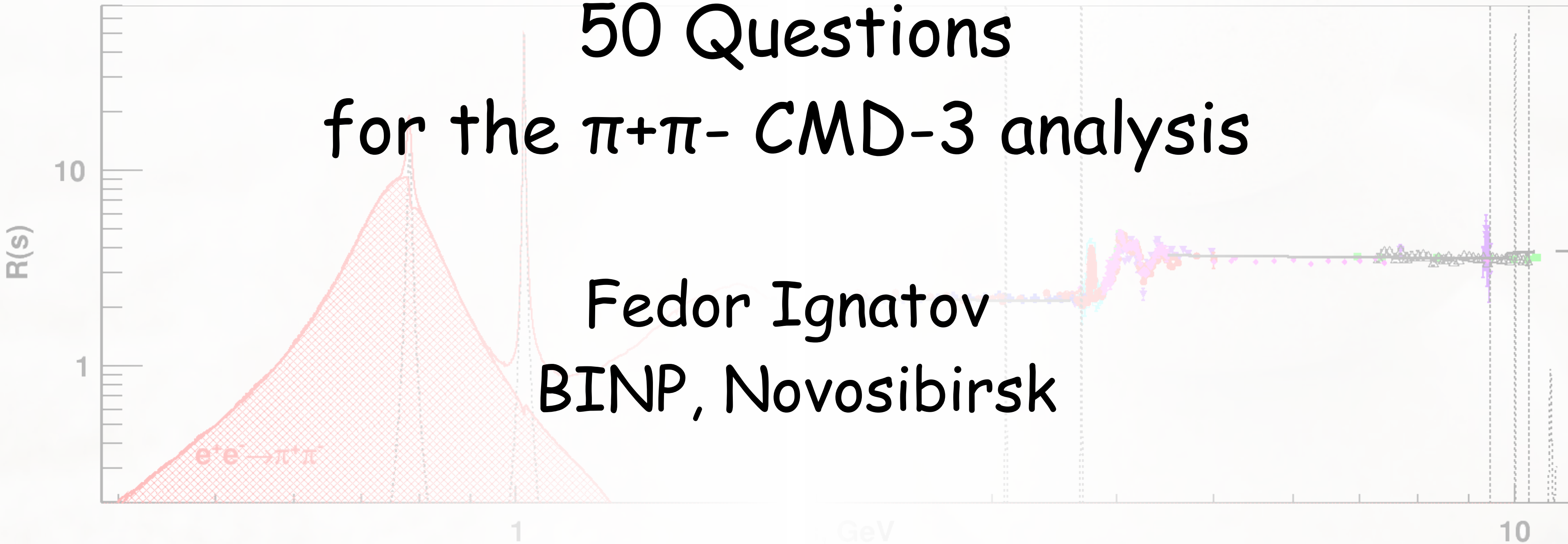
arXiv:2302.08834

50 Questions for the π+π- CMD-3 analysis



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Separation e/μ/π

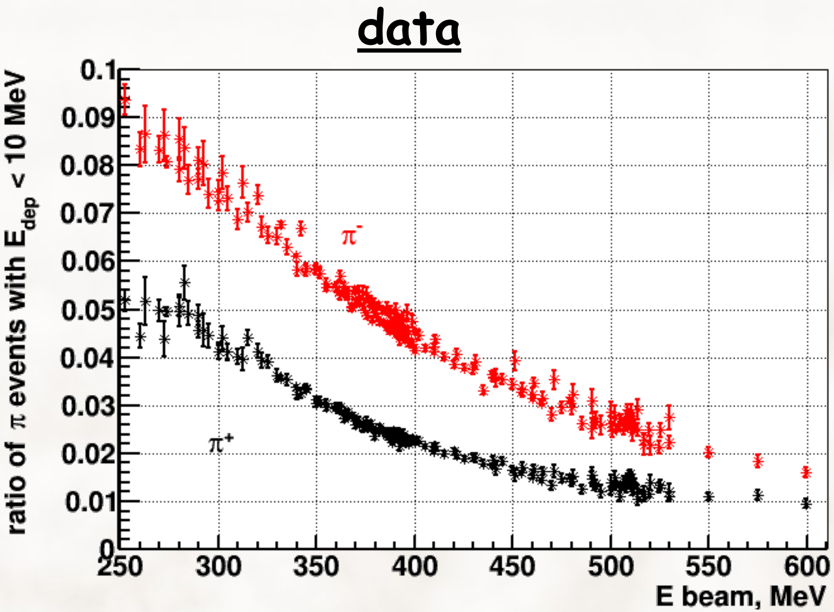
Question 1

1. Energy deposition in LXe calorimeter: is it energy deposition for each of 7 layers or just information about energy in the whole cluster?
Is energy deposition considered to be 0 when the track has no clusters attached?
What is the probability of cluster loss for the data events (and the full simulation)?

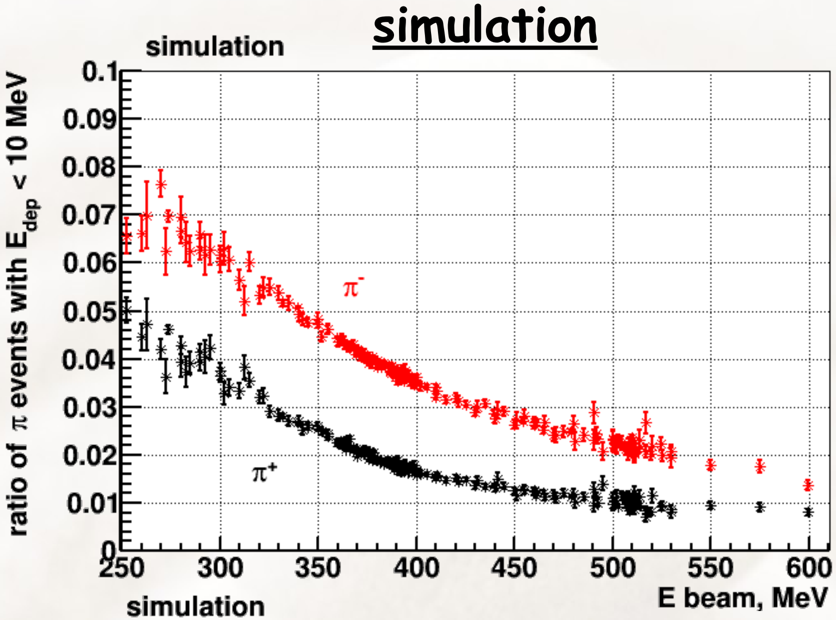
Energy deposition in LXe calorimeter is defined
as sum of "tower" clusters over 0.4 rad angles from track point on LXe and FSR (or bremsstrahlung) photon.
The LXe has different electronics for strips in each layers (256x2 strips per each layer)
for towers (33x8 towers per full Lxe)
Using full information from "towers" and layers is other separation method under development for energies above 1 GeV (using neural network).

If E deposition < 10 MeV (or no clusters) → E_{dep} set to 5 MeV constant value,
and respectively the PDF(E+<10 MeV,E-) = constant x f(E-)

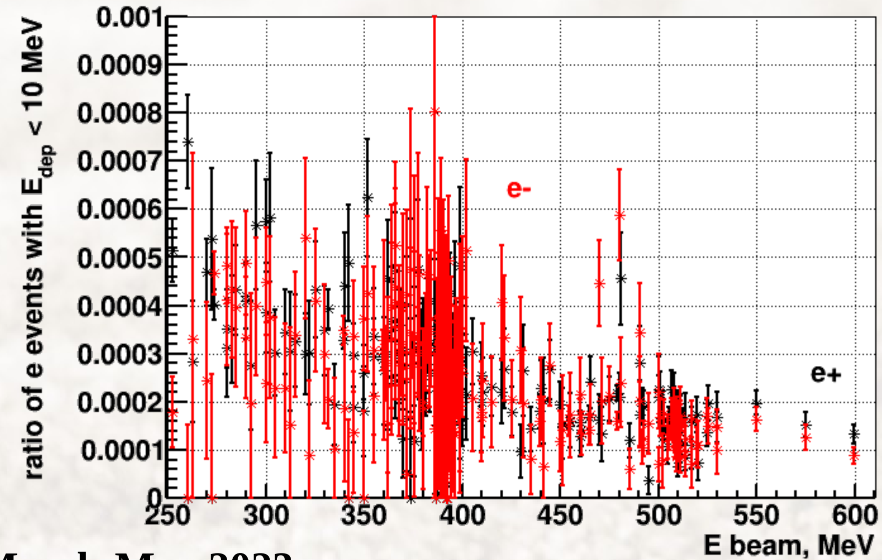
Question 1



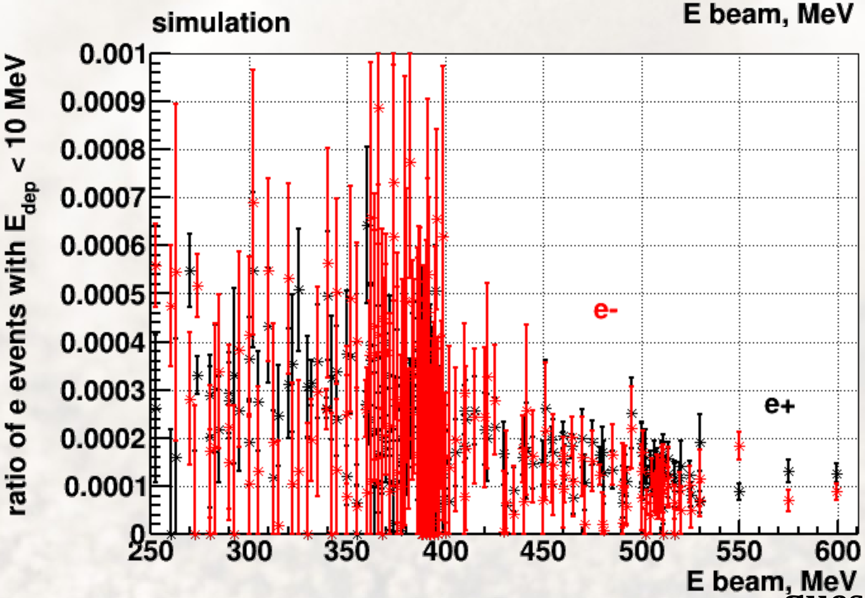
data
ratio of π events with E_{dep} < 10 MeV
E beam, MeV
π-
π+



simulation
simulation
ratio of π events with E_{dep} < 10 MeV
E beam, MeV
π-
π+



ratio of e events with E_{dep} < 10 MeV
E beam, MeV
e-
e+

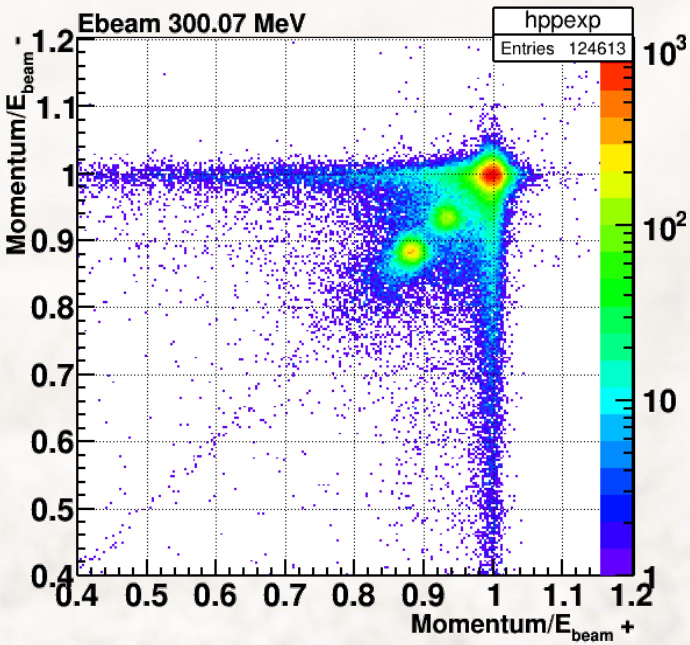


simulation
ratio of e events with E_{dep} < 10 MeV
E beam, MeV
e-
e+

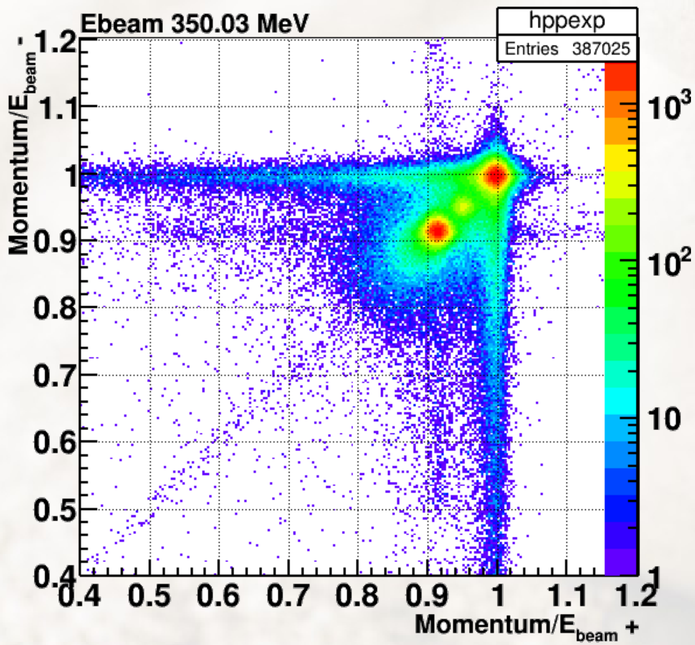
Question 2 (from short list)

Fig.3-4 show 2D-plots for the momentum and energy deposition methods at 2 CM energies, one where each method work best (0.5 GeV for momentum and 0.956 GeV for energy) and the other at their limit where they do not perform well but are still used (0.9 GeV for momentum and 0.548 GeV for energy). In the comparison with other experiments the problematic region is 0.6 - 0.8 GeV. Need to see the corresponding plots at these energies, i.e. 0.6, 0.7, 0.8 GeV.

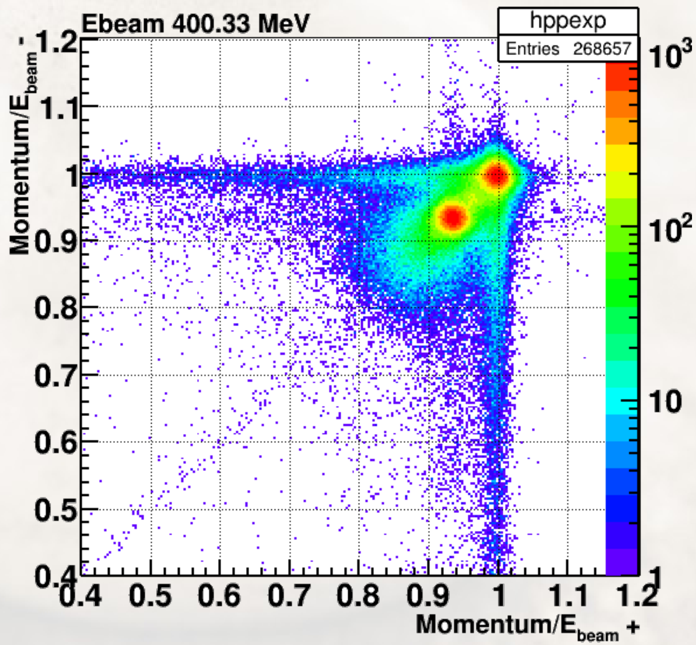
0.6 GeV



0.7 GeV



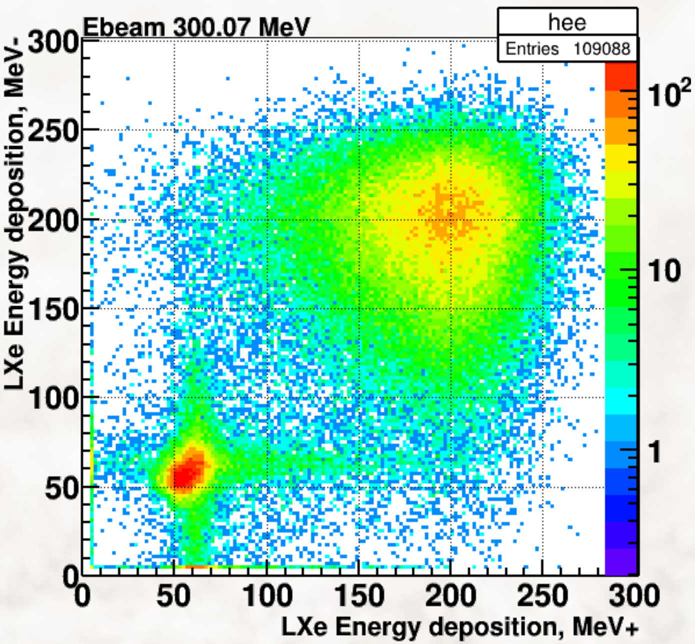
0.8 GeV



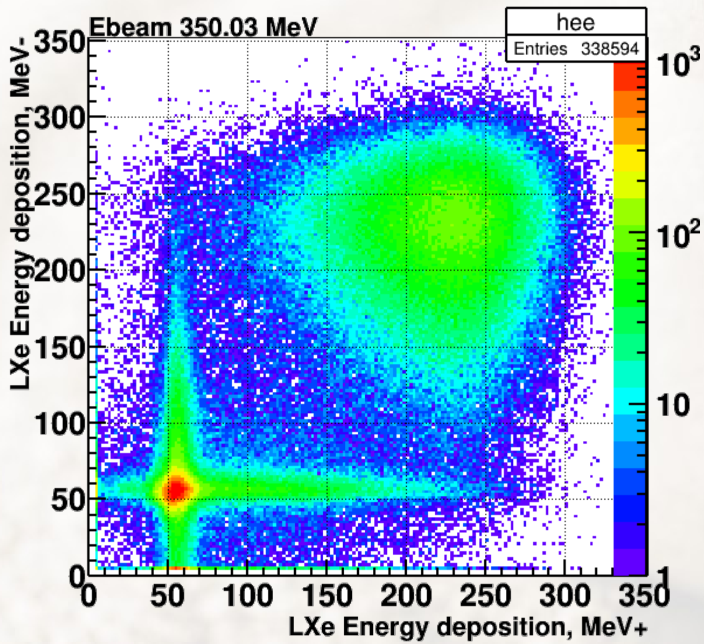
Question 2

Fig.3-4 show 2D-plots for the momentum and energy deposition methods at 2 CM energies, one where each method work best (0.5 GeV for momentum and 0.956 GeV for energy) and the other at their limit where they do not perform well but are still used (0.9 GeV for momentum and 0.548 GeV for energy). In the comparison with other experiments the problematic region is 0.6 - 0.8 GeV. Need to see the corresponding plots at these energies, i.e. 0.6, 0.7, 0.8 GeV.

