Department of Energy
Office of Nuclear Physics Report

Addendum II

Response to DOE Recommendations

Nose Cone Calorimeter Detector (NCC) Upgrade Proposal

Attachment B

PHENIX Collaboration's second written response to 15 questions from the Science Review Committee

1) pg. 2-25: what is meant by "calibrating" the detector? Did you run a full Geant shower simulation for a few cases, distributed over pt, eta, PID, then fit the shower distributions and use a parametrization of the shower for the bulk of the simulations? Or were all simulations done using the full Geant showers?

> All simulations were done using full Geant showers (in the PHENIX simulation framework). The results from the detector response section of the code were essentially sub-tower ADCs. Photons, electrons and pions were thrown into the NCC originating from the nominal vertex (at x=y=z=0), uniformly distributed over cos(theta) resulting in a uniform distribution per unit area of the NCC. Energy deposited in the active and all passive elements in every detector segment was used to compute sampling fractions which were then parameterized in two different ways: (1) averaging over all variables except energy (sf = f(E)); (2) averaging only over the shower orientation (hit vs azimuth) resulting in sf = f(E, rapidity) dependence. The parameterized sampling fraction was used to convert the digitized energy in the silicon into total sub-tower energy. Comparing the two approaches show that shower shape measurements, which are the basis for e/h discrimination in the NCC, are dependent on the rapidity. Hence the latter parameterization was applied as a sub-tower calibration coefficient, used to compute the track energies that are employed everywhere in the analysis.

2) How were the embedded particles added to Hijing? One method is to add the energy depositions in the calorimeter towers, then run the detector response and digitization. The other is to add the separate digitizations. Which was done? And was this done for the entire EM1, EM2, HAD towers or was the energy deposition separated for each Silicon layer, then summed?

> The embedding as used in the simulation was done on the level of the energies seen in the detector components prior to the electronics response simulation and digitization. The software offers multiple options built around merging full GEANT(PISA) simulated events from different input event libraries (single particles, centrality classes, jets, direct electrons and photons etc). Depending on the purpose, we can get access to information at the level of individual pads or strips in individual silicon layers.

3) The cluster position resolution in EM1,2 in the text is stated to be 0.8 mm which is inconsistent with Fig.2.9. Which is right?

> We apologize for the typo - the characteristic value for the cluster to hit point rms in the EM segments is in the 0.4-0.8 cm range (as in fig. 2.9). The 0.8 mm stated on page 2-12 should have read 0.8 cm.

4) I have a hard time reconciling the huge contamination in the single gamma distribution in Fig.2.12 for AuAu central with the good 2-track pi0 invariant mass peak in Fig.2.13. How can there be so much false gamma signal in Fig.2.12 while getting a S/B ~ 1 in 2.13? The text says that much of the bkg in Fig.2.12 occurs at forward rapidity. Is the clean pi0 peak in Fig.2.13 a result of the cuts listed in the text (eta < 1.5, E_symm < 0.8) and the bkg in the invariant mass distribution for pi0 which is suppressed by the gamma acceptance?

> Yes. Fig. 2.12 is for the NCC running without a charged veto from the VTX+FVTX, so the suppression of hadron background is done through the shower shape analysis only (which is contaminated by occupancy). At low transverse momenta and large rapidities (close to the beam pipe) in the central events, the occupancy is too high for the measurements of low energy photons. Fortunately, as can be seen in figure 2.6, the occupancy for eta < 1.5 is much smaller that that at eta = 2. Figure 2.13 is for the full acceptance of the NCC, which is the coverage we wish to use for the highest energy gammas, which are rare. Low energy gammas, primarily from pi0s, are plentiful and a more limited acceptance will still give us plenty of statistics for pi0 suppression studies.

5) In Table 3.2 what does "reconstruction eff." refer to? I assume this means the efficiency for finding and reconstructing the primary vertex. Or, does it mean the pi0 reco eff?

