

Date: Wed Mar 30 23:35:00 2005
To: bmohanty@veccal.ernet.in
Cc: bedanga@rcf.rhic.bnl.gov
Subject: Your_manuscript LB10101 Adams

Re: LB10101

Multiplicity and pseudorapidity distributions of photons in Au + Au collisions at $\sqrt{s_{NN}} = 62.4$ GeV
by J. Adams, M.M. Aggarwal, Z. Ahammed, J. Amonett, et al.

Dr. B. Mohanty
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Dear Dr. Mohanty,

The above manuscript has been reviewed by our referees.

The resulting reports include a critique which is sufficiently adverse that we cannot accept your paper on the basis of material now at hand. We enclose pertinent comments.

If you feel that you can overcome or refute the criticism, you may resubmit to Physical Review Letters. Please accompany any resubmittal by a summary of the changes made, and a brief response to all recommendations and criticisms.

Yours sincerely,

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Report of Referee A -- LB10101/Adams

Information on the rapidity distributions of produced particles in pp and A+A collisions is an important topic of investigation, and this letter provides interesting new information on this topic and is therefore worthy of publication in PRL.

It has been demonstrated for pp collisions that the multiplicity distributions in the fragmentation region near to the beam rapidity are independent of incident energy, an observation which is referred to as limiting fragmentation. Limiting fragmentation has recently been shown to be true for A+A collisions by both the PHOBOS and BRAHMS experiments at RHIC. It has furthermore been suggested by BRAHMS that for A+A collisions, the multiplicity per participant pair is independent of centrality in the limiting fragmentation region, while PHOBOS has concluded that this claim is not true. It has been suggested that the breakup of the spectator matter destroys the centrality scaling. The present letter presents the multiplicity distributions of photons in the forward rapidity region. Since photons predominantly result from π^0 decays, the photon measurement provides information on the π^0 distribution and is not sensitive to spectator or fragmentation baryons. The letter presents important first results which indicate that the photon (i.e. pion) yield/participant pair is independent of incident energy and centrality.

The letter is worthy of publication in Physical Review Letters, although there are a number of questions to be addressed and suggested changes, which are listed below.

* As described, the measured gamma-like clusters are corrected in average for efficiency and for contamination. Due to these effects, one might expect that fluctuations in the data are larger than in the model comparisons of Figure 1. An alternative method is to compare the measured gamma-like distribution of figure 1 to the results of model+detector response. Has such a calculation been made? If so it would be useful to comment about this in regard to figure 1.

* In the third paragraph of page 8, ranges of efficiency and contamination are given for different centralities. The variation is presumably over the range of pseudo-rapidity, and should be stated more clearly that this is the case. * The variation with pseudo-rapidity and centrality is presumably due to the variations of incident energy and occupancy. It would be helpful to the reader to have more information, such as the highest occupancies at the most forward rapidity for peripheral and central collisions and the fraction of the clusters which are the result of cluster splitting.

* It's stated in paragraph 1 of page 8 that the cell-to-cell gain variation was 10-25%. Was that before or after correction? If that was before correction, it's surprising that the largest uncertainty of $\sim 15\%$ is attributed to the gain variation.

* In figure 4 the photon distributions are compared to charged distributions. This is misleading and should be noted so, since as discussed in figure 5, the photon distributions are not the same as the neutral pion distribution, and therefore there is no reason to expect them to be similar in magnitude or shape as the charged distributions. If the

charged and photon distributions are to be shown on the same figure, they should be clearly differentiated as e.g. open and solid symbols. I think it would be better to show all of the photon results only together in a single panel to show the energy and centrality independence of the photon result. Then the second panel could point to the similarity with the pp charged result but disagreement with the A+A charged result, but this comparison requires some discussion of how well one would expect the photon measurement to agree with the charged measurement (see below).

* In figure 4b, the 200 GeV charged results are presumably from PHOBOS - this should be noted.

* In figure 4 (and figures 3 and 5), it's stated that the error bars are systematic errors. Are the statistical errors negligible? This should be stated, better would be to plot the total error as error bars, noting that the statistical errors are negligible, if that is the case.

* Similarly, for figure 2 it's stated that the errors shown are statistical and systematic. It should be clarified if the errors shown are the total error of statistical and systematic added in quadrature.