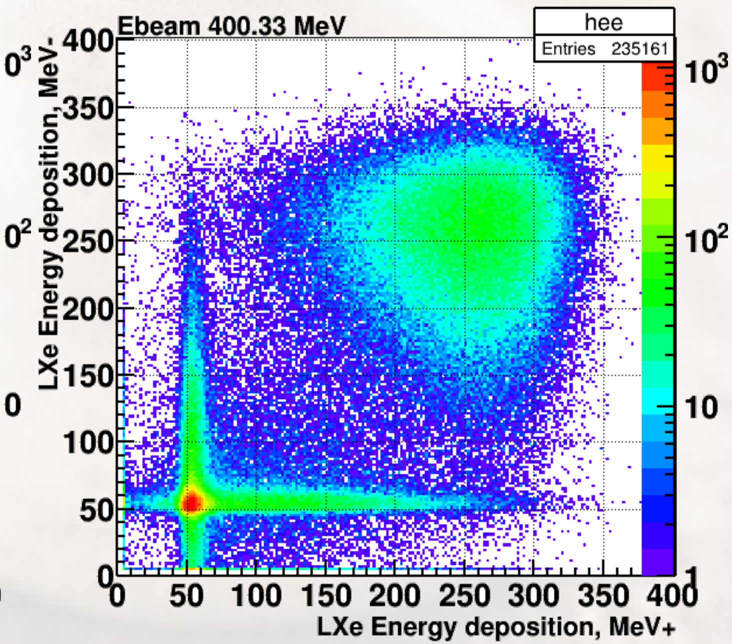
0.6 GeV



0.7 GeV

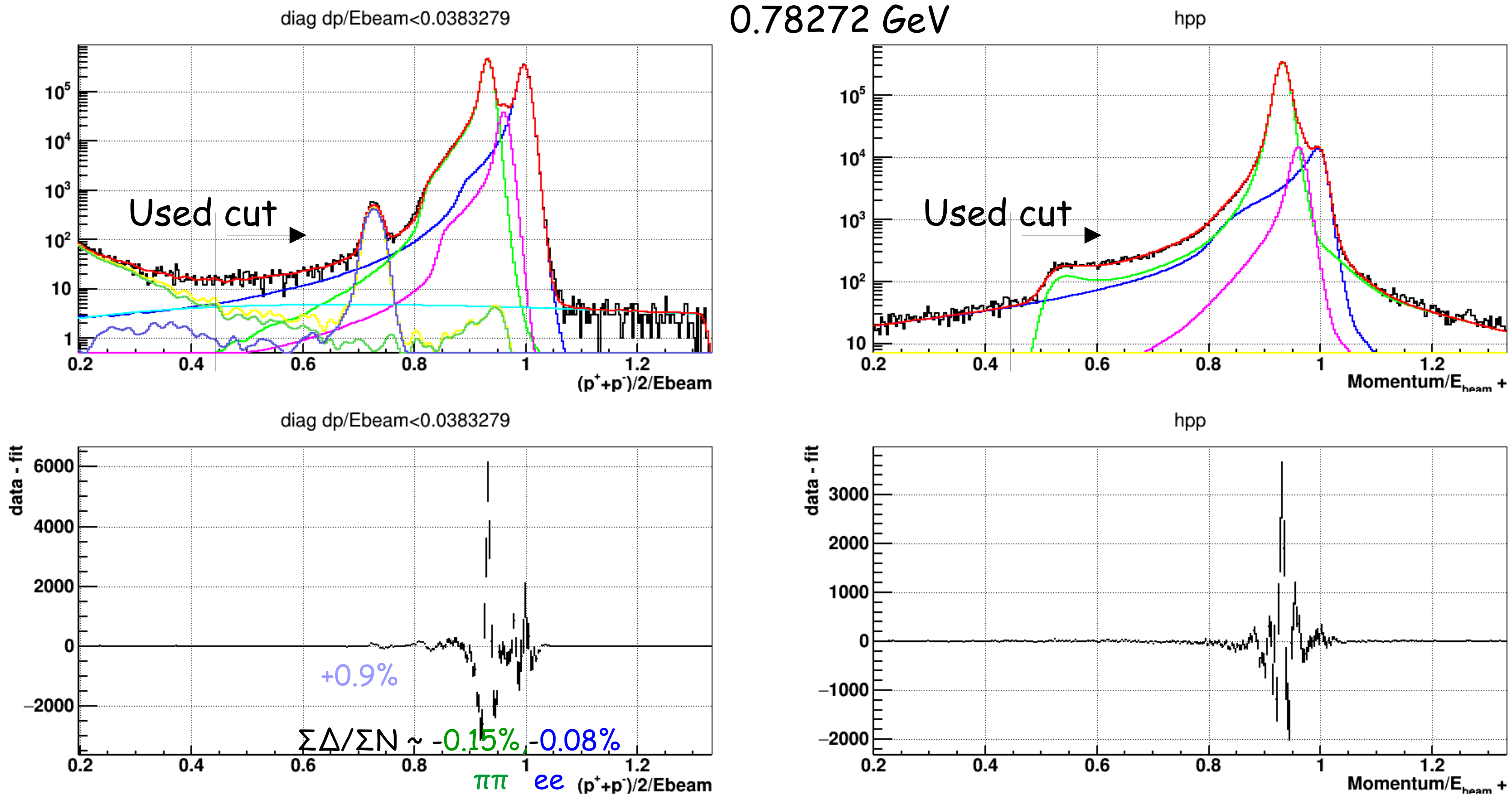


0.8 GeV



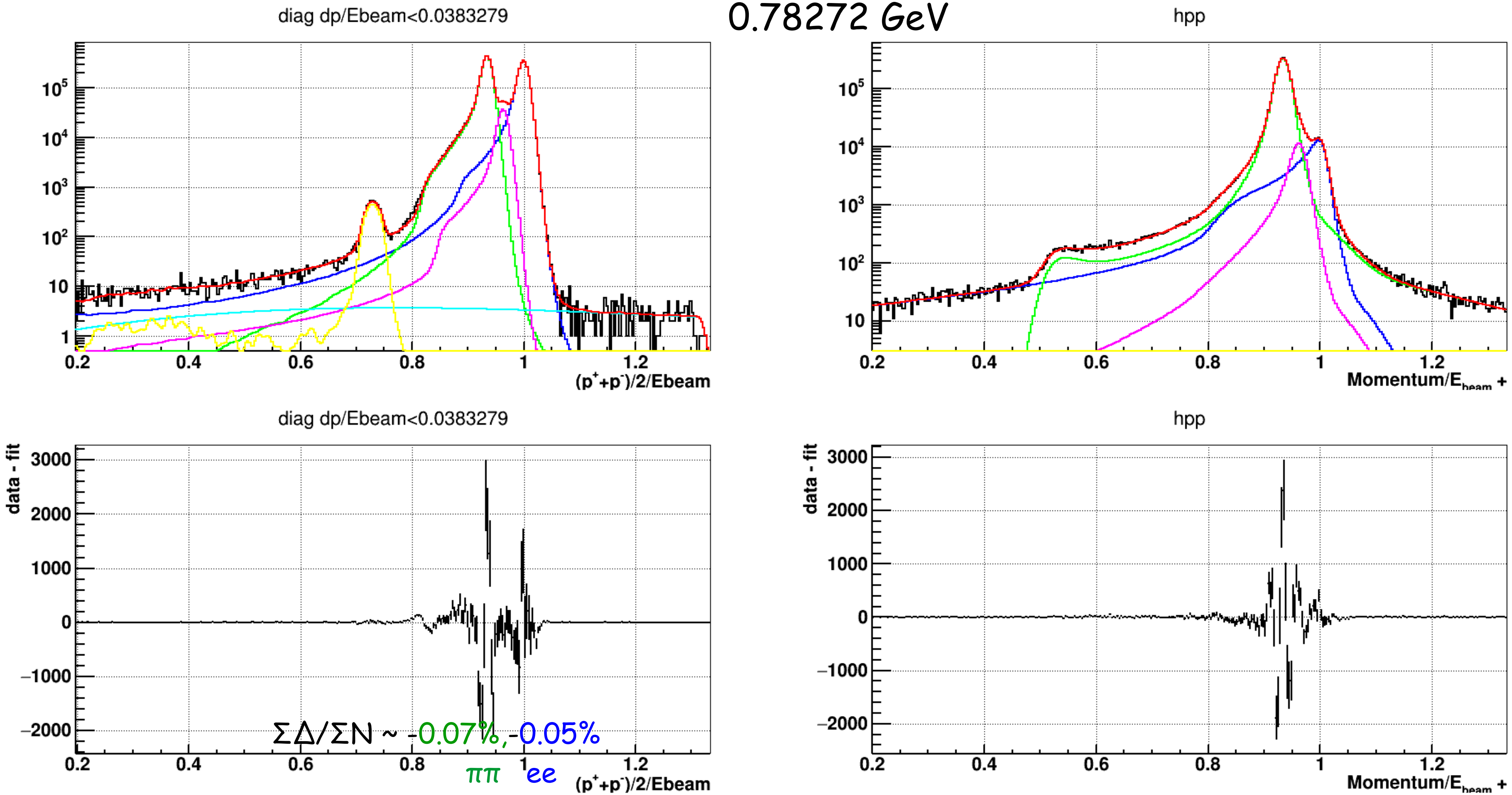
Question 3

3. Could you show the residuals for Fig. 6 and 7?

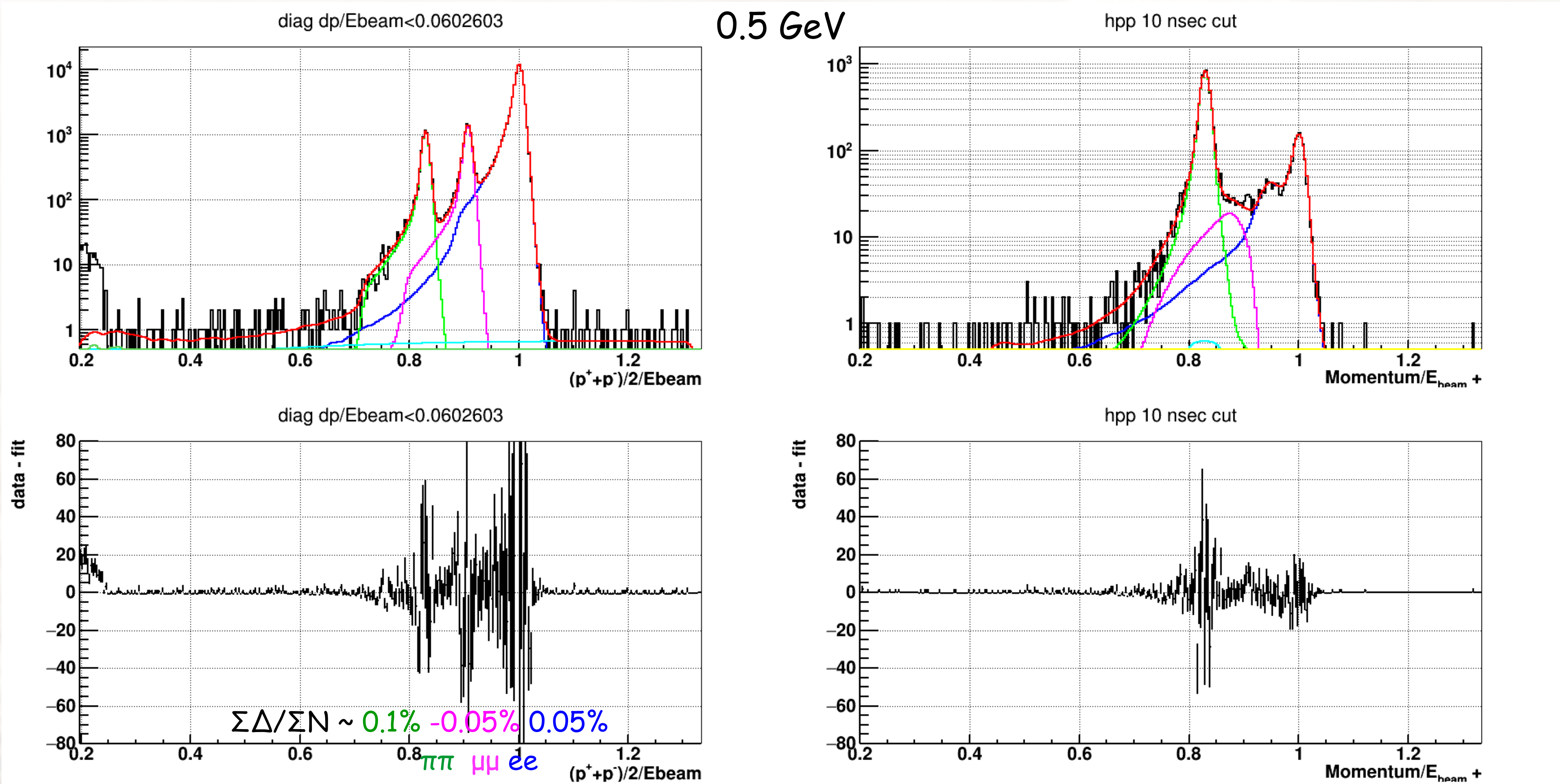


Question 3

Simulation have similar behaviour in residuals

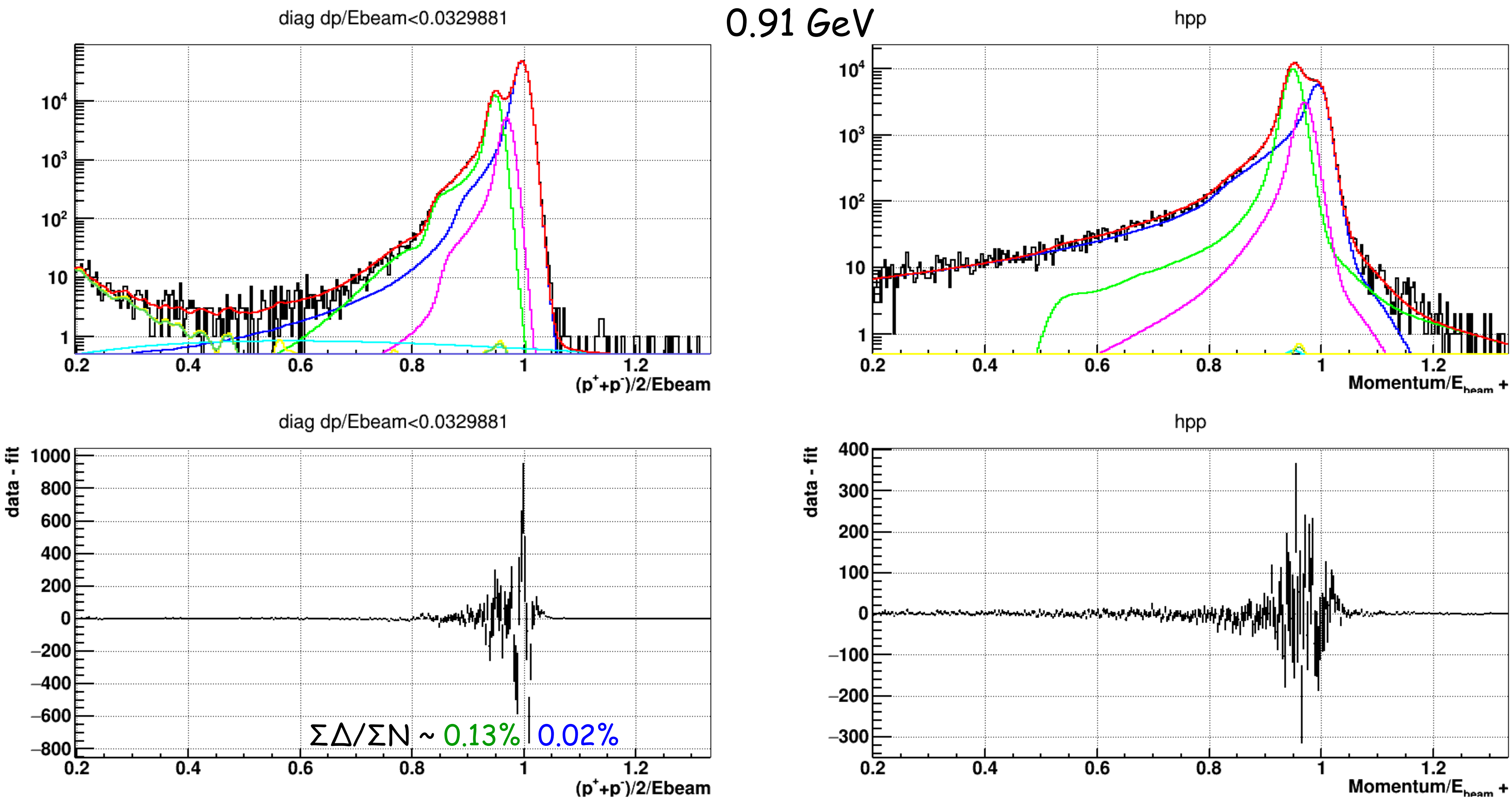


4. Can we see similar plots for CM energies 0.5 and 0.9 GeV?



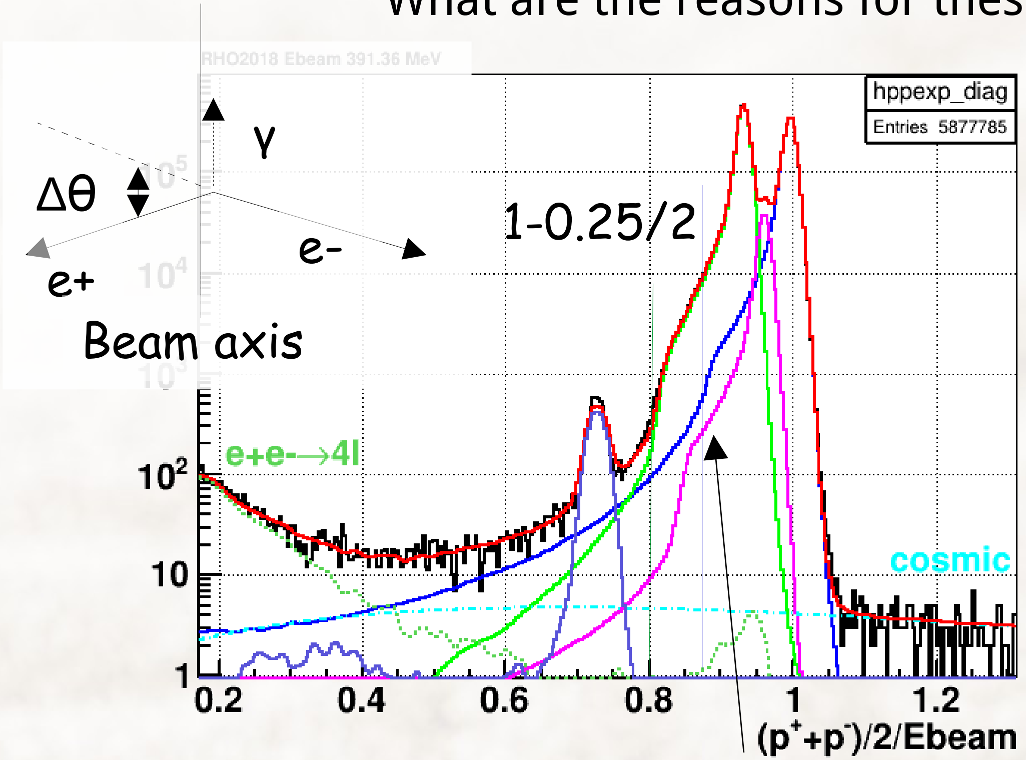
4. Can we see similar plots for CM energies 0.5 and 0.9 GeV?

0.91 GeV



Question 5

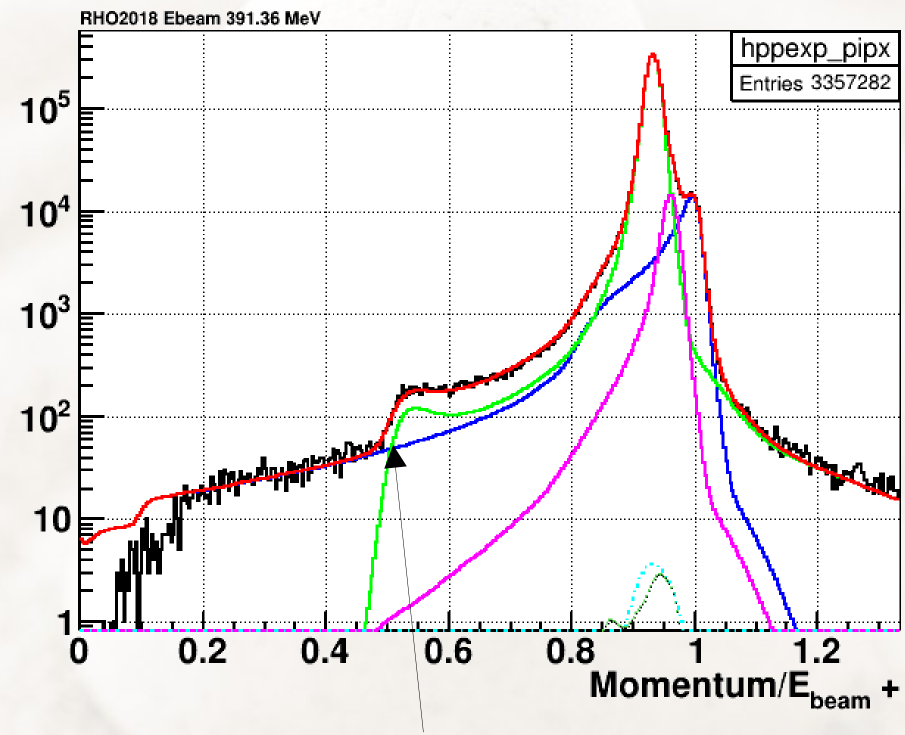
5. Still in Fig. 6-7: the ππ and ee distributions show shoulders in the tails. What are the reasons for these structures?



Related to kinematic region of one photon emission after applied cuts on collinearity: one photon with ω energy over beam axis

→ Δθ (on 2 charged tracks) ~ ω/E^0

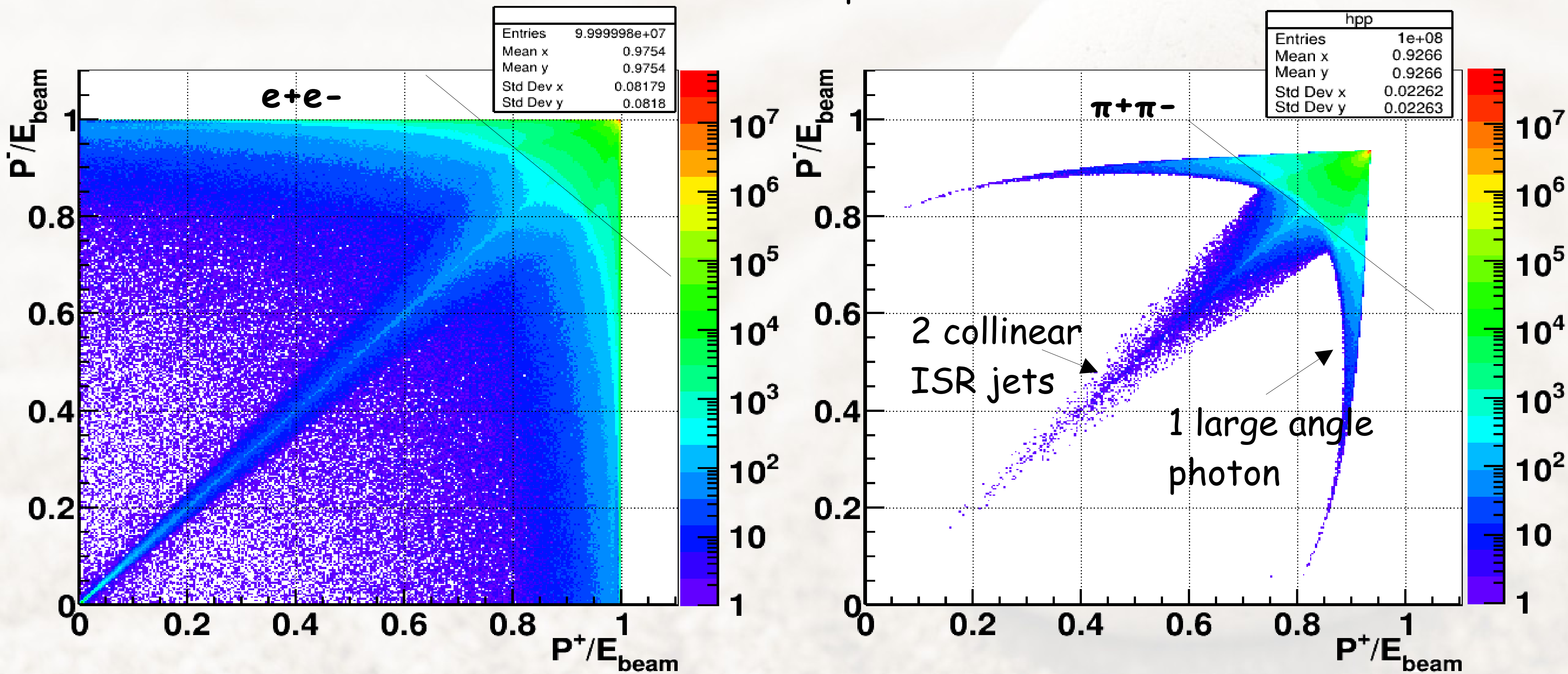
Cut $|\Delta\theta| < 0.25$ rad → $1 - (p^+ + p^-)/2/E^0 < 0.25/2$



End of muon momentum spectrum after pion decay

2D momentum distribution from MC

Momentum PDF's ingredients from MC generators
for the momentum-based separation



Question 6 (from short list)

Question 6
The 2D reference distributions contain 36 and 57 parameters treated as nuisance parameters in the likelihood fit. Provide more information on the nature of these parameters, their time dependence, the checks with data and how they impact the systematic uncertainty on the cross section. Is it possible to show a data-MC comparison for individual PDFs, e.g. by applying strong cuts for one of the tracks?