> “Reconstruction efficiency” is perhaps not the most appropriate nomenclature. This number refers to a set of QA criteria that reject events that were written to tape before they make it to the analysis stage (the rejection can happen either before, during or after reconstruction). Some examples include: no vertex found, not all the essential detectors are operational, there were trigger problems, beam dumped while DAQ running, magnetic field crashes, etc.. The 80% number is a conservative historical average from past experience, and this efficiency has actually been trending upwards of 90% in the past few RHIC runs.

6) The efficiency procedure described at the bottom of pg.3-25 is unusual. Normally the signal + "data" are embedded, then the combined event is reconstructed and the result compared to the Monte Carlo input signal to determine the efficiency. Why did you have to compare the reconstructed plain Hijing to the combined reconstructed Hijing + signal event first, before studying the embedded signal?

7) In Fig.3.4 why does the efficiency decrease for pt ~ 10 GeV/c at smaller eta? This seems counter-intuitive.

Questions 6) and 7) have a lot in common so we combined the answers. The data discussed at the bottom of pg.3-25 and in fig. 3.4 are for the pi0s with momenta greater than 5 GeV/c, which are reconstructed via the "single-track" pi0 approach developed for the NCC. This method provides a solution to the problem of overlaps but requires a whole slew of tools for calculating the efficiency and handling the background rejection, and some of those tools are still in the development stage. In general what is currently available is optimized for the high pT end of the spectrum. In the intermediate range of 5-10 GeV/c, where the two-track and single-track decay geometries are competing, additional work is required to explain the counterintuitive efficiency behavior in fig.3.4 in that pT range. Zero efficiency at large rapidities and extreme pT is due to the Lorentz boost that results in two photons that fully overlap.

The ongoing development also bears on the procedure as described at the bottom of pg. 3-25. Processing HIJING events through the full PHENIX detector simulation and reconstruction is computationally intensive, so it is very difficult to generate a representative sample which covers the range of pTs to 30GeV/c. Unless this is done properly, the classical way of computing the efficiency as described in the question fails to produce an answer that is valid over that whole range. The "search-and-rescue" approach described on page 2-25 was used to compensate for statistical limitations in the sample, and it is a good approximation, but the whole procedure will certainly be improved when test-beam and RHIC data are available.

8) I don't understand the iterative reconstruction procedure discussed on pg. 3-27. Why is it necessary to iterate?

The question is whether the DRF (detector response function), defined as the point-by-point ratio (observed spectrum / true spectrum), depends on the true spectrum, and the answer is yes. Note that in addition to the resolution, the efficiency also changes with pT, which makes the actual form (shape) of DRF even more dependent on the true spectrum. Let us illustrate this by a simple example. Assume our detector has a constant, Gaussian resolution, which does not even depend on pT, the same width everywhere, and there are no other distorting effects. If we now calculate the DRF with a flat input spectrum, the "observed spectrum" will also be flat, i.e. the DRF will be constant (=1). However, if we calculate the DRF using an exponential input, the resolution will cause the well-known change in the slope of the exponential in the "observed" spectrum, so when we divide observed/true, the DRF will rise with pT. Therefore, the shape of the DRF depends on the input. Of course the "true input" is unknown; so we start with an educated guess on its shape, see how the "observed spectrum" derived from this would look like, compare this to the actual measured spectrum, refine our educated guess accordingly, and so forth.

9) Fig. 3.7 is a money plot. Does it include all the factors in Tables 3.1 and 3.2? What's missing is some idea of the S/B. Are the S/B in Fig.2.13 supposed to be realistic? Do the error estimates in Fig.3.7 include the fake pi0 bkgs in Fig.3.5?

a) It includes all factors in 3.1 and 3.2 plus the acceptance including all pattern recognition. For the low pT pi0s shown in figure 2.13, where the two tracks from a pi0

are distinguishable, a standard Hijing event has plenty of pi0 signal, without embedding. We simply take Hijing events, put them though the full Geant simulation chain, including the detector response and reconstruction, and reconstruct pi0 events. After the cuts described in the text are applied, the S/B is reasonably good: ~0.01-0.10 for higher multiplicity events, depending on the pT slice.

