

I have received the reviewers' comments on your paper that are appended below. They are advising that you revise your manuscript before it can be published. If you are prepared to undertake the work required, I would be pleased to consider the revised submission.

If you decide to revise the work, please submit a list of changes or a rebuttal against each point raised when you submit the revised manuscript.

To submit a revision, please go to <http://ees.elsevier.com/nima/> and click "login" underneath the journal title banner. You may then type in your user name/password and click "Author Login."

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On your Main Menu page is a folder entitled "Submissions Needing Revision". You will find your submission record there. Also, the reviewer(s) may have uploaded detailed comments on your manuscript. Click on the "Submissions Needing Revision" from your main menu, then click on "View Reviewer Attachments" to access any detailed comments from the reviewer(s) that may have been included.

With best regards,

Fabio Sauli  
Editor  
Nuclear Inst. and Methods in Physics Research, A

Reviewers' comments:

Reviewer #1: The paper is dedicated to high energy photon polarimetry and describes a polarimeter and its operation in a linearly polarized photon beam. The photon linear polarization was measured using the azimuthal asymmetry of  $e+e-$  pairs produced by photons in an amorphous absorber. The studies of the systematic errors have not been completed yet and the experimental error quoted is about 10% relative, including the statistical error. The method might be useful for several existing and upcoming experimental facilities, for example for the Jefferson Lab.

A sentence in Conclusion (page 9) "..., using first time an analyzing power of incoherent pair production on a nuclei for this purpose" can be interpreted as a claim for priority. The authors might be not aware of a previous work (C.DeJager et al, EPJ A 19, 275 (2004)), which used the same process, but with a superior detection system based on silicon microstrip detectors, allowing to measure accurately the angle of the each  $e+e-$  pair's production plane.

The method presented has a merit of using simple detectors. The price is a lower effective counting rate and potentially larger systematic errors. The beam time needed to obtain the quoted 10% statistical error was not mentioned, while it is of interest.

The paper is mostly written in a comprehensive way, with several important exceptions. I would recommend the following.

1)

First, the English must be improved. Let us consider for example a sentence on page 5:

"In this respect a number of calculations has been done to model an expected dependences and

elaborate a necessary experimental tests for the geometry control."

One should, perhaps, rewrite it as:

"In this respect a number of calculations have been done to model the expected dependencies and elaborate the necessary experimental tests of the geometry."

There are quite many similar cases throughout the text.

2)

Address the priority issue - either remove the "first time" claim in the Conclusion, or explain why the previous work I mentioned could be ignored.

3)

In Ref.1, second line, the page number should be 223 (not 233).

4)

Improve the resolution of Figures 1,6 and 7.

5)

Mention the time needed for a 10% statistical error measurement.

6)

The description of the setup on page 4 states either too much or too little. It is not clear what is a telescope and what is a hodoscope. The structure of the hodoscope is mentioned in the text, but being not shown in Fig.2, this structure remains unclear.

7)

On pages 5-6, in the figure captions and in Fig.3-4, the variables "Z" and "deltaZ" are used. Their definitions are confusing. For example, the variable used in Fig.3 is defined on page 6 as "deltaZ" - "the vertical shift", in the figure caption it is defined as "Z" - "the gap", on page 6, paragraph two it is denoted as "Z", then again as "deltaZ". In both Fig.3 and Fig.4 the axis is labeled as "Z", however different variables are used in these plots. I would recommend to separate these two variable, one should be called "deltaZ" - a half-gap between the telescopes, another as "Z" - the position of the gap's center, consistently throughout the text.

8)

Notations as "app. 0.003" should be replaces either by a full word (approximately or about), or by an appropriate symbol.

In conclusion, I would recommend the paper for publication providing the problems mentioned have been addressed.

Reviewer #2:

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Manuscript NIMA-D-06-00745

F. Adamyan et al.: Experimental study of photon beam polarimetry based on nuclear e+e- pair production in an amorphous target

>> The manuscript is suitable for publication after minor revision <<

This work reports first results of a high-energy photon beam polarimeter that has been developed at Yerevan Physics Institute. It follows up on a detailed simulation study from the same group that was published in NIMA two years ago.

The polarimeter is based on pair production on an amorphous target and exploits the preferential alignment of the pair production plane with the linear polarization vector of the photon beam. The idea has been around for over fifty years (C.N. Yang, Berlin & Madansky, Maximon & Olsen), but its experimental implementation at Frascati in 1962 was somewhat limited in scope. The motivation at the time was the same as that of the Yerevan group, which is to give an experimental test and verification of the standard procedure of using calculated beam polarization values that are based on a theoretical description of the coherent bremsstrahlung technique.

In the following, I append a few comments and suggestions to the authors, more or less in decreasing order of importance:

Abstract, 5th line:

..., applied first time for this purpose.

>> I would suggest to eliminate this, as it is not quite correct.

>> The work of the Frascati group (Barbiellini et al.) in 1962 was

>> based on the same principal, although it was more limited in scope.

Conclusion, 3rd line:

.., using first time an analyzing power of incoherent pair production ..

>> Again, I would eliminate "first time"

Introduction, 3rd-5th line and associated reference [1]:

These include using an oriented crystal as the pair converter and measuring the conversion rate as a function crystal azimuthal orientation [1], ...

Now, reference [1] lists only the theoretical work of Überall, but this technique was actually used once at DESY and this has been published. So this reference should also be included:

>> L. Criegee et al., Phys. Rev. Lett. 16 (1966) 1031.

Fig. 2

>> This figure is very similar, but not identical, to figure 3 of the precursor NIM paper. I am wondering what motivated the authors to change the detector geometry from a symmetrical up/down to the adopted configuration, where the positron detector is below the median plane, while the electron detector is above it at an equal distance. The transverse momenta of the positron and the electron are then equal but opposite, so the effective transverse momentum of the pair should be very near zero. Looking at the NIM article (bottom of page380), one reads:

"The PS-6 detector array is divided into sections which lie above and below the median plane of the spectrometer, so that only pairs with significant transverse momentum with respect to the beam axis are detected."

>> Apparently, this plan has been given up and I wonder why?

That concludes my scientific and technical comments.

With regard to the English, I would urge the authors to employ someone with a native English tongue to polish the text.

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#### EDITOR'S COMMENTS

As indicated by the reviewers, a thorough language revision is needed; this should be easy taking into account that one of the authors is a native English speaker. The general quality of the figures is rather poor: the author are invited to find the reason, and improve their quality in the revised manuscript.