Separation of $\pi^+\pi^-$, $\mu^+\mu^-$, e^+e^-, …. final states is based on likelihood minimization:

$$-\ln L = -\sum_{events} \ln\left[\sum_i N_i f_i(X^+, X^-)\right] + \sum_i N_i$$

Momentum-based separation:

MC generator spectra are convolved with detector response function (resolution, brems., pion decays)

36 free parameters in fit per each point

PDF(e+e-) detector response addition: brems. + 3 Gauss per axis + sigma (x-y correlation):

$b^0(1-p/p_0)^{-1-b1-f(b0)}$ X (ΣGauss(1/p'))

2 + 8*2 + 1 = 19 parameters

PDF(μ+μ-): 3 Gauss from e+e- + 1 Gauss(p) per axis + sigma (x-y correlation):

2*2 + 1 = 5 parameters

PDF(π+π-): 3 Gauss from e+e- + 1 Gauss(p) per axis + sigma (x-y correlation) + fixed from MC form of pion decays tails (ratio in tail free):

2x2 + 1 + 2 = 7 parameters

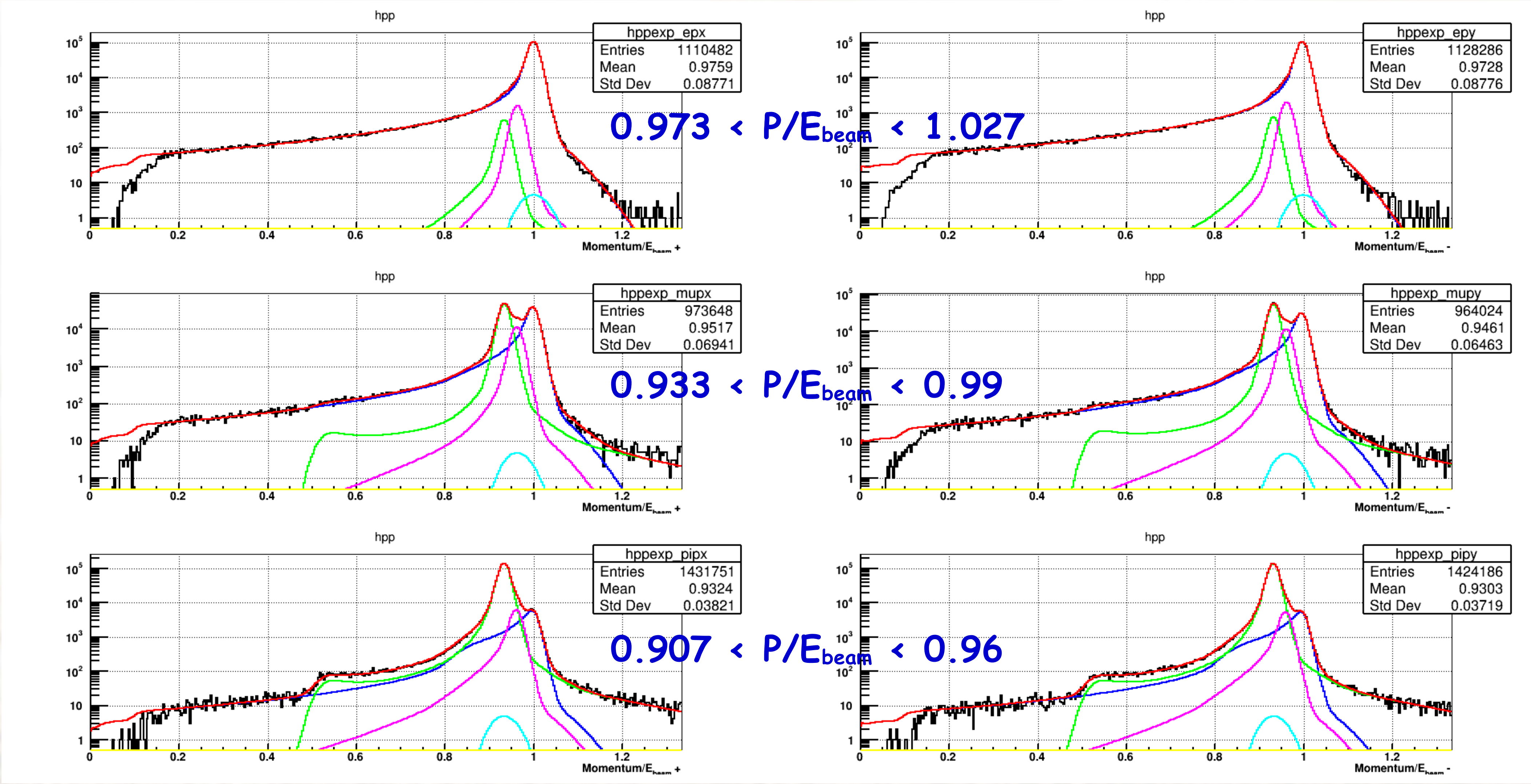
PDF(cosmic): form fixed from clean cosmic sample selected by time of event

PDF(3π, 4l): form fixed from from full MC

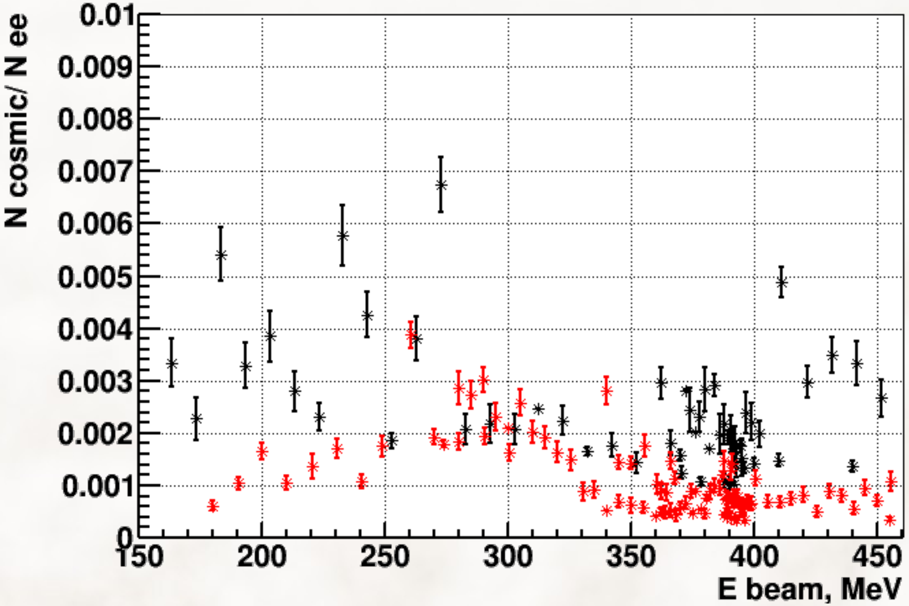
N_{ee} , $N_{\pi\pi}/N_{ee}$, $N_{\mu\mu}/N_{ee}$, $N_{3\pi}/N_{ee}$, N_{cosmic}/N_{ee} - 5 parameters

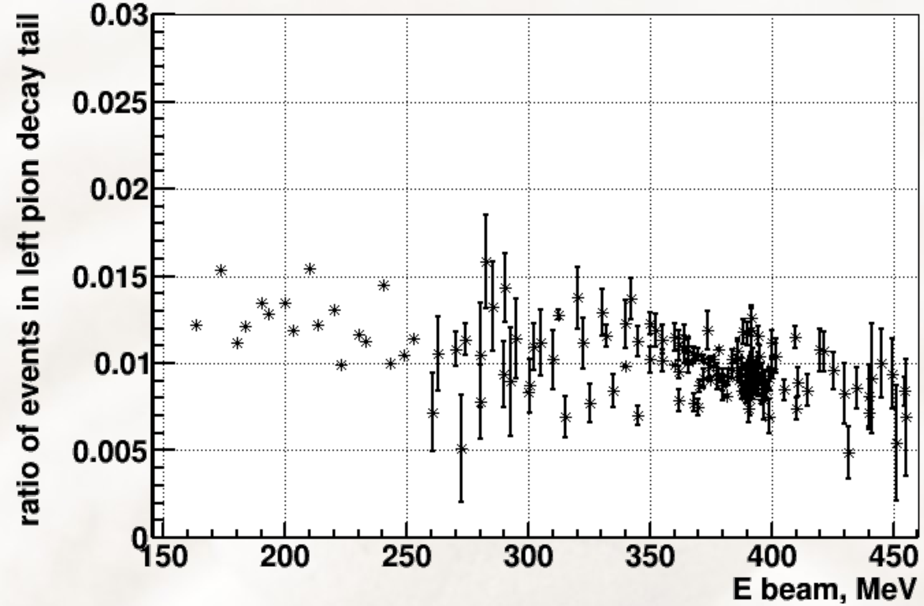
Fit result

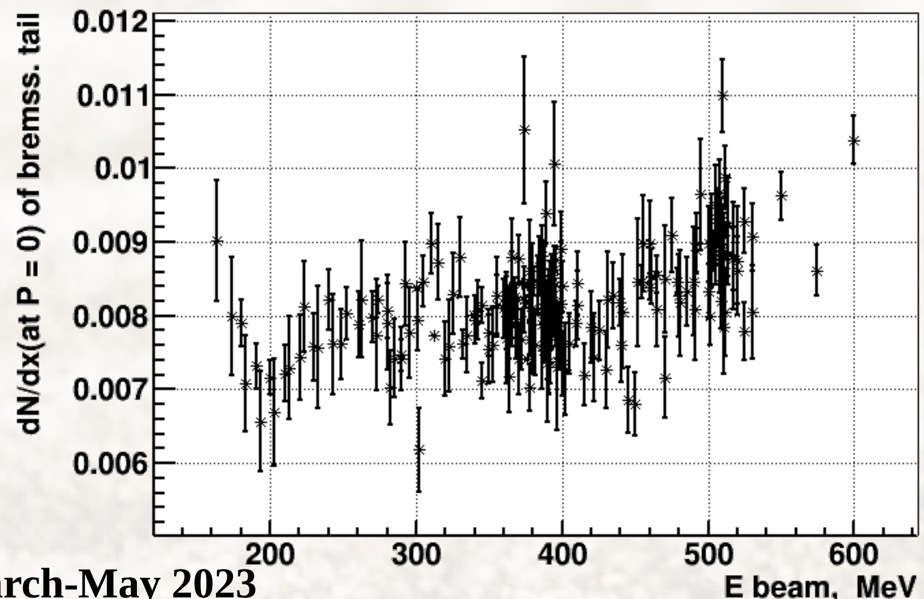
Ebeam 391.48 MeV

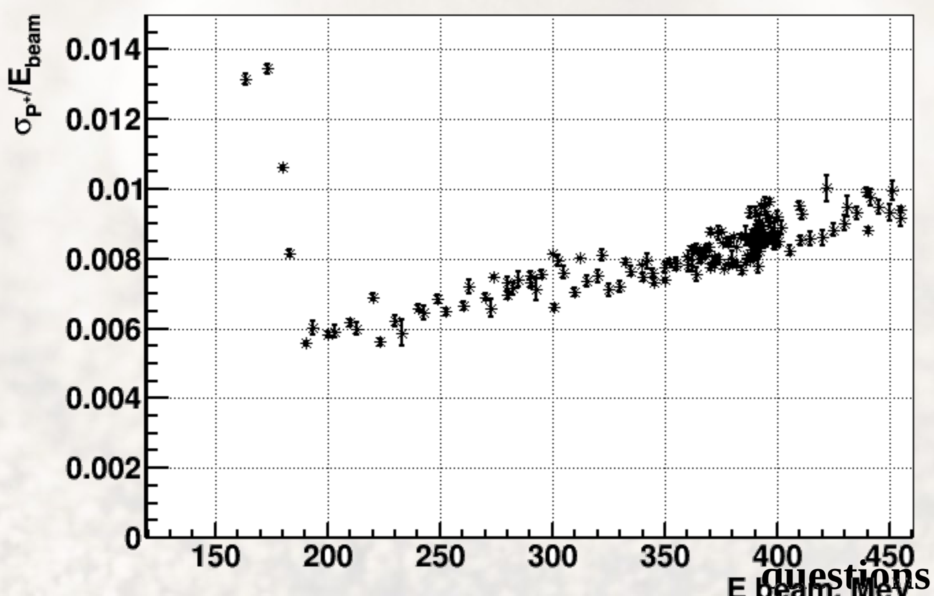


Some parameters dependences









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<u>Energy deposition-based separation:</u>

PDFs is described by a generic functional form (log-gaus, etc),
trained on the data: by tagged electron, cosmic muons

56 free parameters in fit

<u>PDF(e+e-):</u> (2 Logarithmic Gaus + 1 Gaus) + 0-Energy probability – all per axis + fixed from MC X-Y correlation $\Sigma a_i f(k_i X+, k_i X-)$

$10*2 + 1*2 = 22$ parameters

<u>PDF(μ+μ-):</u> form fixed from clean cosmic sample selected by time of event, and momentum, $N_{\mu\mu}/N_{ee}$ fixed from QED

<u>PDF(π+π-):</u> MIP as "2 Logarithmic Gaus + 1 Gaus, 1 shift fixed" + MIP probability + 0-Energy probability + Hadronic tail by sum of decreasing gausses as $\Sigma a_i \, Gauss(X-(E^{max}-E^{mip})*i/n+E^{mip}, \sigma_0)$ – all per axis

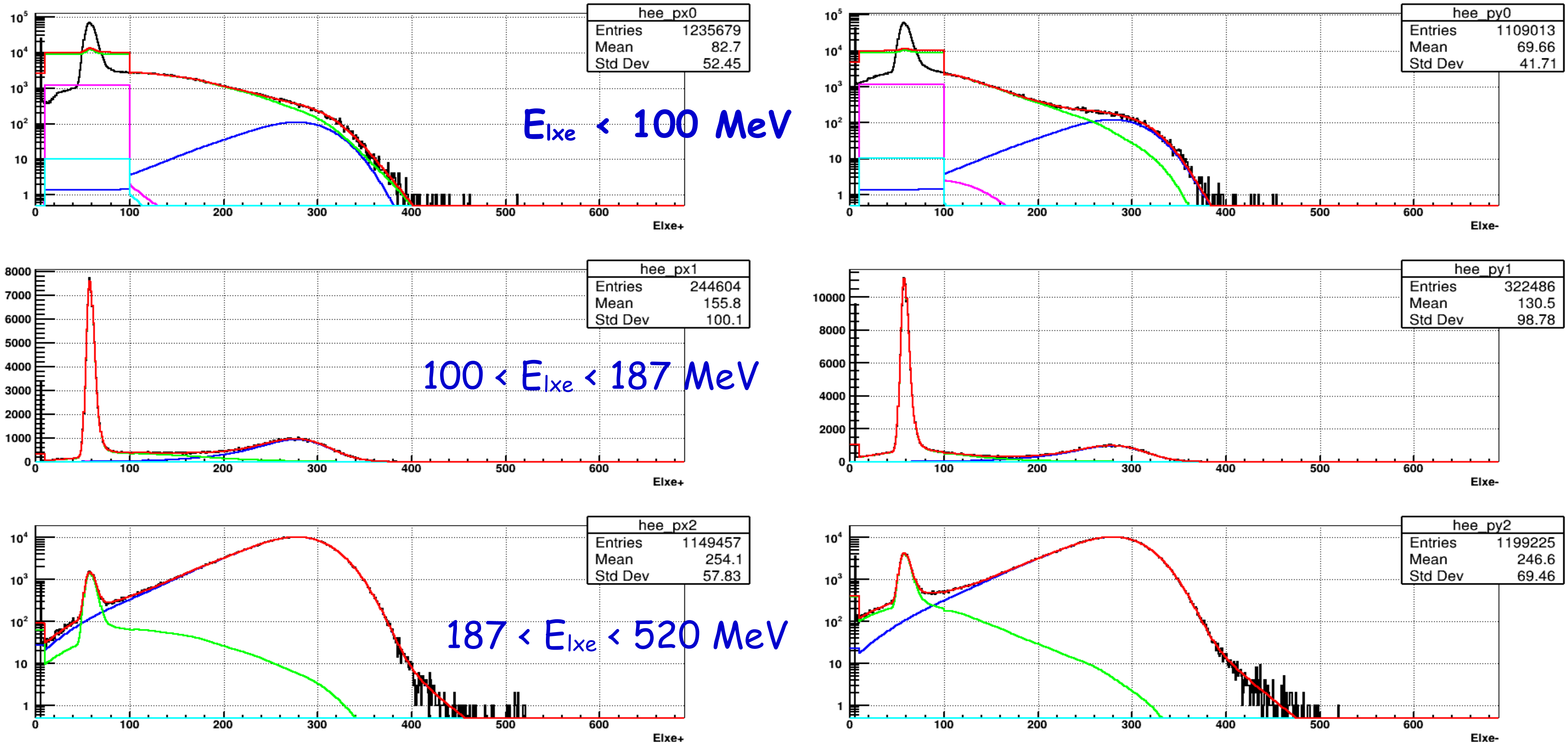
$9*2 + 1*2 + 1*2 + 5*2 = 32$ parameters

<u>PDF(cosmic):</u> form fixed from clean cosmic sample selected by time of event, N fixed from time distribution

N_{ee} , $N_{\pi\pi}/N_{ee}$ - 2 parameters

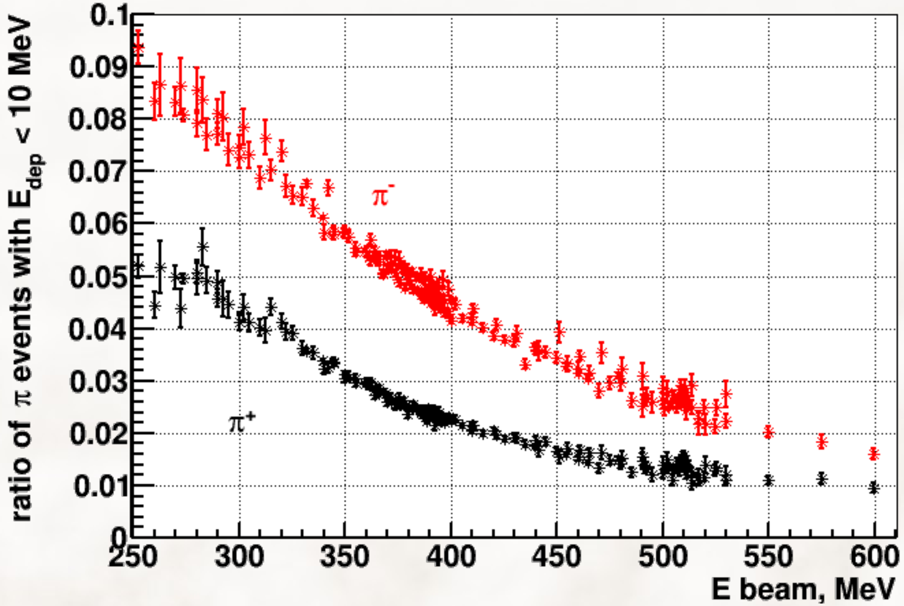
Fit result

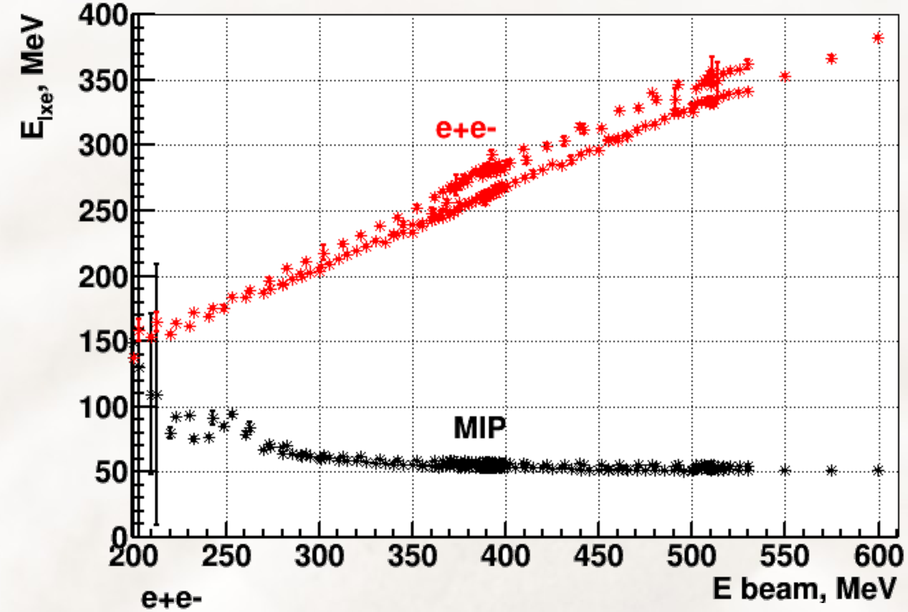
Ebeam 391.48 MeV

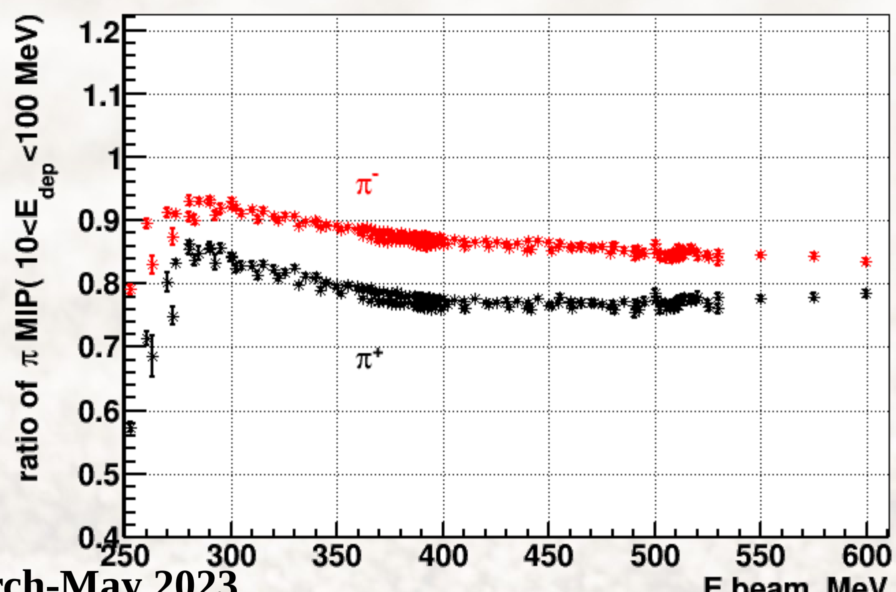


Muon & Pion MIP PDFs are strongly overlap,
In 10 < E_{lxe}^{+-} < 100 MeV constant PDF was used to suppress likelihood systematic biases from not exact PDF descriptions

