Review of the Experiment Proposal
"Measurement of the high energy contribution to Gerasimov-Drell-Hearn Sum Rule"
Spokespersons: J. Stevens, A. Deur, M. Dalton, S. Sirca

Frank Nerling, GSI & GU Frankfurt
Garth Huber, University of Regina

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Charge: We ask you to report your findings back to us (the Collaboration Board) about the merit and technical feasibility of this measurement. If the Board and the Executive group agrees that the proposal has merit for the Collaboration, it will be put to the whole collaboration for a vote of support/non-support.

Summary: We are in agreement that the scientific merit of the experiment is high, and that the measurement is technically feasible. However, we recommend significant changes to the structuring of the proposal, to better make the scientific case for the measurement to the PAC. We have communicated some of our suggestions for improvement to the experiment proponents in a BlueJeans meeting. Once they have completed the suggested improvements, we recommend full approval as a Collaboration supported experiment.

Merit: The GDH Sum Rule, a measurement of the spin difference cross section for photon+nucleon spins parallel minus anti-parallel, integrated over the energy range from 0 to infinity, is a fundamental quantity of hadronic physics. While the majority of the GDH integral is determined from low energy measurements, the un-measured high energy region above 3 GeV has much importance. The current experimental status is that the GDH sum rule is over-saturated, so for the GDH integral to take its expected value, the spin-difference cross section must become negative over a substantial energy region. The current data support this with poor statistical significance. The primary goal of the experiment is to see whether this sign reversal in fact occurs, as predicted by Regge theory. Finding whether Regge theory fails in the high energy spin sector would be a very significant result in itself. But if the Regge theory indeed fails, then the level of failure itself opens up a variety of explanations, including whether quarks are composite (i.e. have an anomalous magnetic moment) at the multi-TeV scale. Measurement of the high energy portion of the GDH Sum Rule has very high scientific potential and is a "must do" measurement.

However, one of the issues in the proposal as it is written now is that the overlap between the existing low energy data (from Mainz, Bonn and LEGS) is not very prominent. We recommend that the experiment proponents consider investing more time to improve the degree of overlap between the proposed and existing measurements in the 1-3 GeV range. The neutron GDH Sum Rule measurements are in particular of low quality, so additional low energy measurements are in themselves of high importance to confirm the existing data. But in addition, by providing a greater degree of overlap, various experiment systematic checks can be done, which would increase the degree of confidence and establish the reliability of the higher energy data. Such measurements should be possible in the <5.5 GeV linac runs that JLab foresees in future summer runs, such as was scheduled in 2019.

Technical feasibility: The experiment proposes to use a new frozen spin target constructed for Hall D, based on the Hall B FROST design. It is our understanding that the JLab Target Group is eager to work on such a target, which would be capable of handling an order of magnitude higher photon flux than the Hall B target, and with a larger angular range (up to 160 deg). The Hall B target flux is restricted in part by the need to accommodate transverse target polarization, which would not be the case with the Hall D target. The combination of higher solenoid field, and improved refrigerator, should allow the intended target improvements with low technical risk.

While testing whether the GDH Sum Rule will converge, or not, require only the use of un-normalized spin difference, the other scientific goals aimed for require absolute normalization. We encourage the experiment proponents to develop this capability, as it is expected to have significant scientific dividends. We do not see this as a long-term stumbling block, as 1) the experiment has a simpler experiment trigger than GlueX, and hence the experimental acceptance issues should in principle be easier to understand, and 2) the experiment will benefit from the continuing efforts by GlueX over the next few years as the polarized target is developed.

We see a future frozen spin target for Hall D to be a very promising development which opens up a myriad of promising scientific investigations, of which the measurement of the high energy portion of the GDH Sum Rule would be a very important first part of an extended spin-physics program.

Below are our specific comments to the spokespersons on the proposal content.

I) Chapter and page-wise referring to the proposal

Chap.1:

a) A Feynman graph would be very useful
b) Even though never clearly stated we guess what needs to be detected is
i) a hadron and
ii) a recoil proton or a neutron
This should simply be clarified once.

