completed. In an early international collaboration, with a group of Collège de France, Paris, Friedhelm started a systematic study of the $\Delta(1232)$ resonance by a coincidence technique. The scattered electron was detected by a scintillator array in a large spectrometer with 2 focusing quadrupoles and 2 vertically bending magnets. The final state protons were detected in scintillator hodoscopes behind a vertically bending magnet. The production of a π^0 was infered from the calculated missing mass.

The french group was led by André Courau (now collaborating again in H1). Two new PhD students, Jörg Gayler and Jürgen May had shown up at DESY in spring 1967 and were devoted mainly to that experiment.

First preliminary results were already shown at the electron and photon conference 1969 at Liverpool. The data allowed for detailed multipole analyses which were compared with models based on Dispersion Theory (G. v. Gehlen) and exploitation of the Bethe Salpeter equation (F. Gutbrod). Fritz Gutbrod had worked in close contact with the group also before. For final summaries see [18].

By the way, the french group had supplied the scintillator hodoscopes for the proton detection. The support structure was originally a gun-carriage, a beautiful example of arms conversion.

5.2 How Do Spin 1/2 Partons Materialize ?

In August 69, S.D. Drell gave a seminar at DESY and speculated on implications of the recently established Bjorken scaling (compare section 3). If there are spin 1/2 partons, how do they show up in the lab after being kicked hard by the virtual photon? Not much was known at the time. Could they materialize preferentially as protons having spin 1/2 as well? Is the yield of forward protons increasing [19] with Q^2 ? Is it larger than in photoproduction? Friedhelm and the group decided to have a look.

To observe the forward particles produced in the deep inelastic region a new spectrometer based on 3 focusing half quadrupoles was rapidly built, which allowed then to measure at small angle (10 deg.). The detectors were scintillators, a Cerenkov detector and a iron/scintillator sandwich calorimeter. With the scintillators, arranged in a special code, a space resolution of ± 1.6 mm was reached (lots of careful work required, especially by Gerhard Singer). All the details are in the PhD theses of Volker Korbel and Werner Fehrenbach [20].

The results [21] (a first preliminary report appeared in April 1971 [22]) showed that the forward proton spectra at $Q^2 = 1.2$ GeV^2 ($W = 2.6$ GeV) do not differ from those of photoproduction.

A convincing negative result.

Besides the protons, also forward inclusive pions and kaons have been studied (see [21] and and the comprehensive review of F. Brasse [23]).

5.3 Exploration of Higher Nuclear Resonances

To extend the studies of resonance decays (section 5.1) to the higher resonance region, a new proton/pion spectrometer was built with larger solid angle [24]. Now the group took up the technique to build multiple wire proportional chambers (mainly Friedhelm with Jürgen May and Klaus Thiele and diploma students G. Glöe, H. Mehrgardt and W. Krechlok). The experiment had extensive on-line monitoring, a great deal due to the skills of the diploma student Rüdiger Schmitz.

Friedhelm led the whole project. In the evening one could still find him alone in the experimental hall ("Brasse Halle") carrying huge ladders around, climbing the new scintillator hodoscopes at the back and checking that everything was properly installed (,but K.H. Frank and G. Singer had done the job before).

Data taking started in 1972. The most striking result was the much flater Q^2 dependence of the transition to the baryon resonance N(1535) S_{11} compared to N(1520) D_{13}. The resonance S_{11} could be isolated due to its large branching ratio to ηp decay. The data (see [25] for the very last paper in the series) could be interpreted in terms of a single quark transition model [26]. The various results on single π^0, π^+ and η production supplied input for the helicity structure analysis based on fixed t dispersion relations (especially R.C.E. Devenish and D.H. Lyth). For details see the PhD theses [27] and reviews [28,29].

An up to date QCD based discussion of transition form factors à la S. Brodsky/P. Lepage was not yet available.

5.4 The π^0 Experiment

A beautiful experiment [30] on exclusive π^0 production above the resonance region resulted from a collaboration with the DESY group F35, which traditionally worked on photoproduction, among them Bjorn Wiik, the 3rd future DESY director experimenting with Friedhelm. All 4 final state particles were detected in coincidence: the proton in the large hadron spectrometer (section 5.3), the 2 γ from π^0 decay in a lead glass wall (provided by F35) and the electron in the "forward hadron" spectrometer of section 5.2, which was rotated by 90 degrees around the beam, such that the electron was scattered upwards leading to a vertical transverse polarization ($\epsilon \approx 0.7$) of the virtual exchanged photon. The polarization was then vertical to the plane of the final state p and π^0.