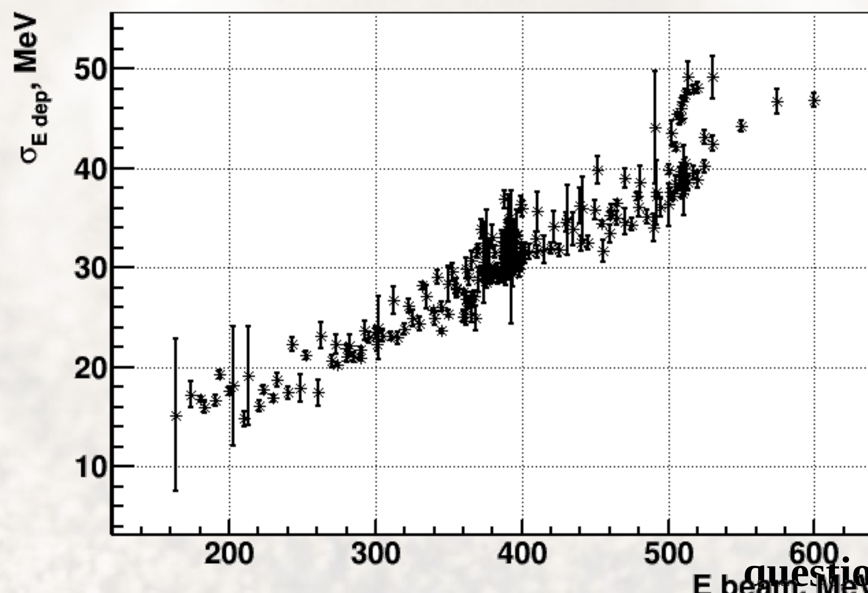
Some parameters dependences







e+e-



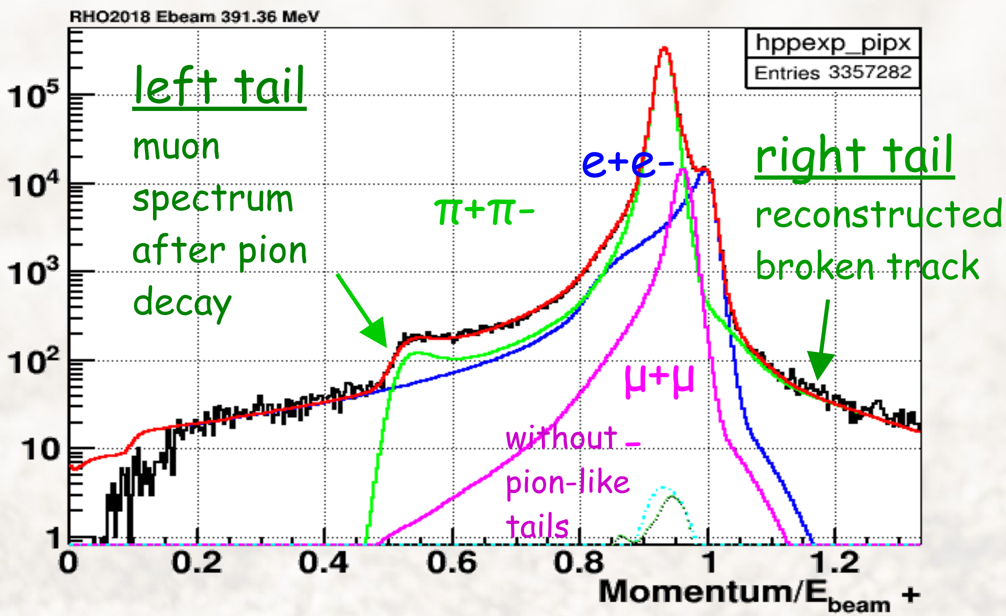
Question 6 & 19 (from short list)

Question 6: The 2D reference distributions contain 36 and 57 parameters treated as nuisance parameters in the likelihood fit. Provide more information on the nature of these parameters, their time dependence, the checks with data and how they impact the systematic uncertainty on the cross section. Is it possible to show a data-MC comparison for individual PDFs, e.g. by applying strong cuts for one of the tracks?

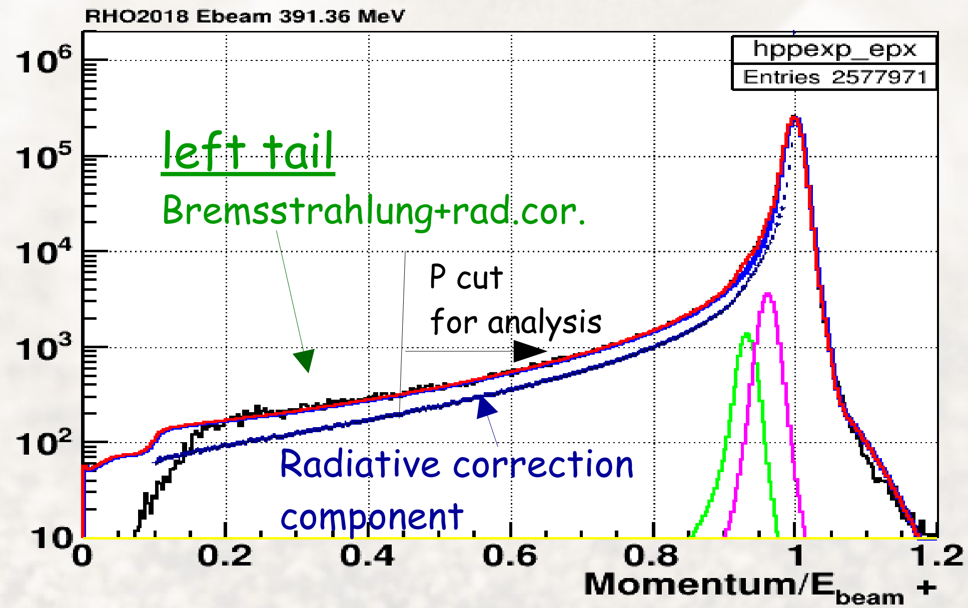
Question 19: Tracking plots (efficiency plot?) are given for MC simulation only. Need to see data/MC tests.

The PDFs are obtained from data itself, they are not necessary to be same as in simulation. Some features of PDF give possibility to control particle specific losses (pion decay, bremsstrahlung loss) – given in slides 27,28.

Experimental P+ spectrum with $|P^{-} - P\pi| < 10$ MeV



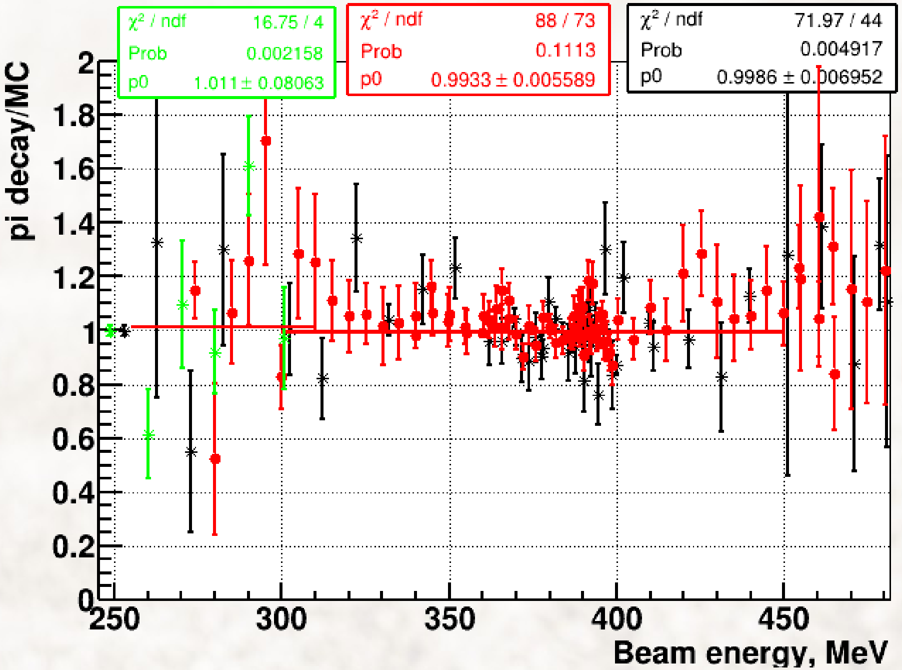
Experimental P+ spectrum with $|P^{-} - Pe| < 10$ MeV



Data/MC checks for particle specific losses

Some features of PDF give possibility to control particle specific losses (pion decay, bremsstrahlung loss) – slides 27,28.

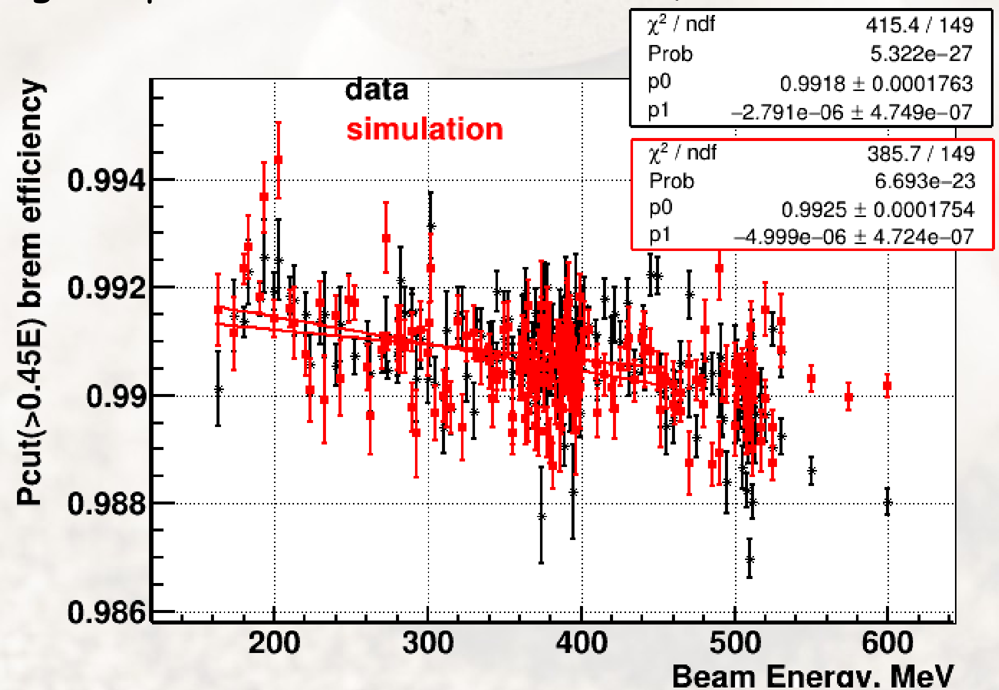
N events in Left+Right pion decay tails in PDF
The monitoring tool to control the reconstruction efficiency of decayed tracks in Data vs MC



Relative consistency ~ 2-3%

N events in Left + Right tails ΔData/MC:
RHO2013: 0.0014+-0.007, RHO2018: 0.007+-0.006

Left tail in electron momentum spectra describe radiative + bremsstrahlung loss
N of events of brems. part at cut P/E_{beam} < 0.45 gives part of brems. correction (0.9% of total 1.2%)



Relative consistency in inefficiency ~ 2%

Question 7

7. How are you sure that there is no double counting, i.e. the corrections applied on the PDF don't include already some of the corrections mentioned after (like for example the ones in Section 4.2)?
Section 4.2 – efficiency from particle specific losses

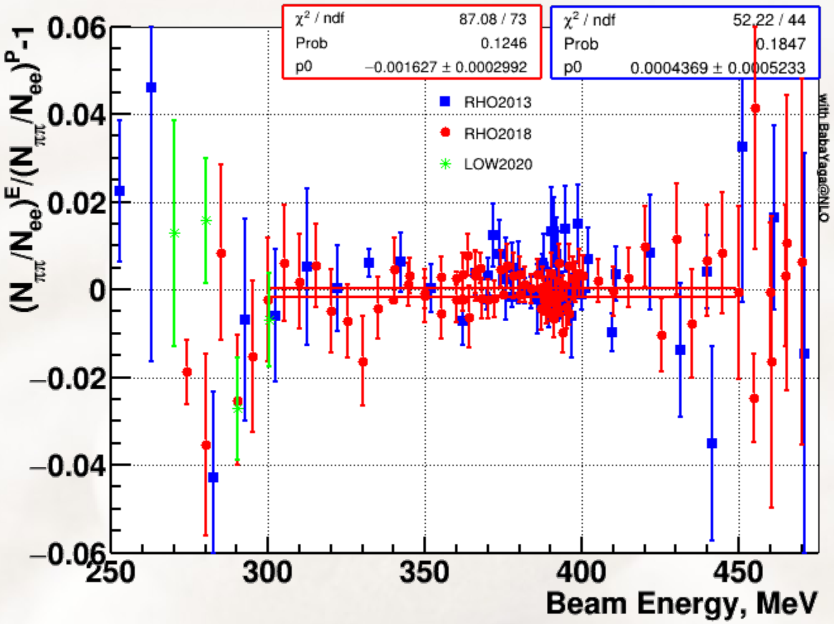
I thinks the PDF and efficiency are two different multiplicative things:
PDFs and minimization gives how much of events of particular type in the selected sample
efficiency correction how much of events from generator pass to this datasample

- The PDF is normalized to "1" in the used momentum range,
- the dependence of efficiencies with momentum gives some modification of PDF form, but still tails (bremsh., pion decay, from resolution functions) are free in the likelihood fit → effectively they should accounts for efficiencies changes in tails.
- So results of the likelihood fit will be still the number of corresponding particles in the selected collinear sample
- Test of the separation on the MC samples gives how well the number of particular types in the selected sample reproduced after minimization

Probably it is different from the ISR approach, where the momentum distribution represent different $M_{\pi\pi}$ points, while in our case most of events present in peaks

Question 8 (from short list)

Fig.8: the double ratio $N_{\pi\pi}/N_{ee}$ for the 2 methods is fitted between 0.6 and 0.9 GeV and found to be consistent with 1 within 0.2%. The fit is dominated by the large statistics at the ρ peak while uncertainties are much larger in the tails. Is it reasonable to quote a constant systematic uncertainty on this ratio of 0.2% throughout the range 0.381-1 GeV?



The Logic is different:

Possible biases are checked on full MC → systematics are estimated independently per each separation method.
Comparison of different methods gives the additional cross-check and ensure us, at least at central region, that 0.2% systematic uncertainty estimation is safe.

Question 8

The separation biases of likelihood minimization was checked on mixed samples of full MC

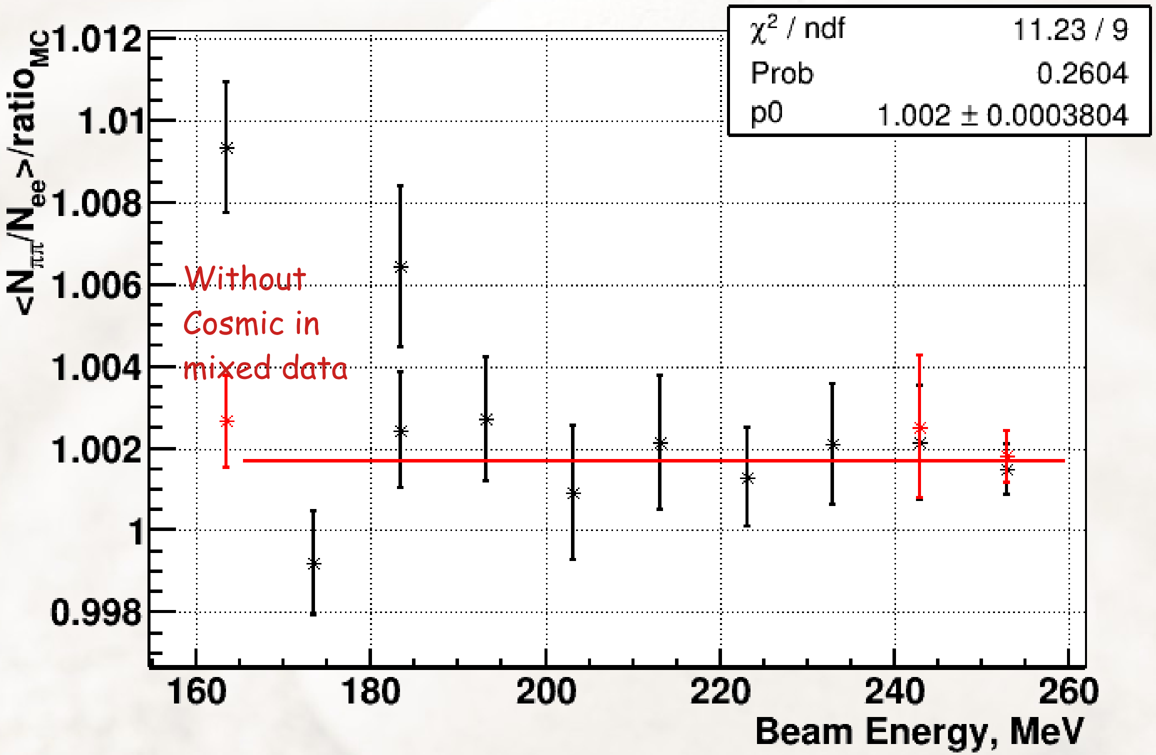
At lowest points statistical precision per point is low ~2-7%

100 independent mixed data samples were produced:

<Nππ/Nee> ~ +0.2%

<Nμμ/Nee> ~ +0.2%

Momentum-based separation on full MC

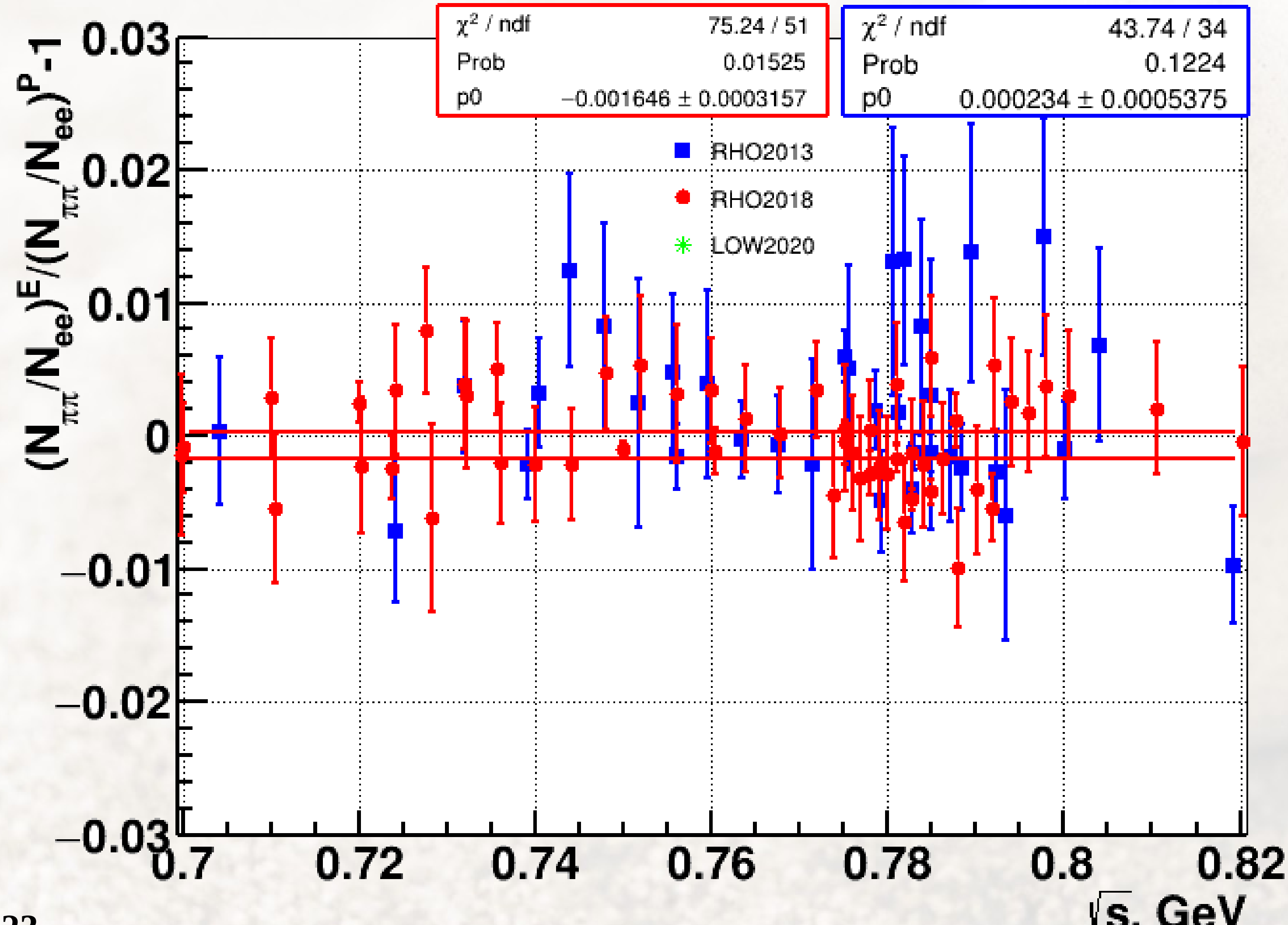


At $\sqrt{s}$ < 0.381 GeV, the detector was operated with reduced magnetic field B=0.65T (1T) instead of 1.3 T → there is not enough data for cosmic PDF determination → systematics 0.5%

