Dear Editor,

below please find the responses to the Reviewers' comments
regarding the manuscript Ref. No. NIMA-D-12-00051.

With kind regards,

Douglas Higinbotham, on behalf of the co-authors

Reviewer #1:
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> This is a very interesting and relevant paper. It is well structure
> and contains lots of details and information so it can be used in other
> applications. I find the idea very interesting and with a large potential.
> Please consider the following suggestions:

> 1.) Fig.1 The color picture I had was not very clear (mainly the magnet),
> I was missing the location of the beam and target. It might be that a
> black and white version is clearer. I prefer Fig3 much more for clarity
> and information.

We have generated a more legible and appealing picture that also
indicates the beamline, the target cell, and the holding field
assembly, but kept the colouring.

> 2.) L65 The figures are not ordered (from Fig 1 to Fig 6) in this line.
> The information of figures is used for several purposed (geometry
> description and result discussion) and it might be better to split them.

We have split the figures, but have duplicated the schematic picture
of the 7-foil target in order to elucidate the reconstructed reaction
vertex histogram in Fig. 6 (now Fig. 7).

> 3.) L82 How was the coincidence obtain ? What was the typical event rate?
> Is there a possibility of event mixing ?

The coincidences were obtained by using the HRS-BigBite coincidence
trigger (electrons in HRS, deuterons and protons in BigBite).
Typical coincidence rates were between 700 Hz and 1 kHz.
Because the coincidence timing, the PID cuts (see present Fig. 12),
and the track consistency cuts are so clean, the selection
of either elastic or quasi-elastic event samples is very robust,
with virtually non-existent event mixing. (The text was changed
accordingly.)

> 4.) L119 and L120 The criteria described here normally bias towards high
> momentum. How you determine the polarity of the track ?

In general, the polarity of the track is determined by knowing
the orientation of the magnetic field in BigBite and measuring
the dispersive angle of the track in the detector package.
Physically possible tracks can then be selected by software.
In our experiment, an even tighter restriction on the tracks
is achieved by not triggering on minimum-ionizing particles,
leaving deuterons and protons as the sole possibilities.

> 5.) Eq. 3 Why the index i,j,k run without limit and the l index run
> until 7? I also realized that actually the number 7 can vary depending
> on the application.

Typically, 7 is the highest order in x that has been attempted
in optics calibrations of Hall A high-resolution spectrometers,
but such high orders are rarely kept in practice. Indeed,
the highest order that is actually used depends on the application.
There is always a trade-off between how many terms one includes
in order to improve the reconstruction of a given target variable
and how much uncertainty one thereby introduces (danger of unwanted
polynomial oscillations). We have omitted the limits on the index l
in Eq. (3).

> 6.) L202 what is the precision obtained for y_tg ?

The geodetic survey that provides us with the absolute position of the center of the target assembly has a sub-millimeter accuracy, typically on the order of a few hundred microns. The relative positions of the individual components of the target cells (e.g. the locations of the carbon foils or the glass walls of the polarized cell) are measured separately, also with a precision that also greatly exceeds the reconstruction capabilities of the spectrometer. (We have modified the text slightly in order to specify the survey accuracy.) The resolutions for y_tg obtained from the real data are presented in subsection 5.1 (present enumeration) below.

> 7.) L232 to 236 what will happen with smaller deviations ?
> Is it possible to show the numerical differences for the two cases
> discussed (100 and 1000 %)

As mentioned above, one needs to compromise on the number of included terms and attempt to find an universal criterion on including or excluding individual matrix elements, ultimately leading to good resolutions. Including the elements that vary by more than 100% does not improve the resolutions; rather, the inclusion of such (invariably high-order terms) may introduce severe reconstruction instabilities. (We have decided to show these numerical differences on an additional plot that shows the reconstructed coordinates of the sieve-slit collimator holes, see present Fig. 9.)

> 8.) Fig 6. The data shown, is the one used for the determination
> of the tensor numbers or is an independent sample ?

The data in Fig. 6 (present Fig. 7) are a subset of all data taken with the multi-foil carbon target which was used specifically to calibrate y_tg. (We have included the histogram that demonstrates the quality of the reconstruction for one of the runs with the empty production cell. See also item 10. below.)

> 9.) L255 The fluctuation criteria is now 20%. Is there any reason for this ?

Yes, the fluctuation criterion could be narrowed down even further in the case of theta_tg. The fit procedure to determine the theta_tg matrix elements was the most well-behaved, and one could reduce the number of matrix elements for it to as little as 31 by the 20% fluctuation criterion. In order not to confuse the reader, we decided to show all results and the numbers of the retained matrix elements with the global, 100% fluctuation criterion. However, as mentioned above, we have included another figure (now Fig. 9) that shows the quality of the reconstruction of phi_tg (or y_sieve, which is y_tg at the sieve) in dependence of the included matrix elements and, in turn, in dependence of the allowed maximum fluctuation. We believe that this substantially improves the clarity of the exposition. (The corresponding changes have been made in the text.)

> General comment:

> 10.) It will be nice to describe the samples used for the tensor
> determination and tests: are they the same ?

Quasi-elastic protons from the multi-foil carbon target were used to calibrate y_tg; the carbon target was also used to calibrate theta_tg and phi_tg when the sieve-slit collimator was in place. In turn, elastic protons and deuterons (from hydrogen and deuterium targets) were used to calibrate theta_tg and delta (momentum). The delta matrix elements could also be determined by quasi-elastic events from He3 under the assumption that the energy losses are well understood. (We have added these explanations to the text just prior to subsection 4.1.)

