Mikhail Kapishin and the analysis team (MK): answers to questions related to the data analysis. Comments to the paper text will be considered later.

Itzhak Tserruya

I am sending you here only comments on the AN. I am not commenting on the paper at this moment.

L 34: Discuss separately event reconstruction and simulations.

Put the simulations on a dedicated paragraph describing the various types of simulations that were done. How many events where generated? What impact parameter range? Where they generated with fixed vertex? Is the lambda decay done inside the event generator or in the GEANT part?

MK: DCM-QGSM generator was used for simulation of Lambda production in C+C,Al,Cu,Pb interactions. Events in the full impact parameter range produced by the DCM-QGSM generator were simulated. Lambda decays were simulated by GEANT. Events were generated with fixed target positions, but interaction points (vertices) were smeared along the target in the beam direction Z and reproduced the beam spot in X,Y. 10M events were generated per target (C,Al,Cu,Pb) per energy (4 and 4.5 AGeV).

To simulate background in the trigger barrel detector, delta electrons and other charged particles produced by the carbon beam in the C,Al,Cu,Pb targets were traced through the target material and air using GEAN4 simulation.

L 53: one should distinguish trigger that is a hardware event selection and the off-line cuts which are software event selection.

Was there any QA analysis performed for the data selection?s

MK: QA analysis is described in item 7) of the addendum to AN. Namely: a Data Quality Check was implemented between the analysis version for QM2019 and AN-2: experimental runs were excluded with low fraction of multiple (4 and more) track events (see Fig.A5) and with hardware problems in GEM detectors caused by HV trips or failures in readout electronics (see Fig.A6). Different fractions of the run time with HV trips and hardware problems resulted in a spread of the efficiencies for different runs. The data quality run selection is implemented in the present analysis version after QM2019.

The figures are not numbered sequentially and they are together at end of the AN making the reading somewhat annoying.

MK: we do not plan to change the order and numbering of figures in an internal AN. There is a direct correspondence between the figures and references to them in the text.

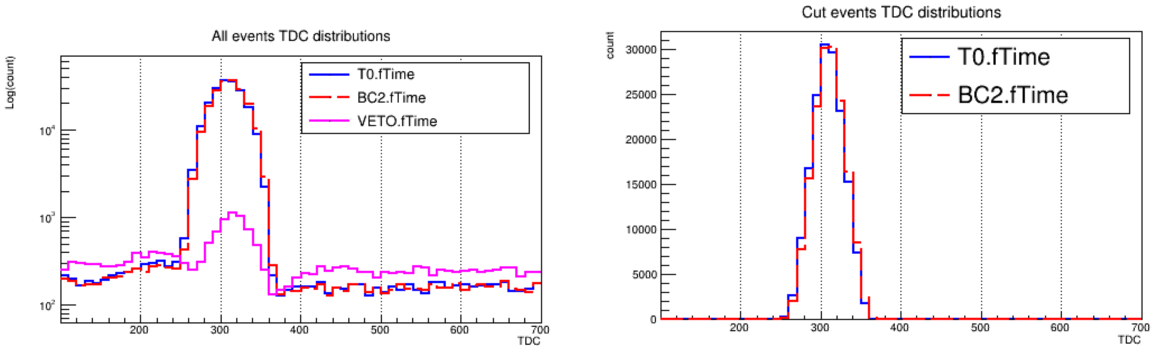
L 53-59: The trigger condition should be explicitly defined in the AN.

MK: Beam trigger: [BC1 && BC2 && T0 && not.Veto] && [BD>=2 (C+C) , BD>= 3 (C+Al,Cu,Pb)]

Coincidence of the trigger element signals should be within 60 ns

Table 1: Specify how do you obtain these suppression factors. It seems that this is an analysis cut. Could you show the distribution on which the cut is applied?

MK: These are software cuts. The factors are the ratios of the numbers of triggered events with a pileup suppression condition: T0=1, BC2=1, VETO=0 within 1500 ns time window applied to all triggered events. The TDC hit distributions for T0, BC2, VETO before and after applying the pileup cut are given in Fig. below.



Left: Multi-hit TDC signals from T0, BC2, VETO counters vs time (ns) in recorded data. The full time window for TDC hit recording is 1500 ns. TDC hits outside the main peak are caused by beam pileup and beam halo. Right: TDC signals from T0 and BC2 after applying the requirement: BC2=1, T0=1, VETO=0 within 1500 ns time window.