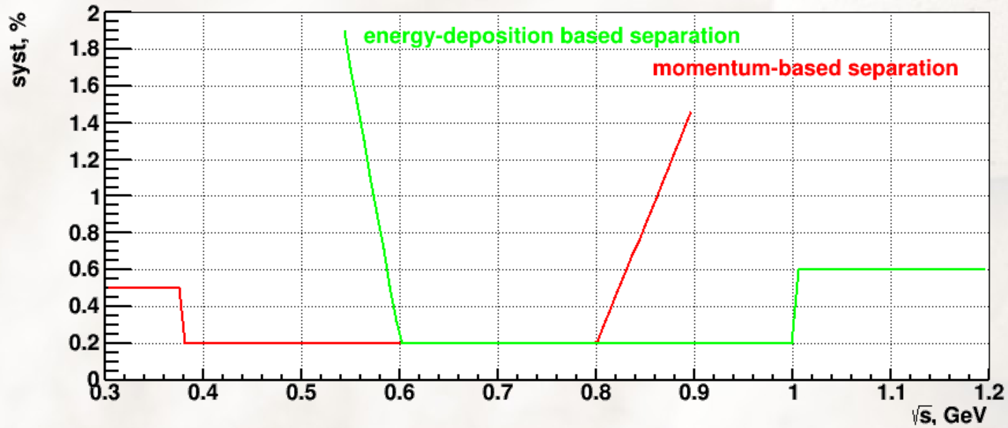
At lowest points stronger cut $|t^{event}-t^{beam}|$ < 10 nsec to suppress cosmic events was applied

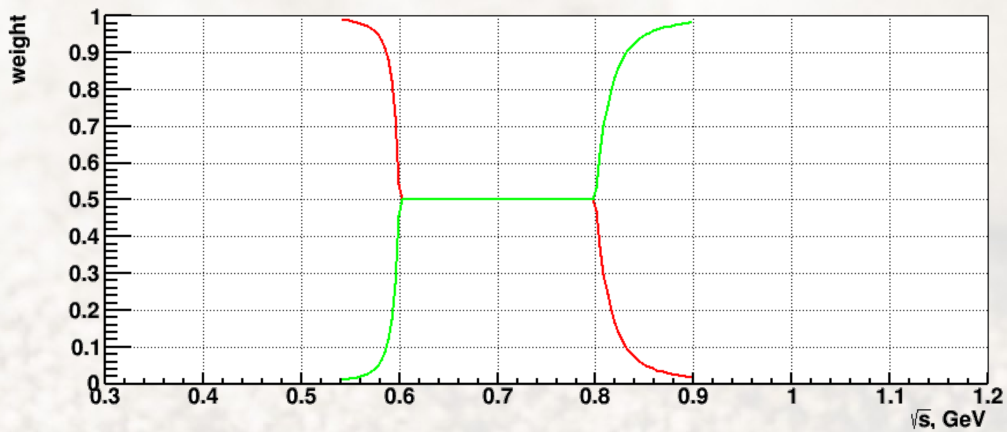
Question 9

9. Show a blow-up of Fig. 8 in the range 0.7 – 0.82 GeV



10. Please show systematic uncertainty for the two separation methods (track, cluster) as a function of CM energy
11. The results of the 2 methods are averaged with weights given by their systematic uncertainties inverse squared. Show the plot of the weights as function of CM energy..





Efficiencies

9. e+e- → γγ (with one photon converted to e+e-, where one of the e+- takes most of the energy) can give a tiny contribution to the test sample. I guess it's pretty easy for CMD-3 to suppress such events, but this contribution cannot be exactly 0.

At Ebeam 391.48 MeV

γγ / e+e- cross sections (θ>1rad) 141./761.5 ~ 0.19 ⇨ <0.3% effect

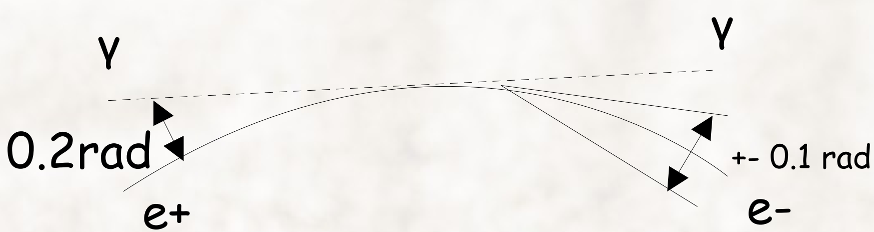
Vacuum tube X/X0 = 0.0072

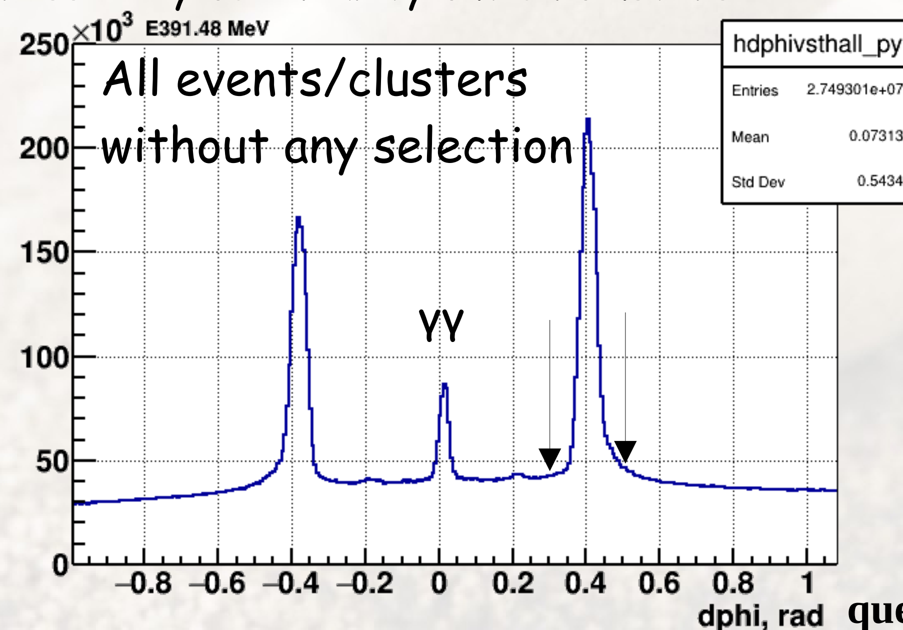
$\Delta\varphi_B^{ee}(\theta=\pi/2) = 0.395$ rad – deflection of e+e- tracks in magnetic field

Test sample selection: $|||\varphi^{cl}_1 - \varphi^{cl}_2| - \pi| - \Delta\varphi_B \; | < 0.1$ rad, ($\min(0.05, \Delta\varphi_B/4)$)

Only two back-to-back clusters, momentum selection, ….

γγ (or with one γ-conversion) additionally suppressed by collinearity cut on clusters





From e+e- → γγ (at 391.48 MeV) simulation

Effect on 1-track test sample ~< 10^{-6}

on double correlated ineff ~ 10^{-5}

Question 13

13. Fig. 11-12: what is the CM energy?

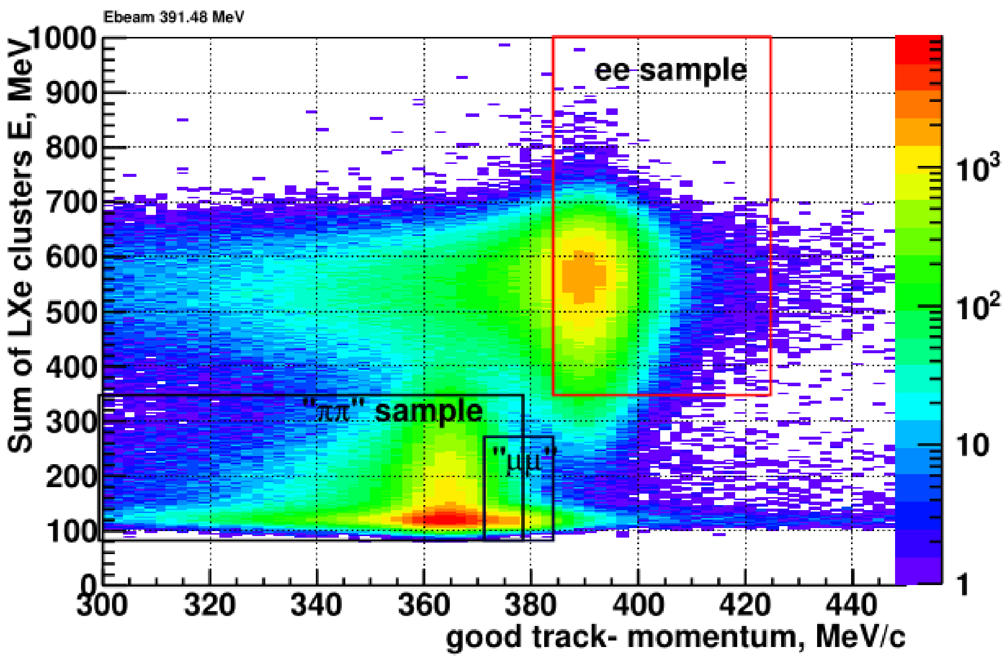


Figure 11: Test event types selection for efficiency determination.

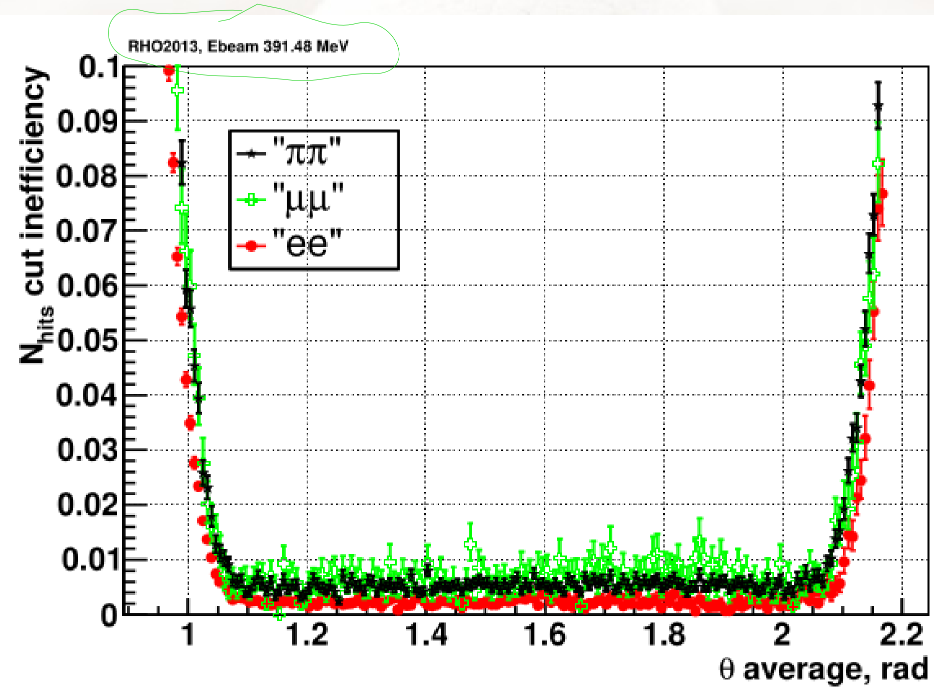


Figure 12: Event inefficiency due to N_{nhits} selection with polar angle of event.

E_{beam} = 391.48 MeV, RHO2013

when 4 layers in the middle were off, N_{hit} inefficiency for RHO2018 3-4 times less (on edge and in good region)

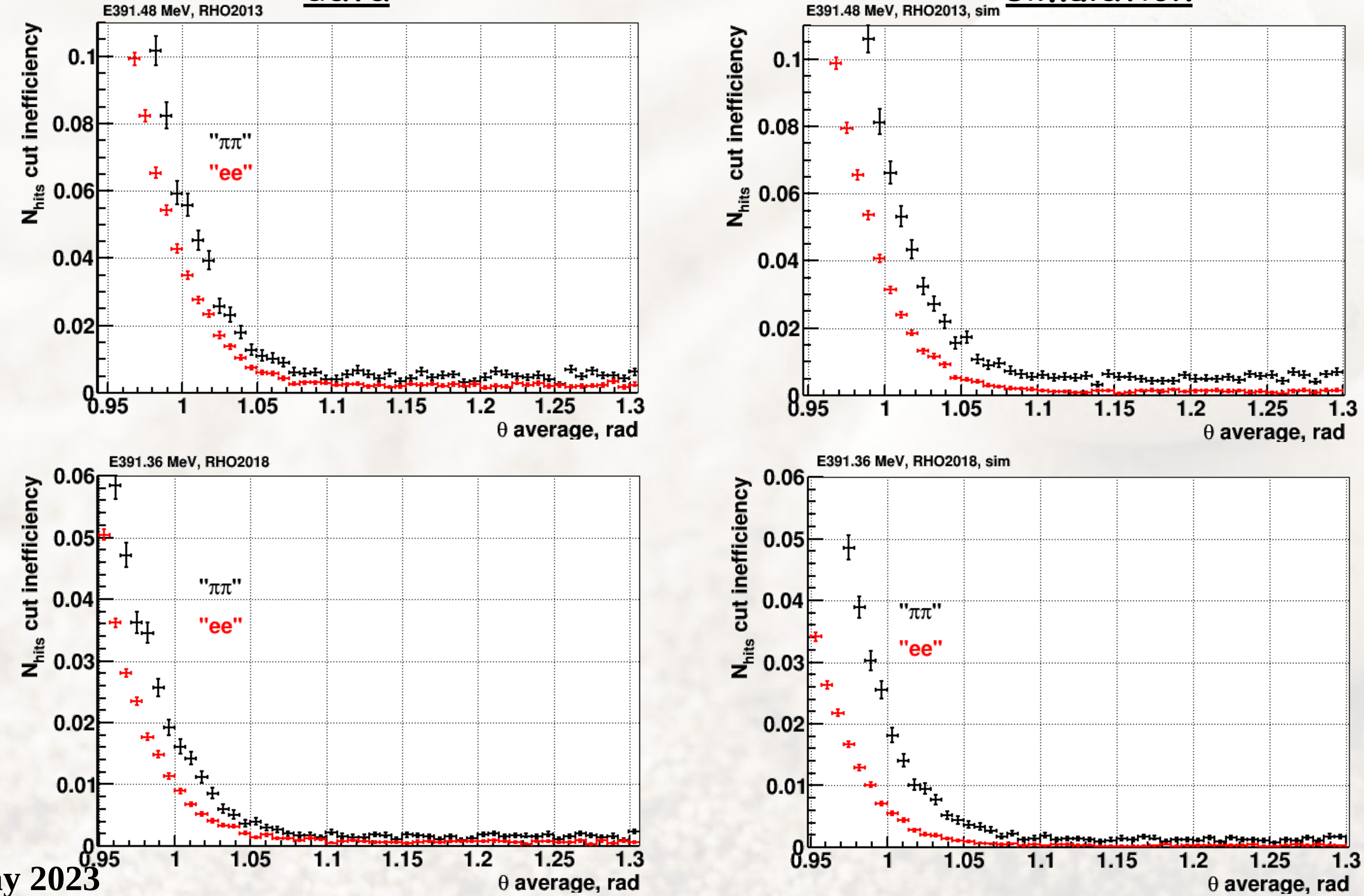
14. Fig.12: Are the differences between ππ and ee at the edges reproduced by Monte Carlo?

Yes, it is.

data | **simulation**

RHO2013

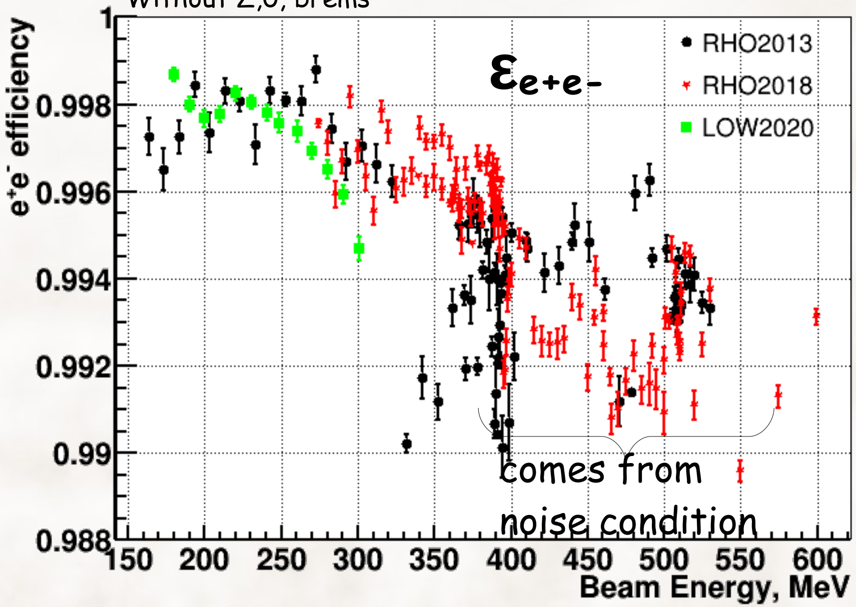
RHO2018



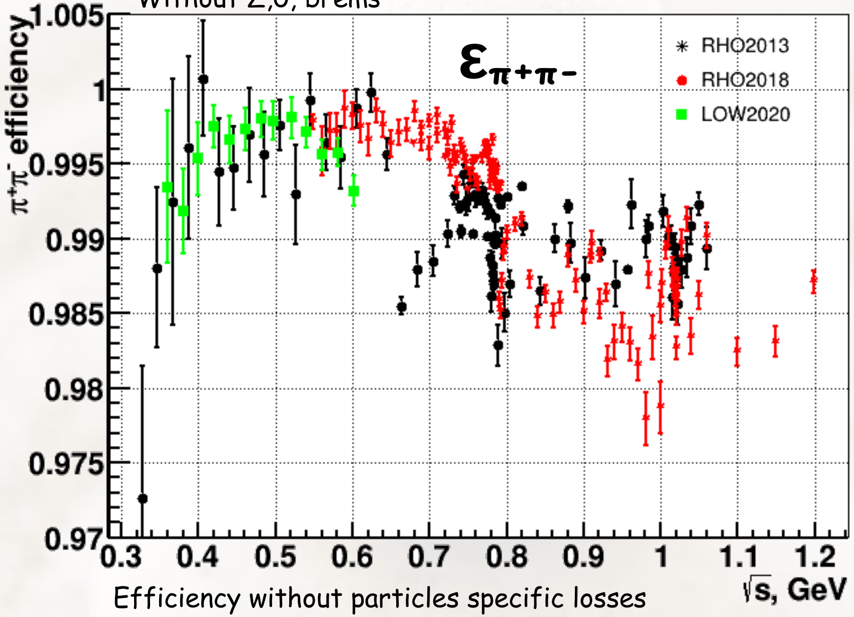
Question 15

15. Fig.15: Is it possible to see the corresponding plot for the ππ efficiency? Why is there a large scattering of efficiencies on a scale beyond the error bars?