> 11.) I am missing a more detailed discussion on the bias that
> can be introduced by this technique and how this is not affecting
> your final physics goal. I would have appreciated a Monte Carlo study

> to prove and control some of the criteria for convergency, bias,
> alignment, etc...

The techniques of optical calibration of spectrometers for the type of nuclear physics experimental setups described in this paper exploit a crucial advantage. Using the high-resolution spectrometers in Hall A to tag an elastic electron provides the recoiling hadron four-vector to practically perfect precision. This also holds true for the quasi-elastic parts of the analyses utilizing the sieve-slit collimator which is positioned before the magnetic field region and precisely surveyed (see item 6). This approach is far superior to any Monte-Carlo study and has been previously proven to work impeccably (see Ref. [3]).

> Reviewer #2:
> ============

> Sieve slit calibrations have successfully been used for focussing magnetic
> spectrometers. In this article the authors show that also for non-focussing
> spectrometers this technique can be used. This makes the article a valuable
> contribution to the literature.

> 1.) The sentence 95: "we have developed. " suggests more than it is:
> the analytical method is just the lowest-order transport matrix approach,
> and both this lowest order and the higher order transport-matrix formalism
> is a normal description for (magnetic)spectrometers.

We agree; we have changed the verb "developed" to "considered".

> 2.) The statement in 4.1 :"Field mapping has shown" is an exaggeration:
> Fig 3 in ref 1 shows that the field inside the magnet is about 1/3 entrance
> fringing field, 1/3 homogeneous, and 1/3 exit fringing field. Using
> an effective field boundary is a crude approximation. The shortcoming
> of this approach can partly be corrected by an adjustment of the central
> momentum. And this might well explain the deviation in the missing
> mass spectrum shown in fig. 4.

This is true, but we did not intend to pursue the analytical model to perfection. Rather, as it has been mentioned in the text, it was used to establish a good starting point for the more sophisticated approaches. Getting the analytical model to function to this high accuracy was a formidable feat by itself; getting the missing-mass spectrum precisely right would amount to an adjustment of several poorly known parameters, and knowing the precise effective-field boundaries is just one of them. We did not consider that investing more time in polishing the analytic model would bear much sweeter fruit, so we stopped at this stage.

> 3.) The analytical model is just a first order approximation,
> and it gives a reasonable estimate of the lowest order transport
> matrix elements as a basis for more sophisticated approaches.
> The effect of fringing field (as calculated in ref. 20)
> is incorrectly described: The subscript in fig 3 says that a small
> deflection of 18 mrad occurs if the particle is not perpendicular
> to the effective field boundary, but it is obvious that this is
> the value for trajectories close to the maximum theta acceptance.
> Also the following sentence that the effect is counterbalanced
> at the exit, leaving the angle unchanged, is not true: the size
> of the effect depends on the angle between track and pole face,
> and that is not the same at entrance and exit.

Indeed the angles between the tracks and the pole faces are different at the entrance and the exit, and we should have been more precise in the wording of the figure caption. There is only a partial cancellation of the deflections, and we have modified the caption accordingly (now Fig. 4).

> 4.) The first approximation in line 157 is called : "Direct comparison..".
> It is just a next order approximation, were x and y are decoupled.
> It is clear that the effects like fringing field focussing are neglected,
> and it just takes into account the large angular and momentum acceptance,

> giving a so a better approximation than the analytical description.
> Maybe something like "Decoupled description"" for chapter 4.2.1
> and "Higher order matrix formalism" for 4.2.2 would be more useful.

We agree; we have changed the titles and the occurrences of "direct comparison" in the text accordingly.

> 5.) I would prefer to separate chapter 4 in a part that describes
> the methods (4.1 up to 4.2.2,) and the obtained results, starting
> from 4.2.3. I would have preferred if the authors also show
> an actual performance of BigBite using the simplex and the SVD method,
> for instance in Fig. 6. Just from the first order matrix elements
> as listed in table 2 is not obvious how the two approaches deviate.

We agree with the subdivision of the present section 4. In addition, we have now illustrated the difference between the simplex and SVD methods in Fig. 2 (now Fig. 3, the sieve pattern). This is an even better way to demonstrate the superiority of the SVD approach, which results in a considerably clearer reconstruction of the sieve-slit collimator. The caption has been modified accordingly.

> 6.) In Fig 7 the word in is used twice:" listed in in table 1."

Corrected.

> 7.) In line 326 (and also im line 314 and the subscript of fig. 11)
> the authors claim that the biggest contribution comes from the air
> inside BigBite. Have they investigated the use of a Helium bag
> between target and detectors? And has an additional scintillator
> plane be considered?

Yes, we have considered the use of a helium bag. For typical deuteron momenta during our production running (about 0.5 GeV/c), their total energy losses between the target and the detector are approximately 7 MeV, while they would drop to about 4 MeV by exploiting the helium bag. This was not deemed to be crucial and the bag was not used. (We now mention this briefly in the text.) The third scintillator plane has indeed been designed and built, but it was originally envisioned to fit in front of the detector package that did not include wire chambers. We have used the wire chambers, so any additional material in front of them would deteriorate the angular resolutions. (Since the use of the auxiliary plane is immaterial to the general calibration procedure, we have not dwelt upon this issue in the text.)

> 8.) Finally, I missed the values for the intrinsic resolution
> (angular and spatial) of the wire chambers. They are probably
> much smaller than the obtained resolutions, but it is nice to see
> those values.

Indeed, they are much smaller than the obtained resolutions which are totally dominated by multiple scattering. The intrinsic spatial resolution of the chambers is about 100 and 200 microns for the x and y coordinates, respectively, and about 0.15 mrad and 0.35 mrad for theta and phi, respectively. We have inserted these numbers in the text in Section 2 and made the corresponding comment near the end of present Section 5.