Fig.1 Since this is a key figure, maybe
--> larger & better quality
==> The one from the APS Talk is better!

Fig.2 Is this a compilation of all data presently available?
The quoted Ref. is a review article dated back to 2006
==> The latest LEGS data to be included

Related to Fig.2 and later in the document:

o The mentioned improvement of the integral determination (94% ± 16 μb → ± 12 μb) needs better to be explained

o Higher emphasis should be given to the overlap with ELSA for proton

o No overlap is presently foreseen for deuteron, why?
--> we think it is important and even crucial, cf. also later

==> Additional Integral plot of APS talk and better explanations needed,
especially a "graphical illustration" of the 25% improvement

Chap.2:

pg. 14: At the end of Sec. 2.1.3:

... 250 nm position difference can be tolerated ..

--> Is this realistic to achieve, and to what false asymmetry would such difference correspond to?

--> This adds some systematic uncertainty, we guess, not yet mentioned or covered in Chap.6?

At the beginning of Sec.2.2 the two target options are discussed, however, the target dilution of HDice is not quantified nor the sustainability for "a high photon flux"

==> brought up on Mon, authors tend to agree

pg.14/16 Complete information on the HDice target would strengthen the argumentation for the FROST target, we guess? (omitting numbers here looks strange).

==> brought up on Mon, authors tend to agree

pg.17 A 12C foil is proposed to correct for dilution by unpolarized material.

--> Can this be better explained how and what about systematics introduced here?

pg.18 Parameterisations of spin structure g1 and g2 are proposed for correction

--> How accurate are those, what about systematics introduced here?

Drift chambers are mentioned for identification of the tagged photon ...

--> shouldn't there be a connection be made to Fig.5, the explanation is not very clear here.

pg.19 Also the Drift chambers are mentioned to identify individual channels, which are mentioned several times here and there ...

--> Can you specify which individual channels you have in mind for instance, and also make clear for what purposes they are needed / planned to be employed?

You give quite some numbers of solid angles for LEGS and ELSA, and claim comparability for Hall-D, could you (more) clearly quote the information what the Hall-D solid angles are?

pg.20 The ν value characterizing a given event ...

--> This sentence is badly and misunderstandably phrased (i.e. ν cannot be measured using the tracking info)

You state that the accidentals are below 25% which is in line with Fig.5, but a priori 25% is a huge contribution, so why this "does not cause significant issues" needs to be explained/argued.

Also no mentioning in Chap.6.

Sec.3.2:

pg.20: What is the threshold on the energy deposit in the calorimeters used for triggering

Sec.3.3:

pg.21: Again exclusive events are mentioned, for which a kinematic fit can be applied ...
--> Again, pls. specify at least exemplarily which channels you have in mind and what and how to learn from them (just systematic checks is too vague).

Chap.4:

pg.21: According to which model events are generated
pg.22: Especially for the resonance region some more details or a Ref. to be added.

Chap.5:

Fig.9: Why not including the fit that is mentioned in the caption?

pg.25: ... results in Tab.5.5 --> should be Tab.1, we guess?
pg.26: ... Tab.1 Caption: ... Fig.5 .. --> should be Fig.11?

Fig.10: The large gap below 3 GeV should get more emphasis,
cf. also our general comments

Chap.6:

pg.28: Fig. 6 shows merely statistical errors, but ok if meant systematic errors on detection efficiencies can be estimated using the MC chain.

But what about theta/phi acceptances?
(i.e. the known acceptance holes)

pg.29: If you say that "A need not be measured over the full ν coverage", you make the assumption that the ν-dependence would be flat, which probably is not true, is it?

"Alternatively, ..." --> Clearly, the acceptance need to be modelled.

Last paragraph on that page: "From the first method ..."
--> We cannot follow and understand how you come to the 25% corresponding to lowering the error on the integral from 16 μb down to 12 μb. This important paragraph and calculation needs to be rephrased and completed so that the reader can follow the steps of the proper calculation.