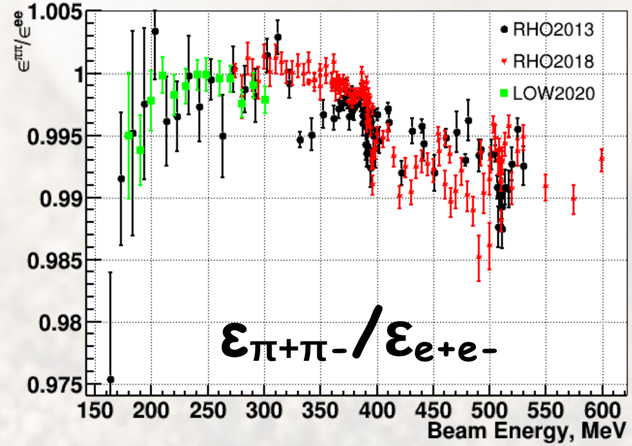
Without Z,θ, brems



Without Z,θ, brems



Efficiency without particles specific losses



Changes of efficiency is because of the detector conditions:

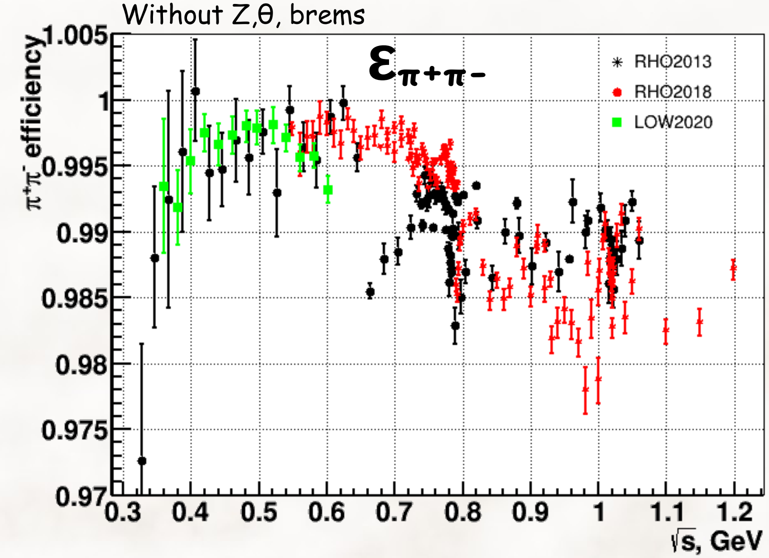
Noise, Amplitudes on wires (depend on Pressure, HV tuning, gas), thresholds to deal with noise levels, ….

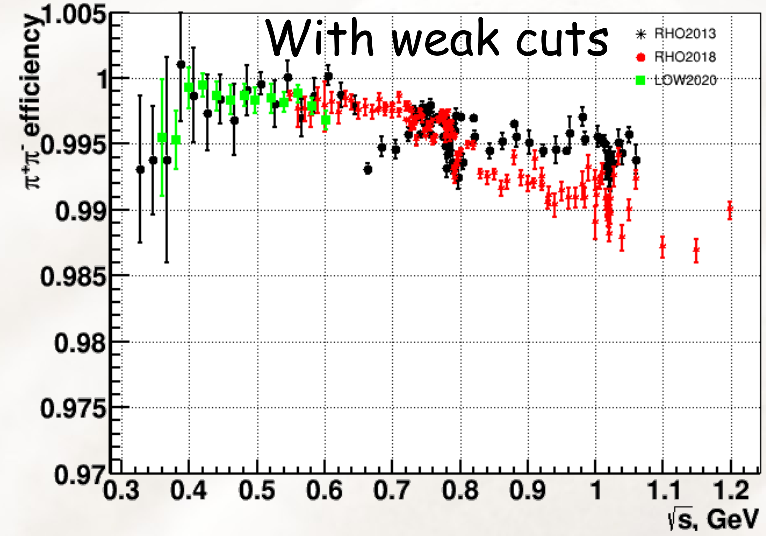
Everything can depend on per day bases, person shifts….

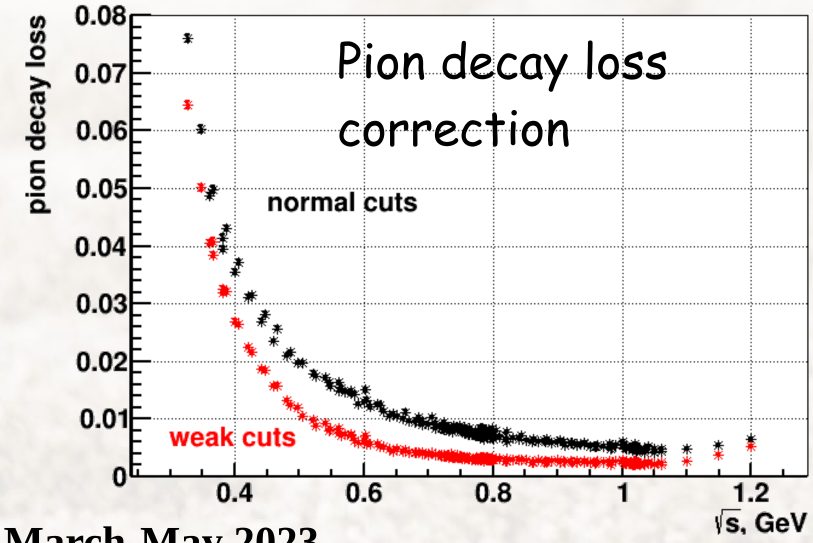
RHO2018: above ω was high level correlated noises

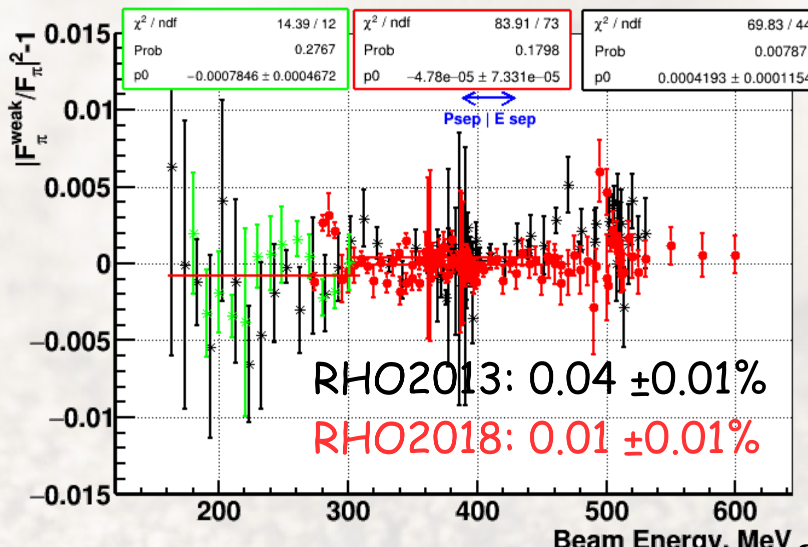
Weak cuts

Nhits >= 10 → 8, χ2 < 10 → 20, |Δρ| < 0.3 → 0.6 cm





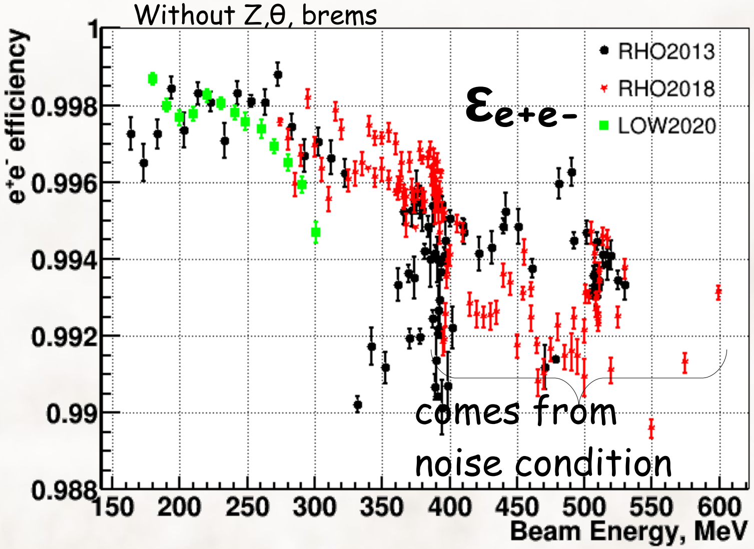




Form factor consistency in new cuts

Question 16

16. In Figs. 15 - 18 there seem to be very significant structures in the efficiency corrections (i.e. very significant differences between neighbouring points). What are these caused by? Maybe some fast time dependence?



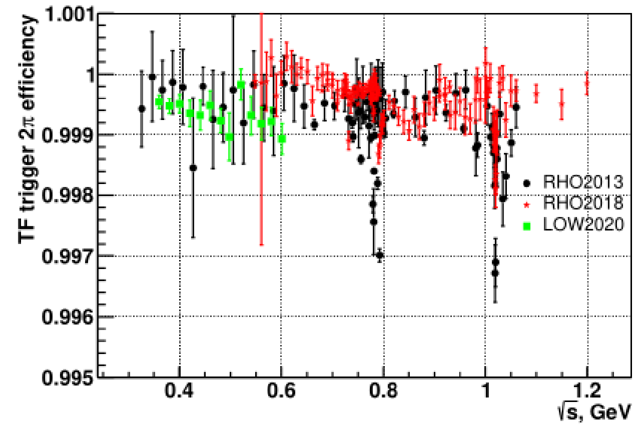


Figure 17: The "charged" trigger (TF) efficiency for the $\pi^+\pi^-$ events.

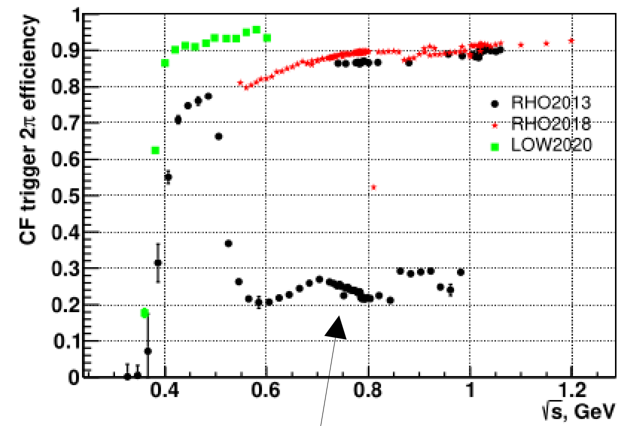


Figure 18: The "neutral" trigger (CF) efficiency for the $\pi^+\pi^-$ events.

Changes of efficiency is because of the detector conditions:
Noise, Amplitudes on wires (depend on Pressure, HV tuning, gas), thresholds to deal with noise levels, ….
RHO2018: above ω was with high level correlated noises

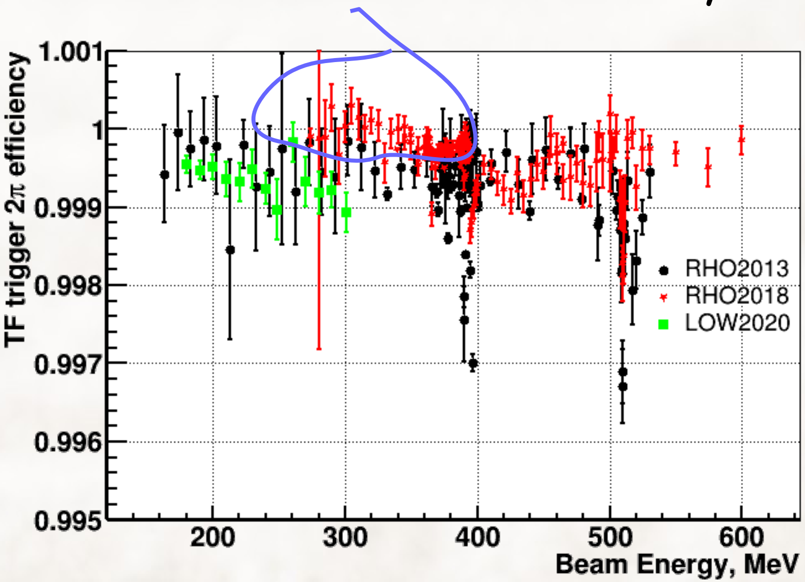
Part of the 2013 data were collected with a higher energy deposition threshold in the Cluster Finder Trigger

Everything can depend on per day bases, person shifts….

Question 17

17.In Fig. 17 a few points seem to have efficiencies larger than 1. What is causing this?

TrackFinder 2π efficiency



Having two "independent" triggers allows to study an efficiency of certain one by requiring that other presents in an event:

$$\epsilon_{TF}^{trig} = \left(N_{TF\,\&\,CF} / N_{CF} \right) / \left(\epsilon_{TF\,\&\,CF}^{rec} / \epsilon_{CF}^{rec} \right)$$

<1

Efficiency correction accounts for correlation via time response

Can statistically variate in the efficiency determination

For example for 305 MeV point:

> 1 value in the ratio $\varepsilon_{CF}/\varepsilon_{TFCF}$ comes from correlated inefficiency component.

This inefficiency is obtained without good track requirement,

This test sample has high cosmic background, and the number of beam related signal events calculated from $Z_{clusters}$ distribution fit → converged slightly differently in the different samples

Question 18 (from short list)

18. Tracking: clarify the separation made between 'base efficiency' (track selection cuts) and inefficiency from sources specific to particle type (decay, multiple scattering, bremsstrahlung, nuclear interactions).

The efficiency analysis is based as much as possible on data itself.
The test sample for efficiency study was selected by 2 collinear clusters in calorimeter.
Unfortunately it is doesn't cover the full data sample used in the particles separations.
Some events, when second cluster is not present, are not taken into account in test sample.
Test sample covers only ~30% of pion specific inefficiency (from ~2%-pion decay, nuclear interact)
~ 5% of electron specific (from ~1% - bremsshtrahlung)

Also some of inefficiencies like cuts on Nhits, Z_{vtx}, resolution in θ are studied separately

Particle specific losses were taken from full MC (and controlled by data).
This corrections are applied as for full $\pi^+\pi^-$, e^+e^-, … data samples used in analysis(added),
as also for each specific test samples used in efficiency study (subtracted to exclude double-counting).

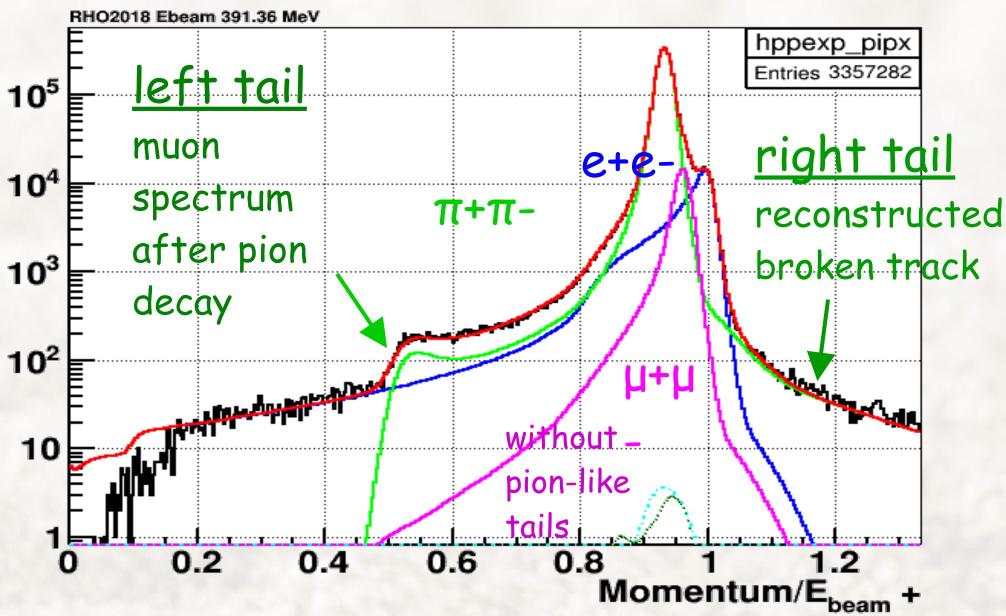
Question 6 & 19 (from short list)

Question 6: The 2D reference distributions contain 36 and 57 parameters treated as nuisance parameters in the likelihood fit. Provide more information on the nature of these parameters, their time dependence, the checks with data and how they impact the systematic uncertainty on the cross section. Is it possible to show a data-MC comparison for individual PDFs, e.g. by applying strong cuts for one of the tracks?

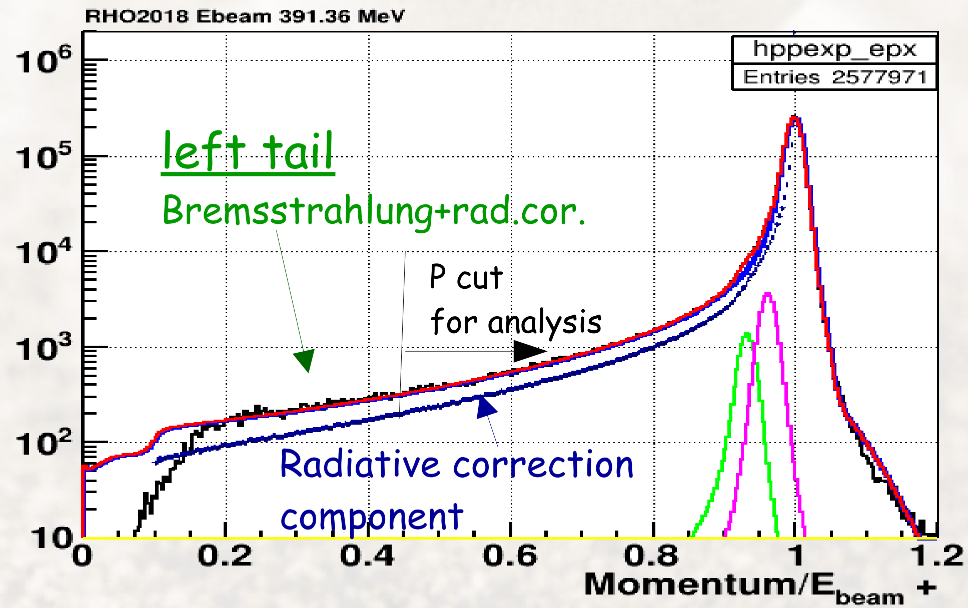
Question 19: Tracking plots (efficiency plot?) are given for MC simulation only. Need to see data/MC tests.

The PDFs are obtained from data itself, they are not necessary to be same as in simulation. Some features of PDF give possibility to control particle specific losses (pion decay, bremsstrahlung loss) – given in slides 28,29 of the TI talk.

Experimental P+ spectrum with $|P^- - P\pi| < 10$ MeV



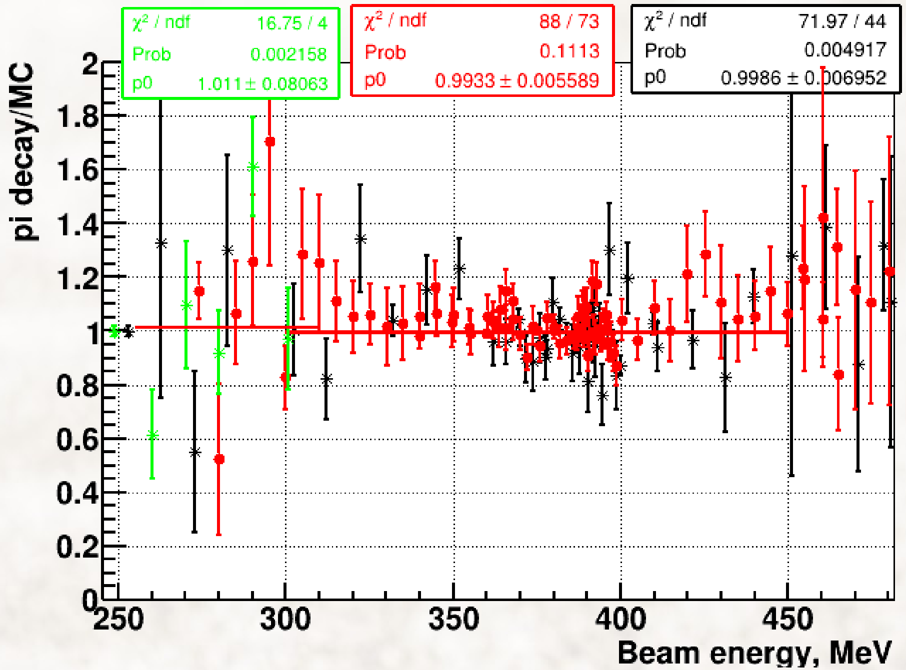
Experimental P+ spectrum with $|P^- - Pe| < 10$ MeV



Data/MC checks for particle specific losses

Some features of PDF give possibility to control particle specific losses (pion decay, bremsstrahlung loss) – slides 27,28.