This was just what was needed to compare directly with photoproduction data which show a distinct dip in the t-distribution (momentum transfer to the proton) at $t \approx 0.5$ GeV^2. However no such dip was observed in electroproduction, even not at Q^2 as small as 0.22 GeV^2.

The beautiful data showed a striking effect, but no convincing theoretical explanation is, to my knowledge, available yet.

6 The European Muon Collaboration(EMC)

6.1 The Early Days

It was natural to think about a continuation of lepton-nucleon scattering at much higher energies. This opportunity was offered by the forth coming CERN SPS ("CERN 2" at the time). The possibilities for interesting experiments at DESY were limited, as the DORIS e^+e^- ring only offered room for 2 experiments.

Typically for Friedhelm, he started first activities already in 1971, although the program at DESY (section 5) still went on until 1977. An important event for the later formation of EMC was the ECFA study week in Tirrenia, near Pisa in Sept. 1971. Two groups performed already detailed calculations on a high energy muon beam in the 200 to 300 GeV range [31], one around Friedhelm Brasse and one around Erwin Gabathuler. They formed a first core for the future EMC, together with Jürgen Drees who worked with Martin Leenen already on radiative corrections for muons [32]. French interest in muon scattering was visible already too [33].

It took until December 1972 that the first meeting of "The 300 GeV μ Experimental Group" took place at DESY (the name EMC only showed up in November 1974). A first list of physics subjects indicates that the DESY group was again especially interested in the field of hadronic final states, where later indeed it contributed some early EMC papers.

In parallel to the on going data taking at DESY (section 5) more and more effort of the DESY group was spent for the EMC apparatus [34]. Friedhelm took special interest in the trigger requirements, as did Werner Flauger, who built (supported by Helmut Weiss) the large programmable coincidence matrixes for the scintillator based muon trigger. Friedhelm's main effort went however into the large drift chamber system "W45" covering an area of $2.5 \times 5m^2$ with wires up to 5 m length [35]. Here again important technical work was done by Klaus Thiele and by Gert Falley. Later the large Cerenkov detector "C1" was built for NA9 (see section 6.2). This was the domain of Johannes Koll, Gerhard Singer and Klaus Thiele. Having done its duty at EMC, this detector was later installed at FNAL in the E665-experiment.

Furthermore the DESY group supplied the active iron target ("STAC"), essential for the discovery of the "EMC effect". The scintillator/iron sandwich was built mainly by W. Flauger, J. Koll and G. Singer. It was supplemented by the smaller sandwich, which helped already at DESY in the studies of the hadronic final states in the deeply inelastic region (identifying hadrons, section 5.2, and also electrons, section 5.4).

In 1977 and first half of 1978 Friedhelm stayed at CERN. It was the time when beam tests of the "W45" chambers and the target "STAC" took place in West Hall with heavy involvement of W. Flauger and V. Korbel, and when the installation of the experiment started in the North Area. Meanwhile Jürgen May, with strong affection to technical coordination as Friedhelm, had found more room for his talents at CERN leaving DESY (and EMC) in early 1975 (today back at DESY in the directorate).

In 1977 Erwin Gabathuler was still EMC spokesman. Clearly, Erwin and Friedhelm were the most striking characters in EMC, and it was unavoidable that they did not always agree in the daily interactions on the floor. For both it took some time to learn fully appreciate the others capabilities.

6.2 EMC Spokesman

In the years 1980 to 1981 Friedhelm stayed again fully at CERN as spokesman of EMC. The collaboration had a democratic procedure to establish its chief. The rules had been redefined at the time of the election of the third spokesman (Friedhelm in the end) at the general EMC meeting at Wuppertal in June 1979. In the spirit of the seventies, the grass roots had required and received more power.

In my eyes Friedhelm changed considerably when he took over this job. Having been strongly concerned with the interests of the DESY group before, he now listened very patiently to everybody. Remarkable was also the change of style of the weekly "Tuesday Meeting" when Friedhelm took over. Before, there had been very lively discussions of the various important people available. With Friedhelm the little fights nearly vanished. Everybody relaxed and expected he will settle the issue anyhow.