b) The text which describes Fig. 3.7 has an incorrect label (it refers to Fig. 3.3.3, not Fig. 3.7, as it should), but it correctly describes the diminishing S/B for single-track pi0s at high pT due to misidentified photons. Basically, the pi0 and photon spectra from Fig. 3.1, together with the pT dependent reconstruction efficiency for pi0s and the misidentification of photons imply that at around 12 GeV/c you have one misidentified photon for each correctly identified pi0. Fig. 3.7 doesn't include this consideration and merely shows the statistical reach of a single-track pi0 measurement. It assumes one would characterize and subtract the background due to misidentified photons, something which is a significant consideration above about 12 GeV/c.

10) On pg.3-28 the text says the pi0 R_AA uncertainties are 100% for 12 GeV/c. This contradicts Fig.3.7b.

This is a problem in wording. The caption of the figure is incomplete: it should have stressed that on the figure the error bars are only statistical, i.e., the figure shows what limits statistics alone would impose on us. The caption vaguely refers to this ("possible in one run at RHIC II luminosities") but doesn't say this explicitly.

11) Chi_c reconstruction, pg.3-35: Are fake gammas included in the backgrounds, specifically in the direct J/Psi + gamma and the J/Psi from Chi_c + random gamma?

Yes. Since the events are made using the full Geant based simulation, fake gammas coming from e.g. charged particles on top of one another, charge exchange, hard delta rays, etc., are all included.

12) In Table 3.5 what are the numbers of events? I.e., the number of events producing a Chi_c in the pt,eta bin or the number of Chi_c's in the pt,eta bins. Which one or something else? How do the 10K and 5K compare to the yields expected from an integrated RHIC II run period?

The 10K Chi_c+J/Psi is simply the number of signal events in each pT/eta bin. These 10K events are then embedded into 10K background events. We require a dimuon with an invariant mass consistent with the J/Psi in each event. The S/B measured in the PHENIX run-4 data is used to get the background coming from random muons (e.g. from charged pion or kaon decay). The number of Chi_c's thrown is far less than what will be available in the real data, by up to a factor of 1000. We then weight the events appropriately to match the rate expected for a running period of RHIC II. In a real

running period we expect of the order of 10^7 Chi_c's decaying to J/psi+gamma in the NCC+Muon arm acceptance (here the J/Psi is required to decay into a dimuon).

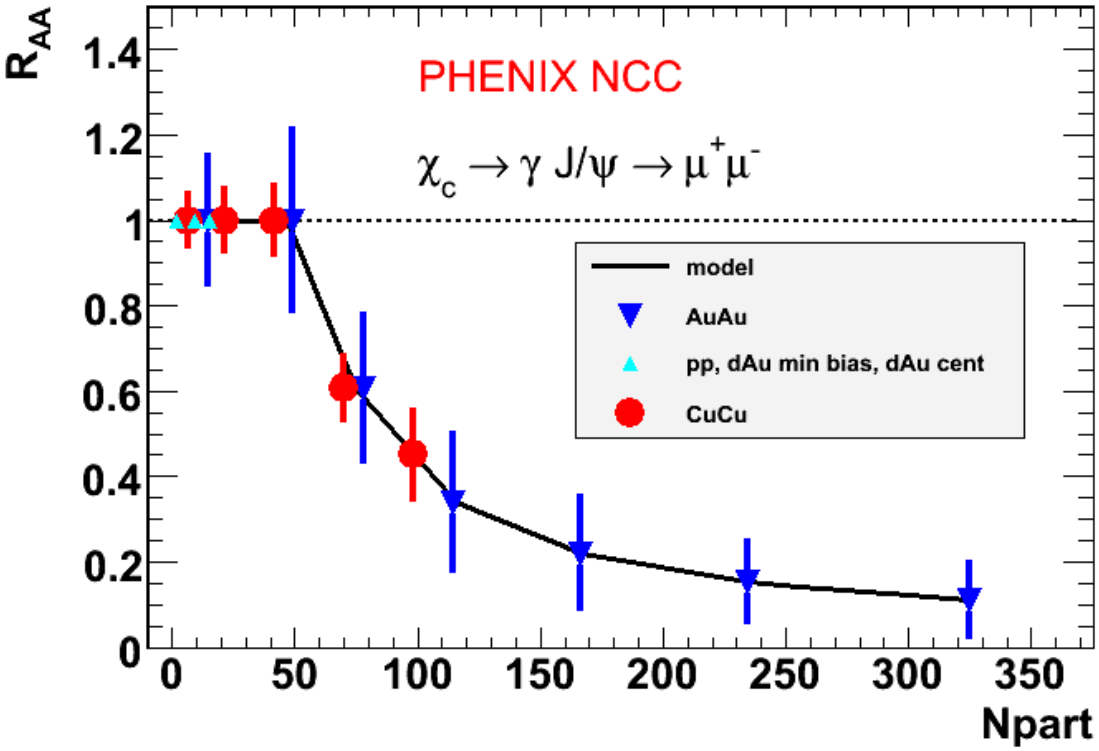
13) I basically don't see how the NCC can reconstruct a typical ~ 1 GeV gamma from Chi_c decay. The shower must be distributed over several towers in EM1 meaning that much of the activated towers will be below the 300 MeV threshold. How can the NCC work for the bulk of the gammas from Chi_c decays?