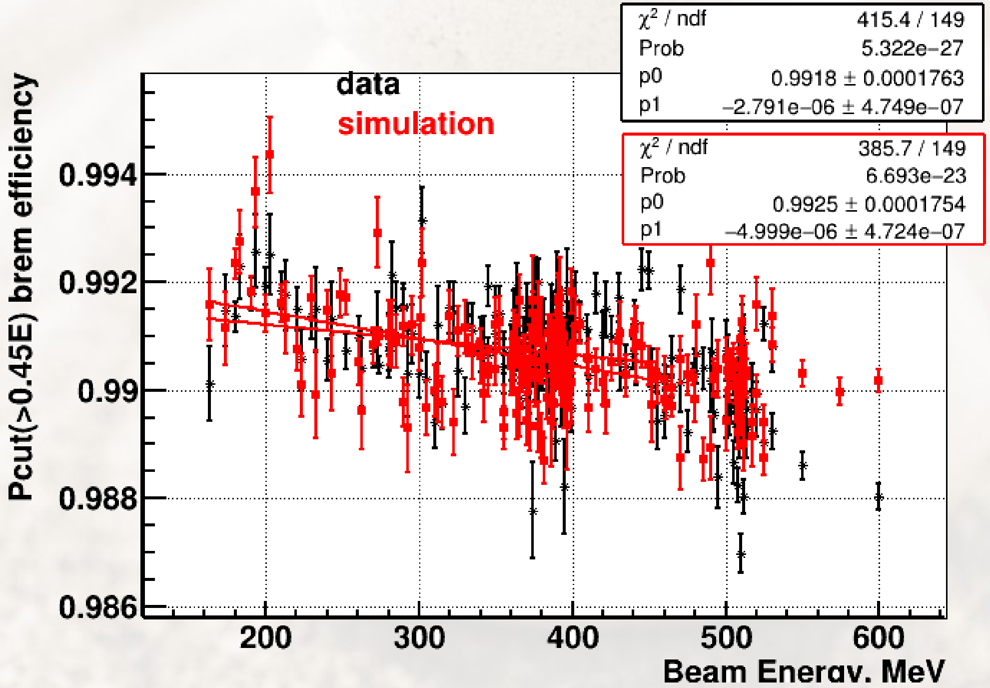
N events in Left+Right pion decay tails in PDF
The monitoring tool to control the reconstruction efficiency of decayed tracks in Data vs MC



Relative consistency ~ 2-3%

N events in Left + Right tails ΔData/MC:
RHO2013: 0.0014+-0.007, RHO2018: 0.007+-0.006

Left tail in electron momentum spectra describe radiative + bremsstrahlung loss
N of events of brems. part at cut $P/E_{beam} < 0.45$ gives part of brems. correction (0.9% of total 1.2%)



Relative consistency in inefficiency ~ 2%

Question 20

20. No systematic uncertainty quoted for tracking efficiency resulting from data/MC tests, however overall efficiency inside fiducial volume is 94% (2018), 87% (2013)

Indeed it is:

Bremsstrahlung loss	0.05%
Pion specific loss	0.2% - nuclear interaction
	0.2% (low) - 0.1% (ρ) - pion decay

“Base” efficiency is obtained from the data itself, it is not necessary to have data/MC tests for them. Consistency of procedure was tested on MC samples.
Particle specific inefficiency is calculated with help of MC, and data/MC tests were performed as it was discussed in Question 19 and Slides 28,29 on the TI talk (27.03.23)

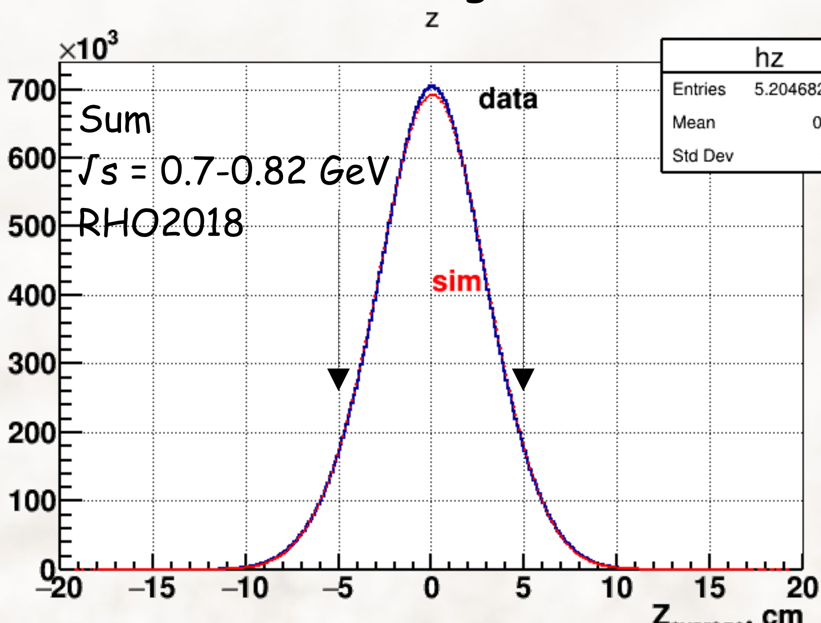
(https://indico.fnal.gov/event/59052/)

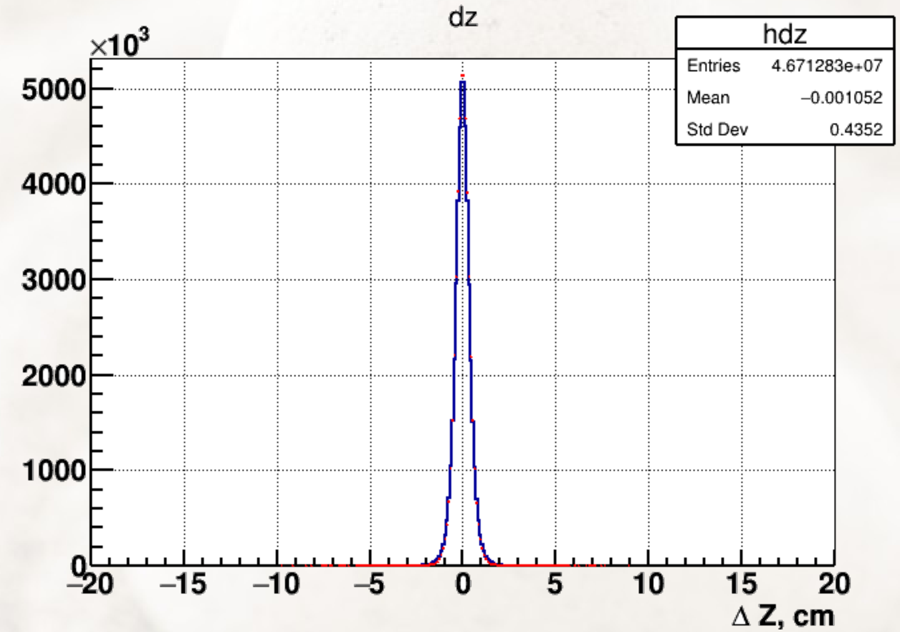
Z vertex cut inefficiency

ππ inefficiency 94% (2013), 87% (2018)

comes from Z vertex cut $|(Z^+ + Z^-)/2| < 5$ cm : 97.0%(RHO2013), 89.2% (RHO2018)

And it comes from length of beams, which are same for e+e-/π+π-





size of the beams interaction region σ_{Zbeam} = 3.04 cm

detector related Vertex resolution σ_{Zvtx} = 0.22 cm

→ detector related effect on efficiency ~ $(\sigma_{Zvtx}/\sigma_{Zbeam})^2$

$1 - \mathrm{Erf}(\sigma_{Zbeam}/5\mathrm{cm}/\sqrt{2})/\mathrm{Erf}(\sqrt{(\sigma^2_{Zbeam}+\sigma^2_{Zvtx})}/5\mathrm{cm}/\sqrt{2}) \sim 0.1\%$

Difference between e+e-/π+π- even smaller

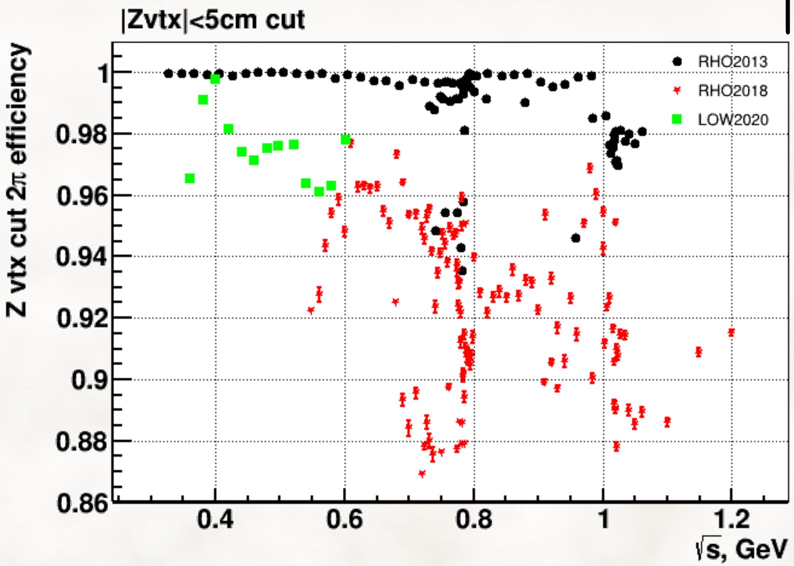
To be exact:

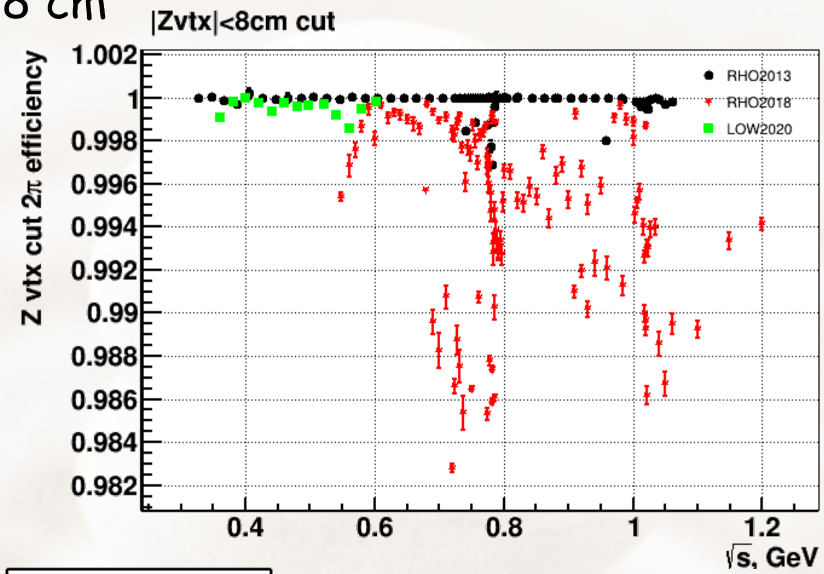
Detector related effect ≲ 0.5%

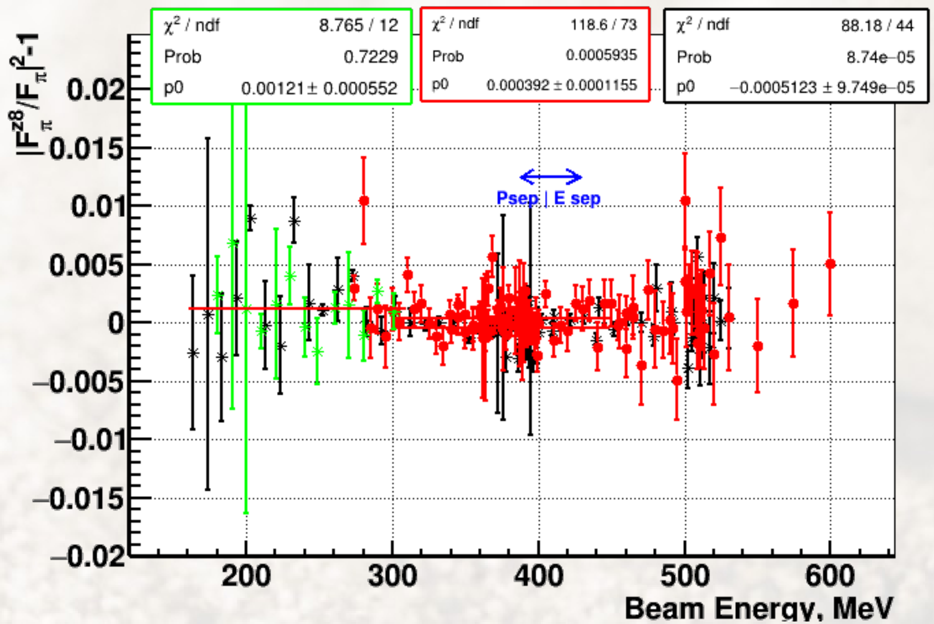
$\varepsilon_{ee}/\varepsilon_{\pi\pi} - 1 \lesssim (0.05 \div 0.10)\%$

Z vertex cut check

|Zvtx| < 5 cm → 8 cm







RHO2013: -0.05±0.01%

RHO2018: 0.04±0.01%

LOW2020: -0.12±0.05%

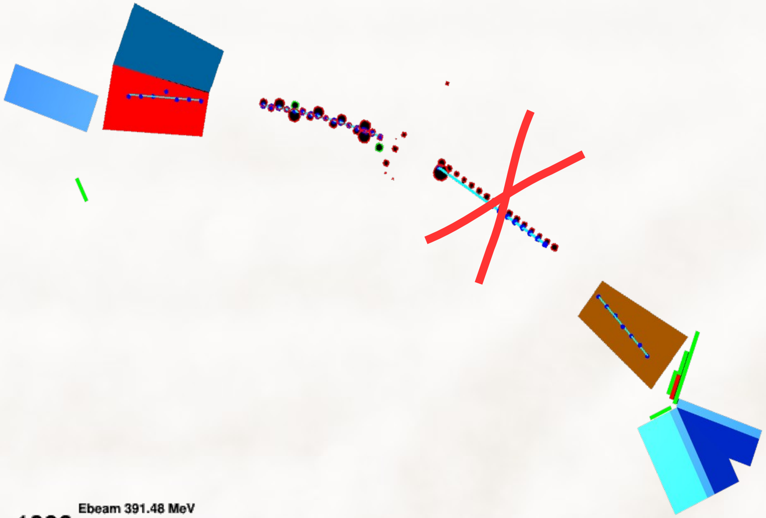
Question 21,22

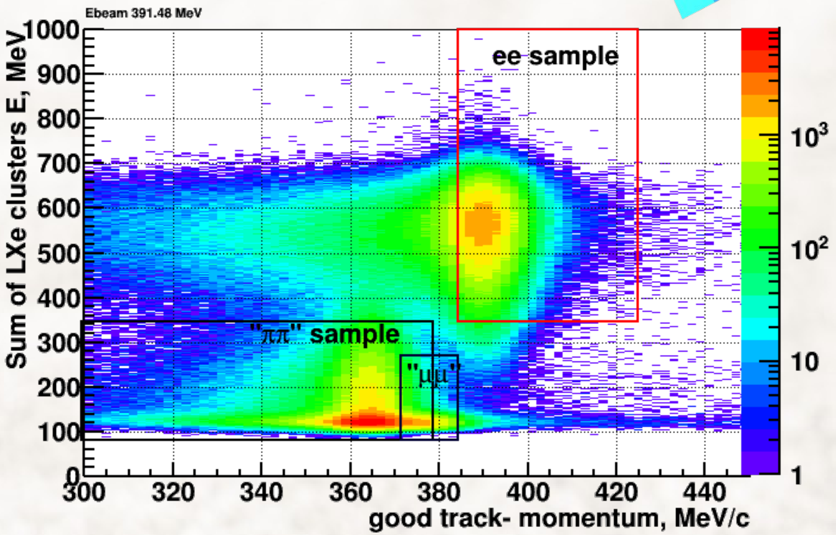
21.“Not all of such lost events were included in the test samples and were accounted for in the efficiency determination described in the previous section”. How are obtained these test samples?

22. “The already accounted part of this losses for ππ events is about 30% at ρ resonance energies, ee 5% and μμ less than 10%.”. What the “accounted losses” do refer to?

Section 4.1 of the arxiv paper, Slides 24 on the TI talk (27.03.23)

Efficiency





Assuming independence of Calorimeter & Tracker,
Using the “test” sample based on LXe information:

two collinear clusters are detected + one good track

gives possibility to study track reconstruction inefficiency

Event type is tagged by
energy deposition and momentum of good track

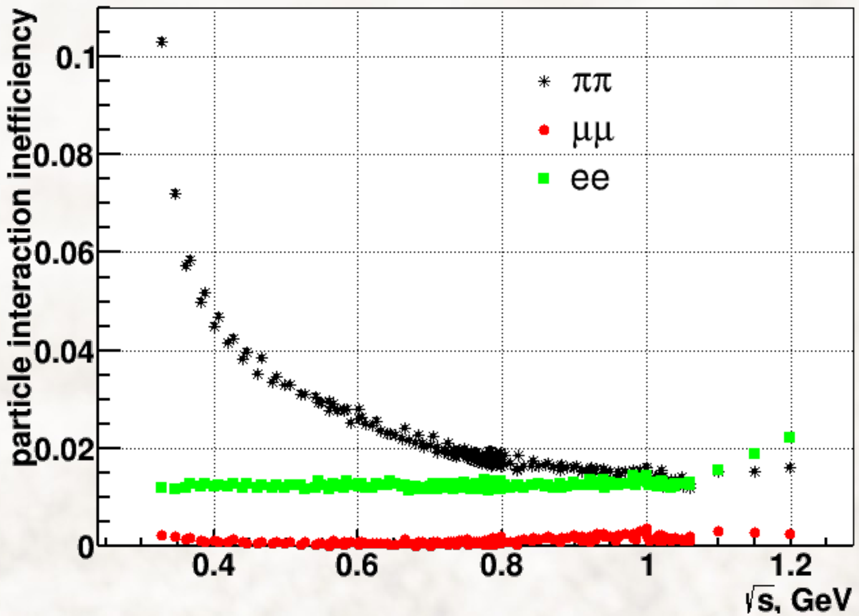
The “test” sample includes only partially some specific losses (when second compatible cluster is not produced):
pion decay, nuclear interaction, .. (~30% ineff. accounted)
electron bremsstrahlung (~5% accounted)

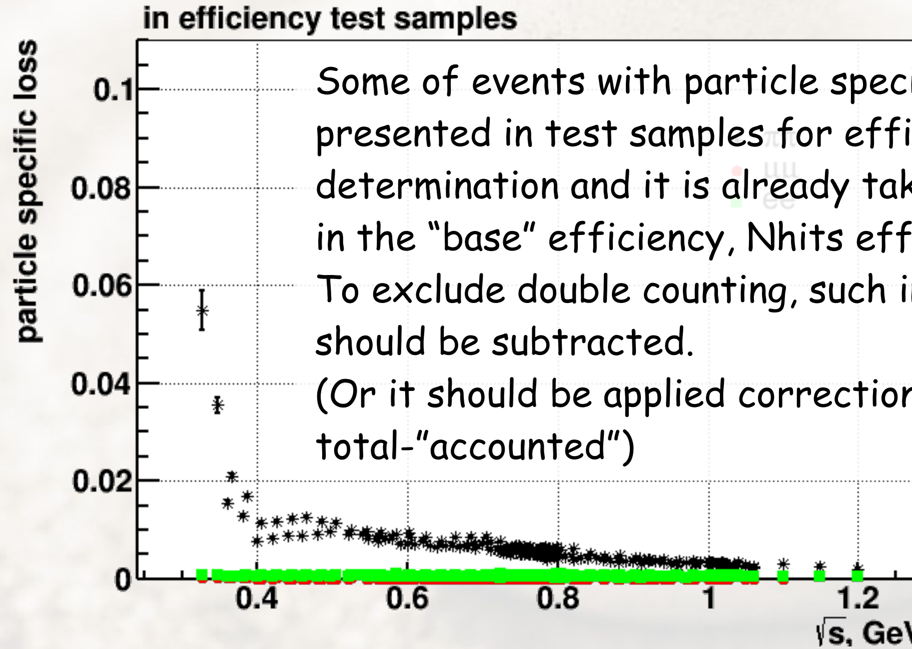
N.B. Correlated inefficiency study was also performed
without requirement on detection of one good track

Question 22

22. “The already accounted part of this losses for ππ events is about 30% at ρ resonance energies, ee 5% and μμ less than 10%.”. What the “accounted losses” do refer to?

After nuclear loss, pion decays, bremsstrahlung: it is not always present second collinear clusters (or just two clusters in calorimeter), etc





Some of events with particle specific losses are presented in test samples for efficiency determination and it is already taken into account in the “base” efficiency, Nhits efficiency, etc.
To exclude double counting, such inefficiency should be subtracted.
(Or it should be applied correction for total-“accounted”)

Question 23

23. How the sample with "subset of events with no any listed above processes" are obtained?

The particle specific losses are extracted by using the full MC simulation of the detector. The corresponding correction is taken as the ratio between the full efficiency obtained in the MC simulation and the efficiency obtained with the subset of events when no any mentioned above process is happened in the tracking volume.

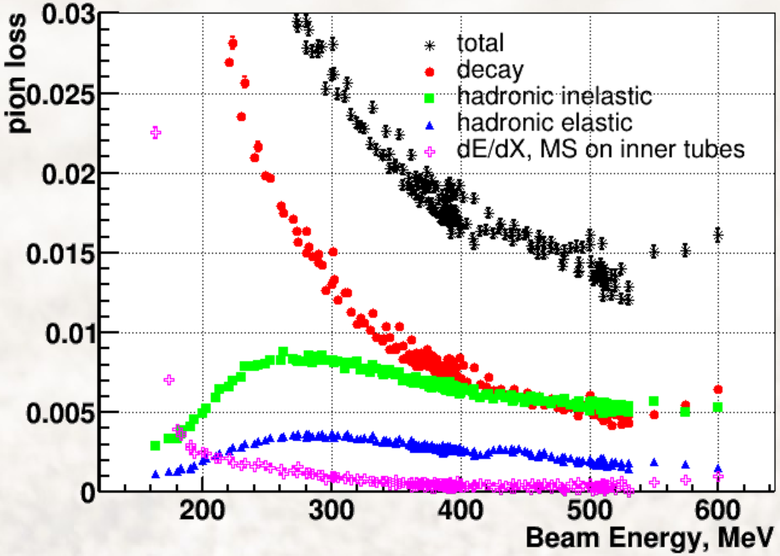
Subset of events with no any listed above process are selected according to the *geant4* information: it should be no vertices with pion decay, nuclear interaction, brems. losses and etc inside of the DCH

$$\varepsilon^{particle\ specific\ losses(PSL)} = \frac{\varepsilon^{full\ MC}}{\varepsilon^{MC\ events\ when\ no\ PSL\ verticies\ inside\ DCH}}$$

Question 24

24. Pion interaction losses: assumed nuclear cross sections in GEANT known to 20%, leading to quoted 0.2% systematic. Do you have a check comparing distributions of data with MC?

Nuclear interaction loss comes mainly from interaction on the beam vacuum tube and the DCH inner wall. It is doesn't depend on detector conditions.
Material description in the full MC is cross checked by bremsstrahlung loss correction (slides 36,37 for Question 19, or slide 29 for the TI talk), and it gives consistent thickness of material between MC/data at <5%.
The 20% uncertainty comes from Geisha/Fluka comparison by CMD-2 and comparison with data.
So I rely also on this knowledge.



