Dear Mr. Jones,

I have received the reviewers' comments on your paper that are appended below. The paper is very interesting and relevant for our readers. I conclude that your manuscript requires a relatively minor revision before it can be reconsidered for publication.

If you decide to revise the work, you must submit a list of changes or a rebuttal against each point raised when you submit the revised manuscript. Revisions that do not address reviewer comments point-by-point will not be considered.

The revision should be submitted by
05 Jul 2022

With best regards,
Christian Joram, PhD
Editor
Nuclear Inst. and Methods in Physics Research, A
Your username is: jonesdc76

Reviewer #1: A. General description
The article is a very specific review of the world data on magnetic properties of iron, nickel and others. These properties are relevant for the Moller polarimetry at Jefferson Lab. A new big project under construction - MOLLER - requires a very accurate measurement of the electron beam polarization - up to 0.4%. One of two polarimeters uses the Moller scattering from a magnetized ferromagnetic foil. The uncertainty of the electron polarization in such a target is an important contribution to the error budget. The polarization is not measured in situ but is derived from the world data. The purpose of the article is to find a lower bound on the polarimetry accuracy coming from the uncertainties in the world data. The subject belongs to the field of instrumentation for nuclear and particle physics research.

B. Originality and quality
The reviewed article extends the research published in Ref.3 (deBever, NIMA 400, 1997). That paper pioneered the method - out-of-plane magnetization of iron foils to saturation for Moller polarimetry. Because of stringent requirements from modern experiments, the results on the electron polarization value and uncertainty are revisited and a more detailed analysis of the world data has been performed. A very thorough review of the world measurements of magnetization in strong magnetic fields is presented. Several findings can be mentioned:

1) Different measurements become consistent to about +/-0.2% if compared properly. One difference with deBever(1997) is the proper definition of the "internal field" value - used in most measurements. This value depends on the "demagnetization effect", which can be understood as the influence of the "return flux" in the magnetized materiel, which depends on the geometry of the studied specimen. The temperature and the applied field dependence of magnetization close to saturation are studied in great detail.

2)The world data on the spin and orbital contributions to magnetization are also studied in great detail (the g' factor). In particular, data on the ferromagnetic resonance (FMR) are included, additionally to the data from the Einstein-deHaas and Barnett-type measurements. It has been concluded that g' in independent on temperature and the external field within the quoted uncertainty.

In total, a considerable amount of work has been invested in this review article. The result is a higher confidence in the expected value of the electron polarization and its uncertainty. On top of this "ideal uncertainty" other effects may contribute, as described in the deBever(1997) paper, for example dependence on the angle between the beam and the foil.

D. Structure and conciseness

The general structure of the article is appropriate. However, there is a room for improvement of its conciseness. The article is lengthy. Along with information-dense parts there are repetitions and redundancies. Some suggestions are offered in the last section, along with the noticed typos etc. If a considerable reduction of the length is required the authors may consider to shorten the part on nickel. Not only the temperature dependence is stronger than for iron, the electron polarization is a factor of 4 smaller, which is a considerable disadvantage for polarimetry.

E. Pictures and references

The symbols in Fig.4 may be difficult to distinguish in BW prints. The other pictures look OK in this respect. The reference list looks extensive.

Made changes to help with grayscale prints. I left the extensive reference list since it is critical to the plot.

F. Typos and other local problems

1. page 2, Eq.1: is E_{CM} is defined as the center of mass energy of the pair, but it rather looks as the energy of one electron in CM, because of the factor 4 in denominator. See ref 4.

Corrected by removing the factor of 4.

2. page 2, line 41+ the >> sign looks odd (typesetting)

Corrected.

3. page 2, line 44, φ_{beam/target} can not be the azimuthal angle of the transverse polarization with respect to the beam momentum since the beam momentum has no definite azimuthal angle. It may be enough to say that φ is the azimuthal angle of the production plane.

Changed to "φ is the azimuthal angle of the scattering plane. $\varphi_{\mathrm{beam(targ)}}$ is the azimuthal angle of the transverse beam(target) polarization"

4. page 16, Fig 3 (and some other figures) typo: citation: "Araj et al" should be "Arajs et at"

Corrected.

5. page 18, Fig 4 upper and lower plots show the same experimental data, also the same as Fig.3 lower plot. The Fig.4 bottom plot seems redundant. The error band can be shown differently. The same comment applies for the plots for nickel

Remade figure 4 to include all information in a single plot.

6. page 19, line 475. Strictly speaking, H is not measured in Tesla. One should use B in this case for the applied field.

Changed to "applied field B_app in Tesla (this is the field of the magnet alone without the added induction of the foil)

7. page 20, line 510 "per iron" ==> per nickel

Changed

8. page 31, many formula 15-19 and those in the text between are redundant or well known. Perhaps, it would be enough to write that g' is measured as g'=2m/e ΔM/ΔJ (in SI), where M and J are the projections onto the magnetization axis. Then, use the relation M=g*e/(2m)*J with g_{s}=2.002.. and g_{o}=1.

We respectfully disagree with the referee, and believe that this detail, even though potentially redundant and very familiar with some physicists, is not at all well known to the nuclear physics community. Therefore we prefer to leave the text as written.