* For the comparison of figure 5, an attempt has been made to convert the photon distribution to the π^0 distribution for comparison to charged pion measurements. This is an interesting and useful exercise, however, the description of how this was done is not very well explained. It's simply stated that "the photon result has been scaled down accordingly to reflect approximately twice the π^0 spectrum." Does this mean the photon distribution was simply divided by 2 to reflect the two photons/ π^0 ? Presumably a Monte Carlo calculation has been made to extract the ratio of π^0 /measured_photon vs rapidity. Was it true that this ratio was uniformly equal to 2? It would be better to use an actual Monte Carlo result and state the actual correction, which would have relevance for the comparisons of figure 4. If the photon/ π^0 ratio is two, then the photon result is essentially the same as the charged pion result, and the agreement with the pp result would suggest that charged pions dominate the charged measurement for pp, but that there is apparently a significant charged baryon contribution for A+A.

* Normally, 80-85% of high p_T photons result from π^0 decays. Is the higher ratio of 93-96% stated in the letter due to the low p_T threshold of the photon detection?

*** Grammatical suggestions:

* In general the use (or lack of use) of articles (the/a) could be improved throughout the text.

* remove or replace "finite" in ...PHOBOS observed a finite centrality dependence...

- * Further insight "into" this question...
- * replace "to 0 to 80% of Au+Au hadron cross section" with "to 0 to 80% of the Au+Au hadronic interaction cross section"
- * The systematic errors on N_γ are determined to be "due to": (a) Uncertainty...
- * replace "However, within the systematic errors it is difficult to make a firm conclusion." with "Within systematic errors, the two models are in agreement with the measurement." or equivalent.
- * I think that it is more conventional to use "distribution" rather than "spectra" when describing rapidity distributions.
- * Figure 3 uses undefined N_γ -tot for y-axis label. Presumably this is just N_γ .

 Report of Referee B -- LB10101/Adams

The paper deals with inclusive photon production at forward pseudorapidity in Au+Au collisions at $\sqrt{s_{NN}} = 62.4$ GeV. Results are presented on centrality dependence of the pseudorapidity photon density and comparisons are made with similar measurements of photons, identified pions and charged particles at various energies in nuclear and pp collisions. The number of photons per participant is approximately independent of centrality in contrast to what has been observed for charged particles at mid-rapidity. The authors do not elaborate on this difference and its possible implications. The comparison of the photon yield vs. $(\eta - y_{beam})$ with similar measurements of photons at lower energies indicates that photon production follows LF behavior. The inclusive photons, predominantly coming from the decay of π_0 , are expected to show the same behavior as identified pions and this is indeed what is observed.

While these results are interesting and certainly worth publishing, I do not consider that they are novel or interesting enough to justify publication in PRL. I also find the paper not easy to read and not clear enough in particular in the presentation and discussion of the results. The text would also benefit from careful reading by some of the native English authors with respect to the usage of definite and indefinite articles. I therefore recommend publication as Rapid Communication after editing work and after addressing the points below. The discussion of the centrality dependence of the LF behavior is confusing. The paper properly describes the contradictory results from PHOBOS and BRAHMS in the

introduction. This is again repeated in p.10. But then in the discussion of Fig.5 the authors describe "The observation of the centrality dependence of LF ..." as a fact ignoring the results from BRAHMS which do not see such a dependence. In the summary, the authors write the opposite: "The photons and pions follow an energy independent LF behavior as has been previously observed for inclusive charged particles." and again the opposite: "However photons, unlike charged particles follow a centrality independent LF scenario". Below are more specific comments on the paper:

1. The first paper to discuss the charged particle density vs centrality at mid rapidity is PRL 86, 3500 (2001). I think that this reference should be added.
2. The last sentence of the first paragraph in p.7 could be merged with the first sentence at the top on the same page.
3. What is the acceptance correction mentioned in p.8? Is this a correction for non-uniformity effects of the detector? How was it applied?
4. In p.8, N_{γ} is not defined.
5. "The photon reconstruction efficiency ranges from 42% to 56% in central collisions..." Also a range of values is quoted for the efficiency in peripheral collisions and for the sample purity in central and peripheral collisions. What is the origin of these ranges of values? Are they due to variations along the pseudo-rapidity range? No variations with the pseudo-rapidity range were reported in ref [9].
6. The discussion of Fig. 4 and in particular 4b is very unclear. The various data sets in the figures should be identified with proper references in the figure or in the caption. The text associated with Fig. 4b discusses the results from BRAHMS and PHOBOS but it is hard to identify what data are plotted in Fig. 4b. If the 200 GeV data is from PHOBOS, then the published data are for 35-40% centrality and not 35-45% as shown in the figure. If the photon data are from ref. 19 then they are from 540 GeV and not 546 GeV as indicated in the figure and the text.
7. The fraction of photons coming from π_0 decays as quoted in p. 11 seems too large. Is HIJING reliable to derive such a fraction? To the best of my knowledge, HIJING does not produce eta particles which are the main additional particles contributing to the photon yield after the π_0 .