Table 2: Specify how do you obtain the numbers in the three different columns, in particular how do you determine the Integrated luminosity. Are all the trigger events recorded? Are all the recorded events analyzed? If not what is the number of events selected for analysis?

MK: Numbers of triggered events recorded by the DAQ system and after QA selection used in the analysis are given in column 1. Numbers of “beam trigger” events (BC1 && BC2 && T0 && not.Veto) which correspond to the number of triggered events in column 1 are given in column 2. These are number of beam events recorded for the DAQ active time, i.e. then DAQ was ready to accept new events. Luminosity values which correspond to the numbers of “beam trigger” events N_beam in column 2 are given in column 3. Luminosity was calculated according to formula: L = N_beam * N_Av * rho_target (g/cm3) * L_target(cm) / A_target

L 74: there was no minimum p required for the positive tracks?

MK: p_pos > 0.5 GeV/c

L 79: Can you plot the "path" distribution of simulated lambdas? From Fig. 7 it seems that you can afford a higher "path" cut

MK: “path” distributions for simulated and experimental events as well as the Lambda path cut selection are given in item 5) of the addendum to AN.

Fig. 6: It seems that you did not apply any cut on the primary vertex. Why? How did you cope with interactions of the carbon beam with the air gas? The SRC group found that the C beam was contaminated with other beams. Is this affecting your analysis? Were any measurements done with target out?

MK: The primary vertex cut was +/-10cm from the target position. The contribution of interactions in air within 20 cm relative to interactions in the targets is expected to be 1.15% for the carbon target, 0.9% for Al target, 0.87% for Cu target and 0.45% for Pb target if the air and target lengths are taken in units of nuclear interaction lengths . The background estimation from experimental data recorded with an empty target show negligible contribution if all the selection criteria are applied.

In the carbon beam run of this analysis a laser carbon ion source was used. It produced clear sample of carbon ions controlled by amplitude spectra in the beam counters showed clear carbon beam signal. In the SRC run "KRION" ion source designed for heavier ions was used to produce carbon ion beam. Satellite oxygen and nitrogen ions present in the "carbon" beam were products of KRION.

L 138-150 I strongly suggest that you follow the approach I discussed with you at the time of the preliminary. I am willing to further discuss it with you.

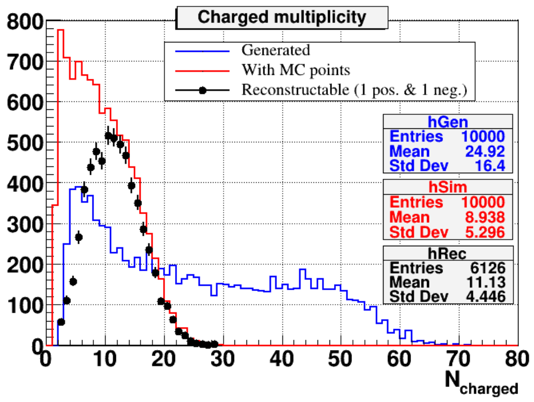
MK: In your approach you propose to apply all the analysis cuts after Lambda simulation and then perform embedding. But if the smearing effects due to embedding are not taken into account than the analysis cuts are idealized. On top of that cuts applied before smearing introduce a bias in the reconstruction efficiency. For example, if the cut on the maximum momentum is 3.9 GeV/c and you apply this cut before embedding than you do not accept simulated tracks with higher momentum, let say 3.91 GeV/c, which after embedding can get momentum 3.89 GeV/c and be accepted. As a result you get somewhat reduced efficiency for tracks with parameters which are close to values of the analysis cuts.

L 162-163 The parentheses need to be fixed.

Fig. 2: the track distribution does not look at all as a minimum bias distribution. This should be understood. Is this distribution obtained from events with at least one positive and one negative track? If yes, please plot the track distribution without this selection.

What cuts are applied to the generated events?

MK: Figure below (left plot) illustrates number of all generated charged tracks per event (in the whole phase space), number of simulated tracks producing MC points in BMN tracking detectors (within the BMN acceptance) and number of tracks with at least three points in consecutive GEM detectors so that such tracks could be reconstructed by the tracking algorithm (minimum one positive and one negative track is required). After implementing detector efficiencies and smearing effects due to Lorenz shift and hit mixing from different tracks in the detectors one gets track multiplicity shown Figure below (right plot). After applying additional requirements on number of hits per track and vertex with at least two tracks one gets track multiplicity spectra shown in Fig 2a and 2b of AN.