This is a basic and important question. First, towers with less than 300 MeV can be part of a shower if the seed tower is greater than 300 MeV. Also, we limit the eta range for the gamma to 1-1.5, so that the occupancy for central Au+Au events is reasonable. Cuts on the longitudinal and transverse Chisq are used to discard showers that may come from multiple gammas, or from charged tracks overlapping with gammas. We do lose efficiency because of the occupancy. This is a large effect which makes the efficiency in Au+Au about 5% as compared to a situation in which the occupancy is very small. We will be able to monitor this efficiency for each centrality bin in a variety of ways. One of the most effective will be to look at muons identified by the muon arms, and match a minimum ionizing track in the NCC.

14) Fig.3.15 - The inv. mass distribution for AuAu 30-40% is pretty bad. Do these distributions assume the numbers in Table 3.5? What should be expected for a full RHIC II run data set? Were the PI1,2 used later on in the analysis to get the small errors in Fig.3.16?

The statistics in these invariant mass plots reflect the number of MC events we were able to throw and not the real statistical error we will get in the real data. As mentioned in answer to question 12, the number of Chi_c's that we will get in the real data will be a factor of ~1000 more than we were able to analyze in the simulation. As is, the simulation takes about 1 week to run one set at RCF assuming we are able to run at reasonably high priority (each job is split into about 5K pieces). What we are able to get from this simulation is a good estimate of the S/B we would see in the real data. This, together with the total number of signal events we expect to see, gives us the statistical significance of the result.

15) I am having a hard time reconciling Figs.3.16 and 3.17 with Fig.3.15. In Fig.3.16 the most central point for Chi_c is about 10-20% Xsec yet the text (pg. 3-36) says that reconstruction for centrality >30% cannot be done (in AuAu).

This has gone though some changes in the past weeks since the report was written. At the time we had not been able to work much with centralities of 0-20%. We now show that we can do the measurement up to the most central 10% of events. An updated plot including Au+Au for all centralities, as well as Cu+Cu, is included below. We have improved the analysis significantly. At the cost of statistics we have improved the S/B for the most central events to better than 1/400 (at the time of the report the S/B for the

most central events was 1/4000). Harder cuts on the shape of the shower to exclude multi-shower gammas were used. This results in a loss of statistics as mentioned in the answer to question 13. Other analyses within PHENIX (e.g. the dielectron analysis) have used a mixed event technique to extract signals where the S/B was ~1/1000. It is reasonable to expect that in this analysis a mixed event technique will enable us to extract the signal, since the S/B is better than this.