The real challenge for Friedhelm was the installation and starting up of the NA9-experiment: EMC got enlarged by a vertex magnet housing a streamer chamber, several gas and aerogel Cerenkov counters, drift tube arrays, new time of flight hodoscopes and more [36]. Here he could show his comprehension of complex systems and his abilities to organize a joint effort of many people of different labs. His installation plans, well known later at H1, were always very tight. But when he handed over to H. E. Montgomery, NA9 had indeed already taken a lot of physics data.

Although Friedhelm was always well informed, he now found less time to work on the data, compared to the early days at DESY (sections 3,4). The EMC collaboration had to exploit fully his technical capabilities. But there were still physics issues in which he was specially interested. Besides the important topics like structure functions, his personal interest was still directed versus surprising features of the final state. At EMC this was especially the unexpected large yield of protons in the fragmentation process [37], which was first observed with the NA2-experiment and could be studied later with much more detail [38] using the NA9 set-up.

The EMC experiment had a very wide program. It pioneered the measurement of charged lepton structure functions in the muon energy range of several 100 GeV on hydrogen, deuterium and nuclear targets. There were many original results on effects of gluon emission observed in the hadronic final states, hadron multiplicities, fragmentation properties, charm production [2] and a quark search, all not to be discussed here. The DESY group did not participate in the polarized target experiments as technical support was needed in Hamburg for the DORIS upgrade and the research for superconducting cavities for HERA.

7 H1 Collaboration

The time at H1 is too fresh in the memory of many persons, I will therefore not say very much about it.

Already before the formation of H1, Friedhelm started to investigate in detail the detector requirements for structure function measurements at HERA. This early study [40] led essentially to the same calorimeter requirements as specified later in the H1 letter of intent.

In 1984 neither the choice for liquid argon nor for the lead and steel absorbers had been taken yet by H1. Friedhelm studied together with the late Werner Flauger especially a calorimeter based on scintillating glass. As soon as the decision was taken in 1985 for liquid argon, the first prototype calorimeters for the electromagnetic section were built by group F21 and tested, first at DESY later at CERN.

The H1 calorimeter tests at CERN were an experiment of its own and, not surprising any more, the full organization was taken over by Friedhelm.

First test data were obtained at CERN in 1986 with prototypes for the electromagnetic and the hadronic calorimeter sections including an iron/streamer tube tail catcher. The data [41] fulfilled the expectation and led to the final H1 design.

[2] Claus Gössling worked as PhD student with the DESY group on charm production [39].

When in 1986 Friedhelm Brasse was elected as H1 technical coordinator, W. Flauger took over the coordination of the forward calorimeter ("IF") with full engagement of V. Korbel and important technical work by G. Falley, J. Koll and K. Thiele and others. Two PhD students, Jörg Marks and Peter Loch [42] worked on the calorimeters and tests.

Friedhelm now had to care for the whole H1 detector, and in April 1991 most of the detector was ready and took cosmic data whilst waiting for HERA beams. When *ep* collisions were provided in May 92, the detector started to record more interesting events, and first results could be presented in August [43].

A significant part of the credit goes to Friedhelm.

8 Final Personal Remarks

It is nice to work with Friedhelm as he always has a very strong opinion on what should be done and chooses straight ways to reach his goals. Of course this could occasionally lead to slight problems if somebody else believed to know what should be done as well, as Friedhelm never avoided conflict if scientific progress seemed to require it. But as far as I can see, he does not resist to clear and convincing arguments, whereas prevarications irritate him a lot. This had to learn also the around 20 diploma and 16 PhD students, in his hands during the last 30 years, before they learnt other things on physics and techniques.

Some people worked with him for more than a decade already before the time of H1 (G. Falley, the late W. Flauger, J. Koll, V. Korbel, G. Singer, K. Thiele, H. Weiss and myself). But some collaborators were lost because they were just too good, as e.g. our secretary of the seventies Karin Schmöger, who is trapped in the directory since then, when H. Schopper needed some replacement during holidays.

Usually elder physicists bother about the general lines only and leave the details to others. Not so Friedhelm. It is characteristic for him that he reached retirement without being part of the "wise men commitee" of H1. He stayed to the end of his time at DESY fully engaged in the daily struggles on all levels (e. g. at April 28 he was working hard to get the H1 coil operational) and in the work for the future, both for H1 (e.g. [44]) and in committees like LHCC at CERN or for HERA-B at DESY.

Friedhelm will be missed, especially by H1.

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