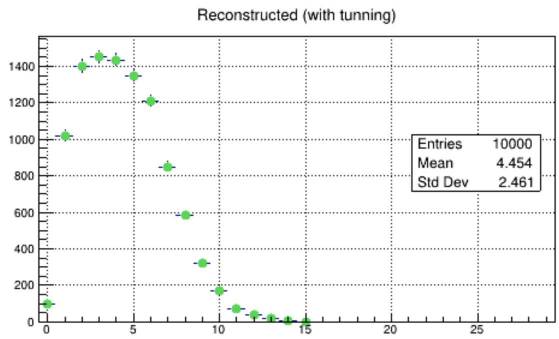


Fig. 3a what are the peaks at p/q = 5 and 10 GeV seen in the generated events but almost not in the data?

MK: These are MC peaks of spectator protons (p/q = 5) and spectator deuterons and alpha particles (p/z = 10). They are not reproduced in data due to losses of tracks in hot zones where the beam crosses the GEM detectors. Lack of proton and deuteron peaks in data was the main reason to implement embedding and analysis cuts on maximum proton momentum < 3.9 (4.4) GeV/c for 4 (4.5) AGeV beam energies.

Fig. 7 Are the lambda decay products embedded in events with a track distribution as shown in Fig.2?

MK: yes

L 168 on: Trigger efficiency. Can you show the plots of trigger efficiency vs b calculated in the same way as described in the text but with any generated event, i.e. without requiring a reconstructed lambda in the event?

All the changes that were made with respect to the preliminary analysis should be described and documented in detail in the AN. This should be best done in a separate section or paragraph.

MK: an addendum is written to the analysis note describing all changes in the analysis since Preliminary.

The rejection of runs, mentioned in the presentation and not in the AN, cannot be based on the quantity that you are measuring, namely on the lambda yield.

MK: experimental runs are rejected from the analysis if in a run there is a drop in the GEM detector efficiencies due to HV trips /hardware problems or if in a run a fraction of “4 and more track” events (at least two tracks are needed for vertex reconstruction and two tracks are from Lambda decay) relative to “two track” events is too low indicating problems with the data quality in a run. The Lambda rates in some of the excluded runs are consistent with zero or are negative.

In the presentation slide 25, you show relevant plots of UrQMD that are not in the AN. How is the selection of inelastic events made?

MK: inelastic events have more than 12(C) + A(target) reaction products. UrQMD generates also elastic reactions. We did not know about this feature at the time of QM2019 and took Lambda yield per event from the generator output. As it is happened Lambda yield per event in UrQMD is normalized to the total interaction cross section taken as pi Bmax^2 of the UrQMD impact parameter distribution.

Arkadiy Taranenko

Please find my comments on the analysis and the first draft of the paper.

I hope that you will find them useful.

Analysis and results

1)I have a big problem to understand how can we have two sets of approved preliminary results, which were presented at two major heavy-ion conferences: SQM2019 and QM2019 and both sets are wrong due to mistakes in the analysis. Somehow I expected to see the detailed addendum to the original analysis note with detailed explanation and discussion inside the collaboration. How can we have the guarantee that the presented results are robust and they will not change in the future?

MK: an addendum is written to the analysis note describing all changes in the analysis since Preliminary.

2) I agree that it is not possible to make a directed comparison with published results for the same systems and beam energies. However, we can make at least a comparison with published results for heavy-ion systems. Apart scaling - see Fig 4 from https://arxiv.org/abs/1812.07304 or Fig 6 from https://pos.sissa.it/311/073 for the excitation function of the integrated yield for Lambda.

MK: We see no sense with comparison of yileds in min bias carbon-nucleus interactions with mid-rapidity yields for central Au+Au(Pb+Pb) collisions: colliding systems are much-much heavier and collisions are central. It is the same as to compare values measured in kilograms and in meters.

3) Is it possible to get the measured proper decay length distributions corrected for all experimental biases and make the comparison with the PDG values?

MK: We will not measure decay length of Lambda, because it requires proper unfolding of the experimental distributions. This measurement is not the aim of the publication.