Fig.13: Again, we think the overlap to the ELSA data, means data below 3 GeV should get more emphasis, especially here:
There might be an energy dependence of the scale factor.
==> Also brought up on Mon, authors got our point ..., even though:
- worries on conflicts with plans by Hall-D
- Question to be clarified about beam time, any limitation for others when running in summer?

Chap.7:

pg.31: ... each of the three Pair Spectrometer configurations.

--> you were talking only about two, isn't it? What is the third one?

==> misleading phrasing in proposal, clarified

Chap.8:

Sec.8.2 We are of the opinion that this part needs a lot of clarification. Can you really determine the Compton amplitude?

One issue is the huge π^0 background orders of magnitude larger than the Compton part.

Fig.14: What do we learn from those figures showing the ν part below 2 GeV, especially the lower figure, also not discussed in the text.

Sec.8.3 The first intro part (pg.34) need better explanations and a better profile.

It seems that here really an impact will be achieved, which however, is not sufficiently well described and explained.

E.g. also the last paragraph (pg.35) is just repeating the impressive factors of about 30 and 10 mentioned earlier already. This topic should be better explained and profiled.

==> In APS talk on Mon different factors were quoted, namely 38 and 14 or so, to be double checked

Sec.8.5 Large paragraph largely not so relevant, knowledge of g1 needs electron beam

Sec.8.7 Big question: Are ±12μb indeed sufficient to constrain quark compositeness?

If yes, would be great ... Anyhow and especially if not move this connection (shortened to the intro part on GDH in general)

Otherwise, it seems this is just a nice physics case for a high precision low ν-range measurement?!

==> Brought up, authors agreed to restructure and try to quantify under assumption of a GDH violation of e.g. 3σ significance

Sec.9 Summary

pg.40: Bjorken's citation is repeated here ..., once is enough.

--> In general, some parts of the summary might be better placed in the introductory part, it seems to us.

==> Brought up, authors agreed

.... Secondary goals for the proposal require absolute normalization.

--> A general impression we get on this 2nd goal is that either it is a goal, and then it should also be quantified in terms of projections of uncertainties, or leave it simply out

All over the document, there are this kind of struggling, namely mention it but saying it is only a 2nd goal (giving the impression that it will simply not be possible)
==> Brought up, authors got our point, rephrasing and minimisation of repetitions to be applied

pg.41: We strongly propose to consider also to take low energy data on the deuteron!
And thus probably to extend the proposal from just 3 weeks to 4 or 5 weeks, accordingly (and anyhow, already due to more emphasis on < 3 GeV data also for proton, and to be more conservative / serious about time estimates).

==> Brought up, authors got our point

II) General important points

o Overlap to "< 3GeV ELSA data" especially also for neutron should have more emphasis and be considered

o The quantification of 25% improvement of the accuracy on the integral needs to be more understandably phrased and explained (also graphically)

o The normalisation of e.g. the proton data to the ELSA data needs to be quoted and explained (Fig.9,10)

o The proposal document needs a major revision, incl. Some re-structuring also to avoid repetitions and improve the motivation as well as ranking of direct more complexe goals

Large part of these our comments we discussed in a dedicated meeting already with the proponents. It was a very constructive, instructive and fruitful discussion. Agreement on most of our points, especially on improvements and additions to the proposal itself were achieved. An accordingly revised version of the proposal will be prepared.

III) Some typos, and other minor corrections/suggestions

The focus was not to check and correct the text, and a careful proofreading of the manuscript is definitely still needed, here just a few things that we already noticed:

pg.10: ... 0.2.9 GeV --> 0.29 GeV
pg.12: ... E is the electron beam energy.
pg.20: ... Hence, they are not expected ...
pg.25: ... Thus, our projected results are ...
pg.30: ... and and ... --> delete one "and"
... no single-polarization asymmetry for ..
pg.32: ... it will provide a photon-point ...
... found to be violated, using ...
... on the % level, the ...
pg.37: ... A good accuracy ...
pg.38: ... implies that quarks have non-zero ...