9. page 35, line 791 "material" is typed twice
Removed

10. page 36, Eq.15 uses the Gauss units (c in denominator), while the formula before Eq.23 seems to use SI units. Also the formula on page 45 uses SI units.
Changed equations 15 and 23 to be in terms of mu_B/hbars making them independent of the system of units

11. page 45, lines 1017,1022 It is the magnetic moment, not the spin. The formulas between lines 1020 and 1022 are redundant, using a projection in the right part only. It would be enough to write instead μ_{e}=g_{s}/2*μ_{B} without projections.
Reworked according to suggestions

12. page 47, line 1040 "must be demonstrate" - demonstrated?
Corrected

Reviewer #2: The paper should be published with minor corrections:

- Page 4, line 102. The term "inverts" is somewhat unclear. Can you indicate what are the consequences of this inversion, perhaps indicating a mirror plane/line in Fig. 1?
This information is not critical to the point of the paper. It is our opinion that adding this extra information to the figure will be confusing rather than helpful. Instead we added the following explanatory sentence in parentheses: "(From Eq. 1 one would expect the higher momentum electrons to have smaller associated scattering angles and thus be closer to detector center, but instead we see the lower momentum events closer to center due to quadrupole focussing.)"

- Page 10, Line 259 xi(T) should read xi(s)
Corrected
- Page 10 Line 269 "magnetic charges". A question: Is it accepted language to replace dipole moments by "magnetic charges" (since magnetic monopoles do not exist)
Many authors have pointed out that a term M.n arising in the magnetic potential where n is the surface normal plays a similar role to the bound electric surface charges in a dielectric and have suggested the utility of thinking of this as a magnetic charge density due to this analogy even though it is not magnetic monopoles. I dropped the sentence to avoid confusion.
- Page 17, Line 457 The parametrization is presented like an equation but there is no "=" sign here, which looks strange to me. Perhaps M(H)= 217.76...
Corrected

- Page 20, Line 509. "iron" should be replaced by "nickel"
Corrected

- Page 26. What is the effect of the heat resistance of the interface between iron and aluminum? This can lead to an overall temperature increase of the target. (Probably negligible, but will underline the plausibility of assumptions)
We requested a collaborator (Silviu Covrig) to perform a COMSOL simulation of the target foil+ladder for a single setup close to typical operating conditions and he found a deltaT from heat resistance at the Al/Fe interface to be a few milliKelvin and thus insignificant for

the accuracy of this calculation. His simulation of delta T agreed with our simple model to within 1 deg C for the chosen setup, providing confidence that our simple model works acceptably. In the process, Silviu advised us that it would be more realistic to use as the foil radius 0.325" instead of the 0.25" we were using reflecting the point of contact with the bulk target ladder frame. This increased our temperature correction by less than 1 degree C from 12.1 deg to 13.0 deg at 1 muA. This change required slight modifications to figures 9 and 10 and the temperature corrected columns in Tables 7 and 8.

- Page 30 also probably negligible, but along the same line: The beam will induce impurities over time by nuclear reactions. How much impurity content will these represent after typical operational periods? What about lattice defects by the nuclear displacement?

Impurities induced by nuclear reaction presumably refers to transmutation by removal of one or more protons from the iron nuclei. These e-p processes are measured in microbarns but are IR divergent and thus sensitive to the threshold. However the binding energy per nucleon in (mass Fe56-Mn55) is 29 MeV which is 6 orders of magnitude higher than the threshold for dislodging an iron atom from the lattice, so we focussed on lattice defects assuming that if they are not an issue neither is transmutation.

Lattice defects where an iron atom is permanently displaced from its ideal position in the lattice is more difficult to calculate. For a simple model we tried to calculate the Mott scattering cross section with a cutoff of the displacement energy E_d for an atom in the crystal. In this case the threshold for permanently displacing an atom in the lattice is a few tens of eV. Typical values in the literature for permanently dislodging an atom in a metal lattice like Fe are around 40 eV. The Mott cross section (e-Fe nucleus) is significant with a threshold this close to zero. Further complications arise because each displaced atom will displace others until its kinetic energy goes below threshold so the cross section must be weighted by a multiplicative factor to account for this. Putting numbers in for this gives an effective lattice displacement cross section of ~100 barns which is large but would still take a century of typical 1 muA running to have a significant fraction of lattice displacements. This is not the only process (one could imagine convolving the brem spectrum in the thin foil with the photo-nuclear absorption cross section for example) but it sets the scale and is consistent with our non-observation of the effect. We added a section at the end of the target polarization with the the following text:

"Another potential source of systematic error in determining target saturation magnetization is the effect of radiation damage. If a sufficient fraction of lattice sites are dislodged/damaged this could potentially change the target saturation polarization. We estimated the radiation damage by integrating the Mott scattering cross section from momentum transfer of infinity down to the threshold set by the permanent lattice displacement energy (nuclear recoil energy of 40-eV) weighting the cross section by the number of additional atoms that are dislodged by the initial atom using the NRT method to estimate the displacements per atom \cite{Norgett1975}. This produced a total cross section of order 100 barns. While this effective displacement cross section is relatively large, it would take more than 100 years in our typical 1-μA beam for a significant fraction of the target lattice sites to be displaced. This is consistent with non-observation (to the best of our knowledge) of such an effect in any M\o ller polarimeter worldwide. Given that we have not observed such an effect directly at Jefferson Lab in our extensive use of precision M\o ller polarimeters in both Halls A and C, and that our order of magnitude estimate suggests insignificant fractional damage, we have chosen not to add an additional systematic error to account for radiation damage."

- Page 35, Line 791. "material material" --> material.

Corrected

- Page 38, line 870, "effect Si impurities" --> effect of Si impurities

Corrected