4) Comparison with the experimental data on carbon-carbon interactions measured at lower energies. Here you use the results from Propane Chamber experiment (JINR preprints from 1983/1985 and HADES for C+C at 2AGeV (PhD thesis). Why should we trust and publish these preliminary results from other experiments? The HADES preliminary results - from the PhD thesis of Kalliopi Kanakiwere were obtained in 2006 and collaboration decided not to publish them in any refereed journal for 14 years! - probably they had some strong reason not to do it?

MK: We will inquire the HADES management to check if they see a reference to this result appropriate. The results from Propane Chamber are published in Sov. Nucl.Phys. 43(1886), 234 (see comments of Anatoliy Solomin who participated in Propane Chamber measurements).

The first draft of the paper:

1) The current draft of the paper looks like a technical analysis note - not the paper for the general reader. The English proofreading is requested. If the target journal is EPJ A - it will be nice to see the next draft in the proper format.

The main problem I have is the physics message of the paper - I do not see it in the present draft of the paper. There some measurements and some comparisons - but no conclusions. Title: "Production of Lambda hyperons in 4 and 4.5 AGeV carbon-nucleus interactions at the Nuclotron: who is Mr Carbon? pion, kaon, carbon.. propsal to change the title to "Production of Lambda hyperons in C+C, C+Al, C+Cu and C+Pb interactions at 4 and 4.5 AGeV with the BM@N spectrometer at the Nuclotron."

Proposal for the abstract = Measurements of inclusive spectra and mean multiplicities of Lambda hyperons produced in inelastic C+C, C+Al, C+Cu and C+Pb interactions at incident projectile energies of 4 and 4.5 AGeV were performed at the JINR Dubna Nuclotron using the large acceptance BM@N spectrometer. Spectra are presented as a function of rapidity and transverse momentum and are compared to predictions of several hadronic interaction models.

The measurements serve as the baseline in the BM@N study of the properties of the .....

These measurements are essential for a better understanding of .....

L8 - the intermediate energy range between the SIS-18 and NICA/FAIR facilities? I propose just to put the energy range for Nuclotron: from 1 to 4.5 AGeV

L64 - I was not able to find the trigger barrel multiplicity BD detector on this plot

L 78-87 hyperon selection criteria - it will be nice to include the figure Lambda decay from slide 5 of the data presentation (26/01) which helps to understand the meaning of the variables and cuts.

You have many nice plots which show good agreement between experimental distributions and Monte Carlo GEANT distributions of events generated with the DCM-QGSM model - proposal to include some of the in the paper

Figure 2 - invariant mass distributions: Can one use the same system of units? In X - axis you have GeV/c2 - in Y axis Entries/ 2.5 MeV/c2 - in the text also MeV/c2. It will be nice to see the background-subtracted invariant mass distributions - at least in the analysis note

Figure 3 - there is no lable for Y-axis In some figures, you use: y for rapidity and some you use rapidity like in Fig 3

Figure 7 - how did you calculate Npart in each of these models? they expected to be different.

MK: Calculation is done in DCM-QGSM model. It is written in the texy and

Alexey Stavinskiy

Feedback on the draft article: Obtaining Λ-hyperons in carbon-nuclear interactions 4 and 4.5AGeV at the Nuclotron

Questions and comments:

1) in the abstract and introduction, the goals of the BM@N scientific program and in General the study of ion-ion collisions are sometimes limited to heavy ion beams. This is generally, in my opinion, an unnecessary restriction, and in this article, in which the carbon beam is just nonsense.

2) in the introduction of energy beams are sometimes specified in the lab.system, sometimes in the SCM. We need one thing.

3) in the first sentence of the introduction, it is better to replace extreme densities and temperatures with extreme conditions, since for the general public, extreme temperatures are now associated with high ones and this may cause confusion

4) the Phrase energies are high enough to study strange mesons and (multi)-strange hyperons produced in nucleus-nucleus collisions close to the kinematic threshold is better given without a red-colored additive, since its semantic load is not disclosed, and the thresholds for different particles are different.

5) Mention in the introduction of recently obtained data on argon and krypton beams, but not discussed in this paper, and especially about future data on gold beams, in my opinion, is not a plus, since the work is devoted to real data. But it would be useful to add a phrase about why the data of this work is needed in the introduction (and here it would be appropriate to say about the birth of hyperons near the threshold). I propose a phrase: In the energy range of several GeV / nucleon, the growth rate of the lambda hyperon formation cross-sections is sensitive to model assumptions about the characteristics of nuclear matter due to the proximity of their formation threshold. Obtaining new data in this area is the goal of the presented work.

6) at the beginning of paragraph 2, the list of detectors in the second sentence does not quite correspond to Fig. One BM, MWPC, ECAL do not appear in the list, but instead they immediately refer to the Central tracker and BD, which is not in the figure. In addition, in the future, data may appear, for example, with ECAL(from the same session), and in the list of detectors from which information was recorded, it is not-this will be a problem.

7) it is better to divide the detector Geometry and trigger configuration into different paragraphs. Only GEM dimensions are given in the geometry. It is assumed that Fig.1 Dan in scale? In General, if this is the first publication on the 2017 session, then shouldn't the geometry be given in detail so that this work can be referenced later. If the geometry is described separately and in fragments, will there be overlays? The same goes for the trigger.

8) the Field is given in the center of the magnet, which is useful for General representation. But for pulse resolution, the field integral is more important, and it is not.

9) Obviously, the lambda region was ignored when performing the polynomial. But I didn't find where it was said.

MK: yes, Lambda signal range was excluded from the fit.

10) the systematics Assessment is made by comparing the polynomial background 3 and 4. In my opinion, 4 and 5 are more logical, since we evaluate the quality of the polynomial 4, and the measurement tool must be known more precisely than the measured object.

MK: 5th degree polynomial is too flexible if one fits signals in (y,pt) bins with low statistics

11) σΛ(y) = Σy[NΛ(y, pT)...σΛ(pT) = ΣpT [NΛ(y, pT)...Is this a typo? Usually with the sum sign the summation variable is specified, it looks like the opposite here?

MK: you are right. It is a mistake in the summation.

Karl-Heinz Hiller

Here my first input for the paper discussion.

I red the comments of Itzhak and in general agree with them. As he I will not yet comment the written text. For my understanding there is still a lot room for clarifications. Before discussing the text I suggest to add some more lines for a better transport of the analysis procedure and the message for physics.

Introduction

In general fine. Since you start with the hint to "nuclear matter at extreme densities" I expect to outline this more in detail. In particular how Lambda production differs in NN and nucleus collisions. What deviations are expected compared to pure NN collisions - T0, yield or what ? To outline this aspect more in detail is also useful for the Monte Carlo section. Here some differences in the generation process of the 3 used generators needs to be discussed. Since some generators describe the data better than others it is of interest which physics is behind.

Experimental set-up

In general fine but too short. Since this is the first BM@N physics paper some more details of the detector components including R/O electronics and DAQ should be given. Also details about beam intensities and other beam parameters are if interest.

Event selection

If trigger details are not given in the section sett-up they needs to be presented here. The calculation of luminosity and reduction factors for clean Lambda events is missing. In the longer backup note there were some reduction factors described. The luminosity is known only to certain precision. What are the errors ?

MK: we put the luminosity uncertainty (2%) into the normalization uncertainty. The estimation is based on fraction of background under signals of T0,BC2 counters used to measure beam flux. The background fraction was estimated from TDC distributions (see plot in the answer to Itzhak’s question to Table 1).

If data taking is included in this section it becomes longer, or a separate section is chosen.

The selection of good Lambda events is quite understandable. Some explanation for the definition of systematic errors should be given.

Monte Carlo

Since efficiencies are calculated by simulation either "Efficiency" is notice in the section headline, or a separate section is opened.

Not only the DCM-QGSM model is used, but as shown later also 2 others. For my feeling they need to be introduced all at the beginning of this section.

MK: Simulation to calculate acceptance and reconstruction efficiency was done only with the DCM-QGSM model. This model gives reasonable description of experimental distributions in control plots.min

UrQMD model does not generate nuclear fragments, i.e. colliding nucleus are dissipated into individual nucleons. As a result the track multiplicity is overestimated by the model. We used UrQMD predictions only to estimate the model uncertainty of the extrapolation factor from the measured kinematical range to the full range. Otherwise UrQMD and PHSD models were used for comparison with experimental results.

The embedding is understandable. Sometimes the text is a bit overcomplicated. Once explained it is sufficient to call such type of events just "embedded Lambda events / or simulation".

The role of delta electrons for the trigger needs some more text, also how the systematic errors are calculated. I trust most of the analysis steps, but cannot really reproduce them. There is some confusion in the term minimum bias events. Either they are min. bias, or events with Lambda particles ...

MK: The systematic uncertainty (2%) covers possible changes in the trigger efficiency for different beam positions inside the BD multiplicity detector as well as contribution of delta electrons to the BD multiplicity taken from simulation. Simulated background in the BD detector is consistent with the rate of BD TDC hits beyond the trigger signal range (see TDC distributions for T0, BC2 counters as examples).

We measure Lambda in events without any cuts on centrality, i.e. min bias events with Lambda.

Results

I would like to see before the tables with the results one for raw data w/o corrections: Lambda signal, luminosity, correction factors, trigger efficiencies and all with errors. When I correctly remind something like that was in the backup note.

MK: Ok

The term "beam-target nucleon-nucleon CM" is unclear for me. What is the aim to transform rapidity into the center-of-mass system ? The text is not really guiding to the target.

MK: “beam-target nucleon-nucleon CM” assumes nucleon-nucleon interaction rather than interaction of nuclei as combined objects (with different masses). This system is commonly used for heavy ions. The aim to transfer rapidity from laboratory to CM system is to compensate Lorentz boost and compare with measurements at different energies.

Here first time the alternative models appear. To compare and to judge physics impact some ore text is needed. For the parametrization over Pt the derivative to Dy should be removed. Integration over the y range was done before.

MK: derivative dY is used to present our results in the way consistent with other experiments. It only scales the result. Usually two dimensional distributions like dN/dPt dY are measured in many Pt and Y bins. In our case there is only one bin on Y with the width of dY.

To use the average of all models for the extrapolations is for my feeling not appropriate. One could do it for all models separately and define a range for the extrapolated values. As the differential comparison indicates the models are quite different and this should not be lost by averaging.

MK: we used the average of two models (DCM-QGSM and UrQMD) for the extrapolation and the difference between the models to estimate the model uncertainty of the extrapolation factors. This model uncertainty is included into the systematic uncertainty of the extrapolated values. It is also possible to use the extrapolation factor from one model and estimate the model uncertainty by comparing with the result from other model.

Fig.4+5: the given errors are unusual. Bars should give the total error, and boxes statistical ones. What matters at the and is the total error which is not given at all.

MK: I agree with you, but the heavy ion community has strong feeling towards different tradition – to show statistical and systematic uncertainties and do not show full uncertainties.

Fig.6: to link more precise to the text one should introduce labels a), b) c) etc.

Feel free to use or to skip the comments. In general I think a longer text

would be more appropriate for a first publication. In next publication the fisrt one can be used as reference for many details.

Anatoly Solomin. Comments on the first draft of the paper

1) About the energy notations:

Throughout the text, figures, tables, etc. the main term for the collision energy should be consistently sqrt(s_NN), which is a contemporary de facto standard for the relativistic heavy ion physics. It is to the point with respect to the physics. However, sometimes for better clarity we can also put in parentheses momentum in lab system, or energy, or kinetic energy (all should read like 4A GeV, not 4 AGeV), if we refer to the results, that were originally published using just those notations.

2) LaTeX:

For further polishing of the paper it is time to migrate to the EPJ LaTeX template and their recommendations. Especially, because the references will float inevitably and the automated sorting and formatting using is a salvation.

That will also bring the "clickable" references of all kinds in the pdf file.

3) Everywhere: V0 --> V^0

4) All Figures:

4a. For better readability the legends should be slightly reworked, in particular, in Figures 4 - 7 the order of explanations should follow the heights of the depicted data as much as possible, of course.

4b. Notations for the colliding nuclei should be consistent, more readable are, obviously, like C+Al, not CAl.

4c. Agree with Itzhak: the Figures should be placed where they belong. Only when sending to EPJ they must be moved to the bottom as that is their requirement.

4d. "lines" --> "curves"

Title:

------

5) definitely, as proposed by Arkadiy.

Abstract:

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6) as proposed by Arkadiy + at the end add:

... models and with the data on C-C interactions from other experiments at lower energies.

Introduction:

----

7) The main motivation for the Λ^0 study should be stated here. Complementary to what Alexey suggests, something like the following can be written (with the references inline):

... are high enough to study strange mesons and (multi)-strange hyperons produced in nucleus-nucleus collisions close to the kinematic threshold [4, 5]. Interest in the study of these phenomena is due to the following. An increased yield of strange particles, compared to the expected background one, is supposed to be one of the first indicators of the phase transition of hadron matter into quark-gluon plasma.

With the enhancement of the strangeness content, the probability for the production of multi-strange hyperons will, in turn, also be enhanced~\cite{Chapline:1974zf, Rafelski:1982ii, Wong:2000if, Lee:2007wr}.

8) Figure 1: BM should be defined somewhere; BD should be pointed to.

Line 64: barrel multiplicity BD detector --> barrel multiplicity detector, BD,

Line 68: in the barrel BD detector --> in BD

Line 66: The carbon beam intensity was few 10^5 per the spill, the spill

duration was 2-2.5 sec. <-- units would be nice, and how does it correlate with the responsiveness of the detector as a whole?

Section 3 Event reconstruction

Line 80: (X and X' views) <-- should be clarified what it is to a reader, otherwise, it can be omitted, perhaps.

Line 88: p and pi- --> ... pairs

Line 107: degree Legendre polynomial --> degree Legendre polynomial instead of 4-th

Line 103: Sorry, I have forgotten: why sqrt(0.5 * bg)?

MK: estimation of the background from 2 times larger mass range used for the background fit than the mass range of the Lambda signal: the uncertainty is decreased by a factor sqrt(2)

Section 4 Monte Carlo simulation

9) Is it true, that Lambdas were embedded in the events where no Lambda was found? Needs to be mentioned.

MK: small fraction of real Lambda signal was subtracted

10) Does using the embedding technique mean that the reliable modeling of the detector response is not made?

11) There should be a purity estimation made, that is complementary to the efficiency. The final choice yeaof the reasonable region for the measurements is typically where the product of efficiency*purity demonstrates a sort-of plateau around its maximum versus ranges of various cuts

MK: The purity in Pt was estimated for Preliminary. It will be estimated once more in 16 (Pt,y) intervals. It is expected to be high because the pt and y bins are wide.

In the (Pt,y) bin with maximum Pt and minimum rapidity y the reconstruction efficiency for C,Al,Cu,Pb targets is in 6-10 times smaller than in the neighboring bins (see Fig.A3 in the AN addendum). This bin was excluded from the measurement. Instead, in the minimum y bin 1.2<y< 1.45 the extrapolation of Lambda yield was done from lower Pt values to the full Pt range using DCM-QGSM predictions. The extrapolation was done in the following way for y bin 1.2<y<1.45:

Ndata(0.1<Pt<1.05) = Ndata(0.1<Pt<0.8) * Nqgsm(0.1<Pt<1.05) / Nqgsm(0.1<Pt<0.8)

Ndata(0.8<Pt<1.05) = Ndata(0.1<Pt<1.05) – Ndata(0.1<Pt<0.8), Err{Ndata(0.1<Pt<1.05)] = Ndata(0.1<Pt<1.05), i.e. the assigned error is taken to be equal to the extrapolated yield value

In the context of the embedded Lambdas, efficiency is (number of the generated AND identified) / (number of GENERATED), while the purity is (number of generated AND identified) / (number of IDENTIFIED).

Section 5 Results

Lines 161 - 162 Arkadiy!

12) Concerning "preliminary" Propane bubble chamber results: [20] and [22] were properly published as "final" results later in English (see blow), except for the less precise results of [21] JINR-P1-83-354. The latter is, however, important, because the method of the calculation of the cross section was different there, and the fact, that the result coincided, although less precise, is a very important complementary confirmation. The reference to it, I give here, is correct, and the Title is from its original JINR's abstract. By the way, their point is wrongly placed at a different E_kin on

Figure 6, while it was exact same 4.2 GeV/c per nucleon lab momentum as in the Collaboration paper, just the same material as in [22].

I'm also sending to the Analysis Colleagues the scans of the two English publications that I obtained from CERN.

….

13) How the measured Lambda quantities were recalculated to the fill ones (Figure 6 and 7 and the tables)? If using the model predictions, based on the comparison with the models only within the actual acceptance (and corrected for the efficiency), but later on, extrapolated to the rest of the phase space, leaving the normalization as it is, then it should be explained briefly. I think, that will attach better "honesty" impression to our results. Otherwise, comparison with the models is not clear.

In other words, it should be unambiguously clear what is what on figures 6 and 7.