Indirect data/MC comparison with zero energy deposition in LXe of pions ~ 20% compatible (Question 1) – in data nuclear interaction is higher

Hadronic inelastic loss ~0.7% at ρ

20% estimation is not far from BaBar, KLOE experience for cumulative MC/data effect
0.5% at 1.7%
0.3% at 2.5%

Question 25

25. Pion decay losses: large effect even at ρ peak (1%) limited test from momentum distribution, but affects tracking, momentum, energy deposition, 0.1% quoted systematic. How well is the “decay” correction known?

Yes, it is known with 0.1% systematic precision.

Main problems comes from detectors related conditions on reconstruction of decayed tracks.

Number of decayed track is controlled by number of events in momentum tails (or by other words reconstruction efficiency of this decayed tracks).

Relative consistency <2-3% between data/MC → <0.1% systematic precision of correction

Other check by “weak” cuts (slide 31): Nhits >= 10 → 8, $\chi 2 < 10 \rightarrow 20$, $|\Delta\rho| < 0.3 \rightarrow 0.6$ cm

Pion decay correction is smaller by factor 2-2.5, just 0.3-0.4% at ρ peak

Formfactor consistent with $\Delta|F^2|/|F^2|$: RHO2013: 0.04 ±0.01%
RHO2018: 0.01 ±0.01%

N in tails in momentum distributions are free in the momentum-based separation.
It doesn't affect the energy deposition-based separation: no any knowledge on such events is necessary for PDF construction, moreover decayed events give same “MIP” signal as not decayed ππ events

Radiative corrections

Question 26 (from short list)

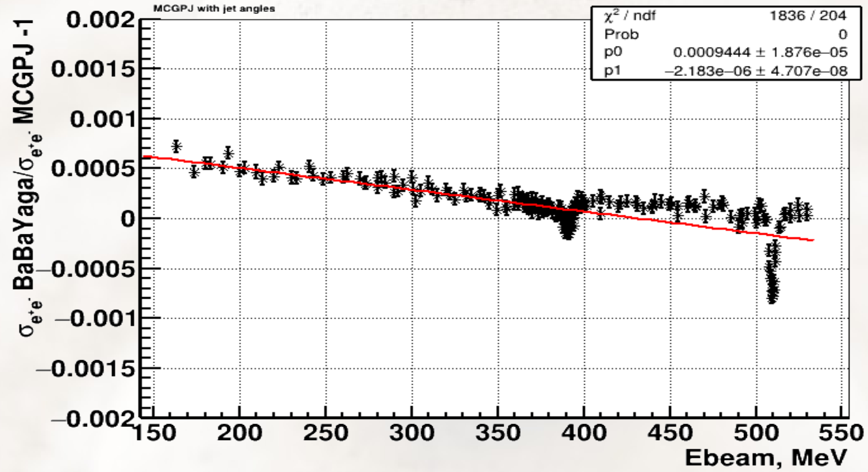
26. Two generators used (MCGPJ, BabaYaga) NLO+NNLO approximative with some differences found for ee: give more information. Does it affect also the μμ and ππ samples?

Please see more details in: https://agenda.infn.it/event/28089/contributions/147298/

Yes, μ+μ- and π+π- differential cross sections have also some uncertainty

<u>e+e-:</u>

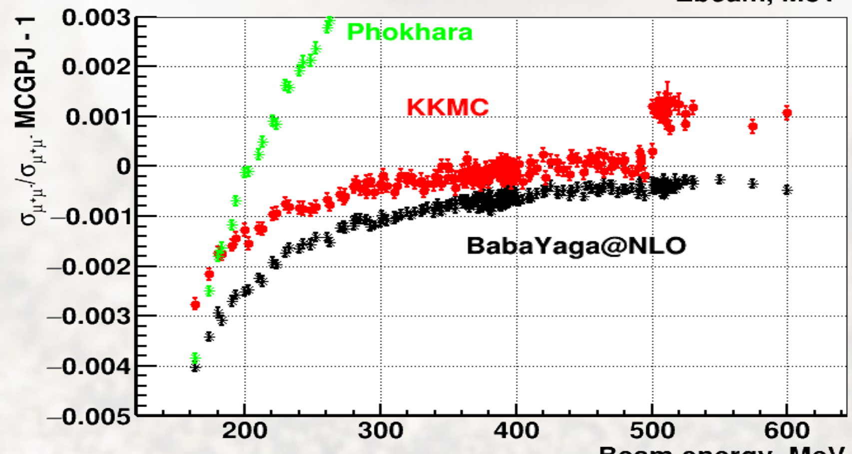
Integrated cross-section is consistent at the level <0.1% between generators



<u>μ+μ-:</u>

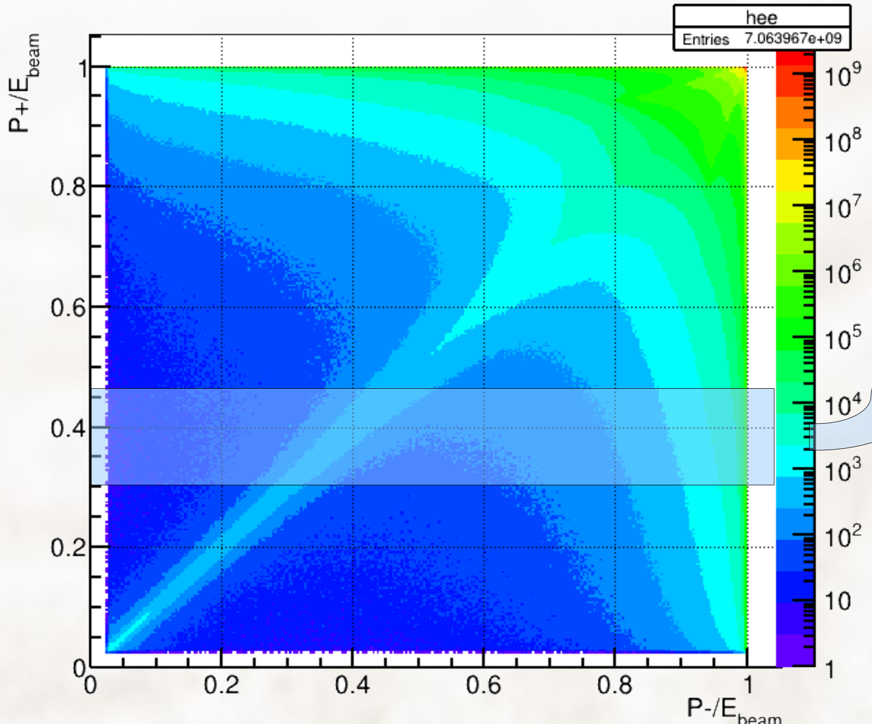
Integrated cross-section is inconsistent up 0.4%

BabaYaga@NLO, KKMC, etc – missed mass term in FSR (arXiv:hep-ph/0505236)

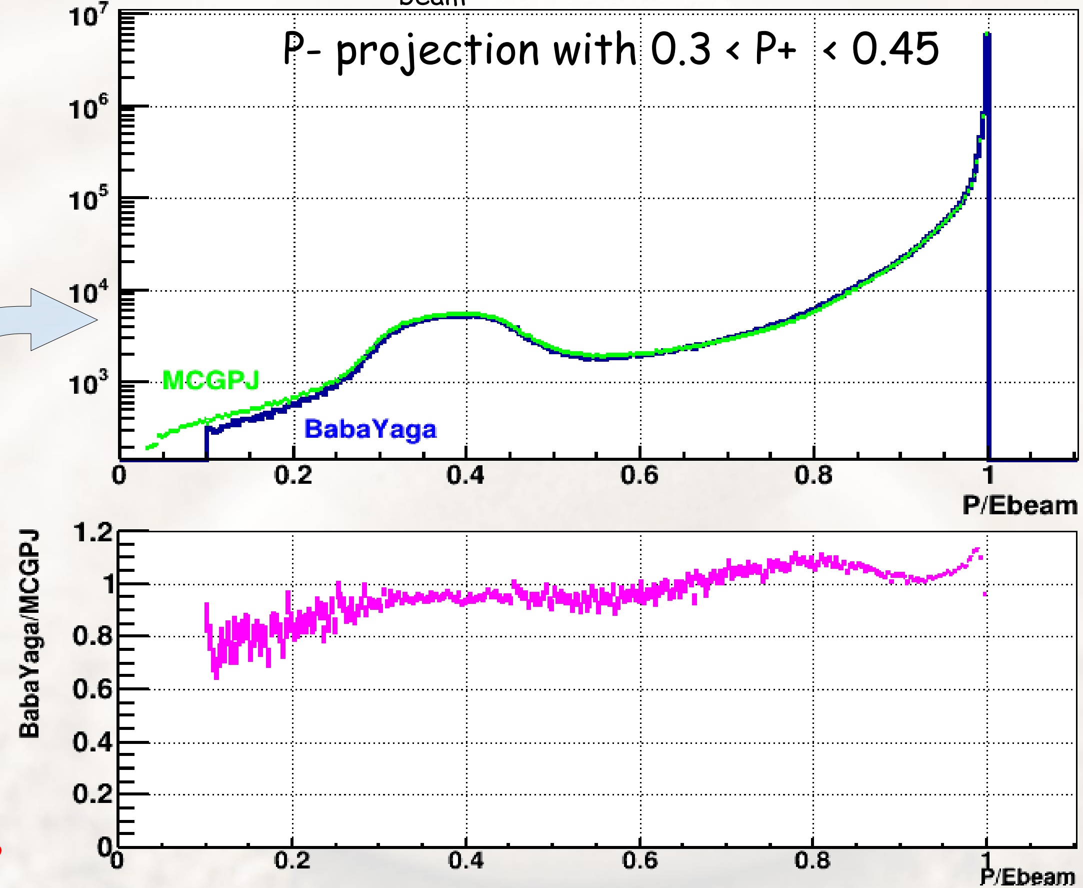


MCGPJ vs BabaYaga bhabha P+ vs P- spectrum

Differential over momentum spectrum comparison



E_{beam} 391.48 MeV



MCGPJ last improvement with jets angles reduce discrepancy from x1.6-3 to x1.1

Momentum spectrum still disagree at level ~ 10%

Tails comes from e+e- → e+e- γγ , NNLO order

Very desirable to have more precise generators

Such discrepancy gives ~0.1-0.2% systematic for π+π- at ρ-peak using momentum analysis at CMD3

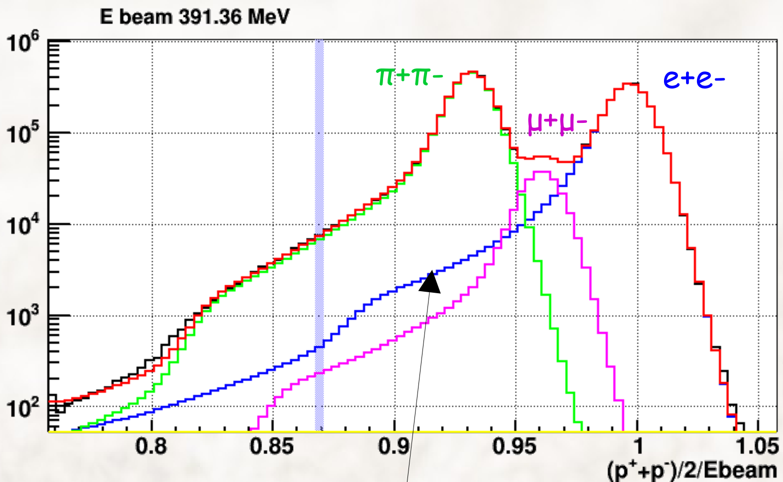
Differential cross section effect on form factor

Differential cross section knowledge is necessary for momentum-based separation

(not used in energy deposition separation)

Effect ~ 0.1-0.2% at ρ-peak

Effect comes when momentum peaks from π+π- and e+e- become close

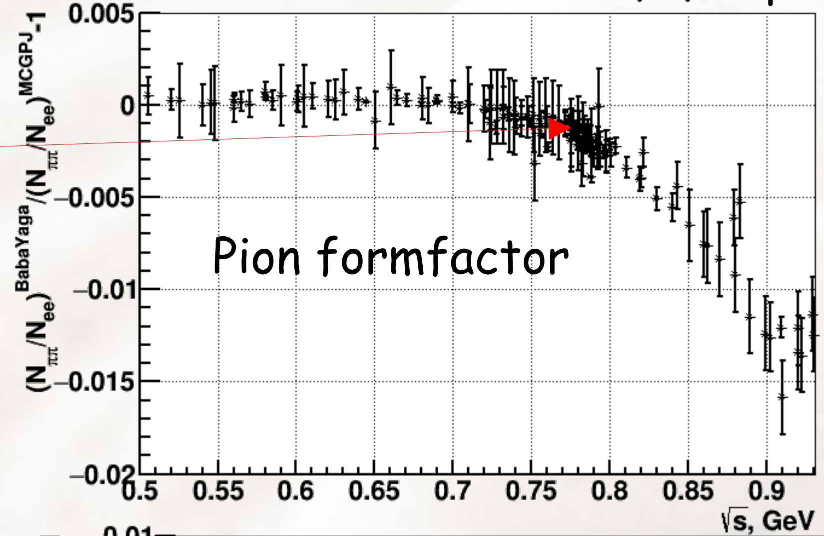


Important here soft photons radiation distribution:

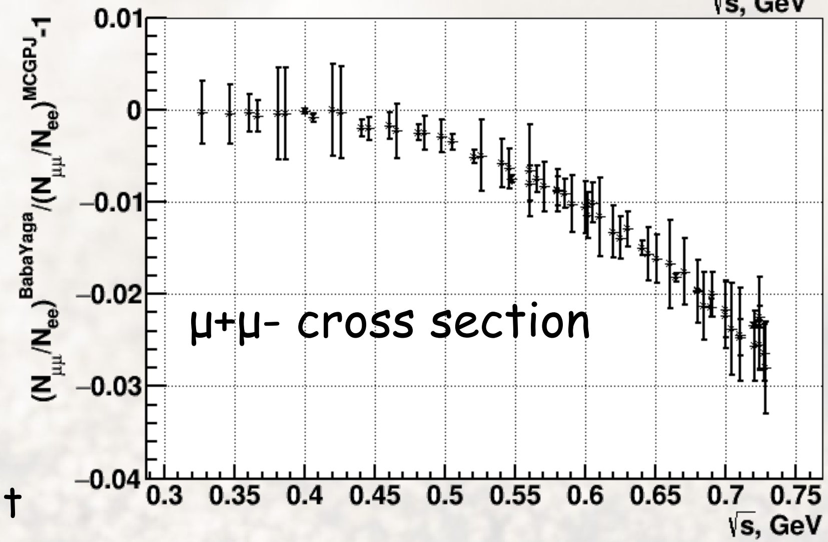
Looks like BaBaYaga@NLO approach with iterative photons generation gives better result

Difference of MCGPJ vs BabaYaga@NLO

Cumulative from e+e- and μ+μ- spectra



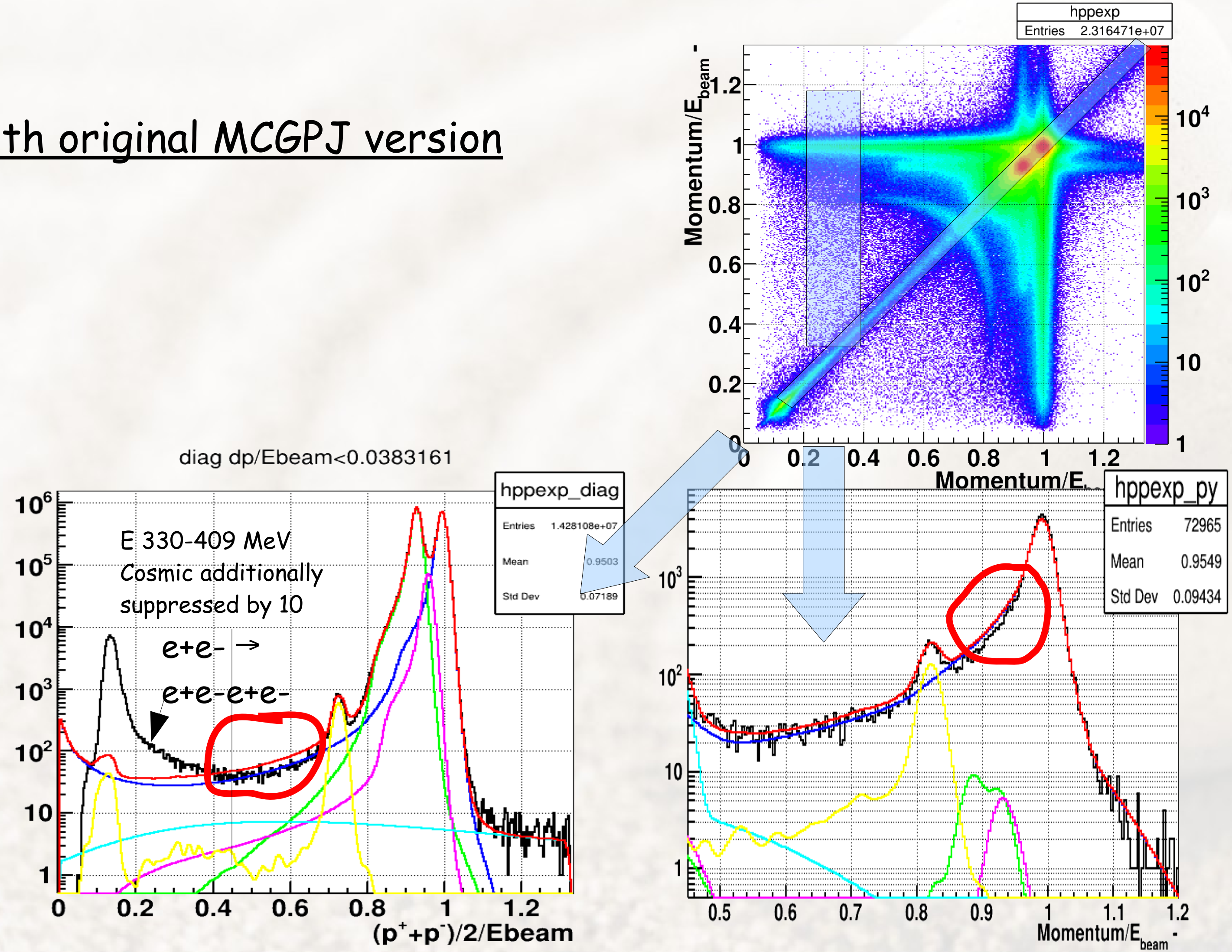
μ+μ- effect <0.05-0.1%



μ+μ- effect ~ 1/4 - 1/3

27. A problem is mentioned for the momentum distributions with MCGPJ. Please show Fig. 6-7 using MCGPJ.

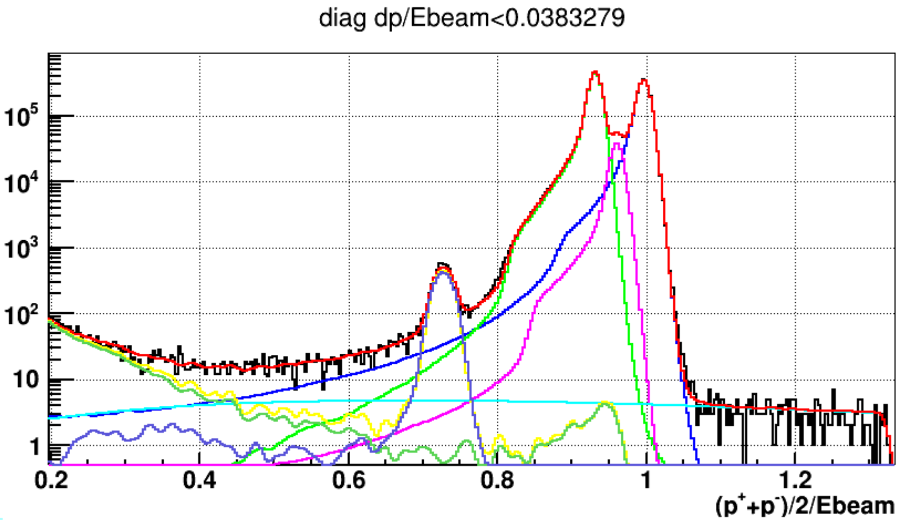
With original MCGPJ version

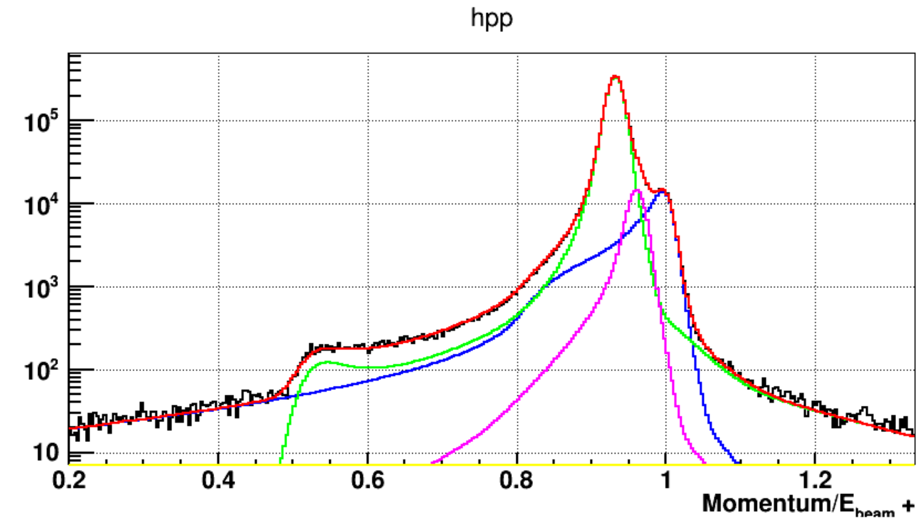


All events from RHO2013 scan
(~ 10 millions of e+e- and π+π-)

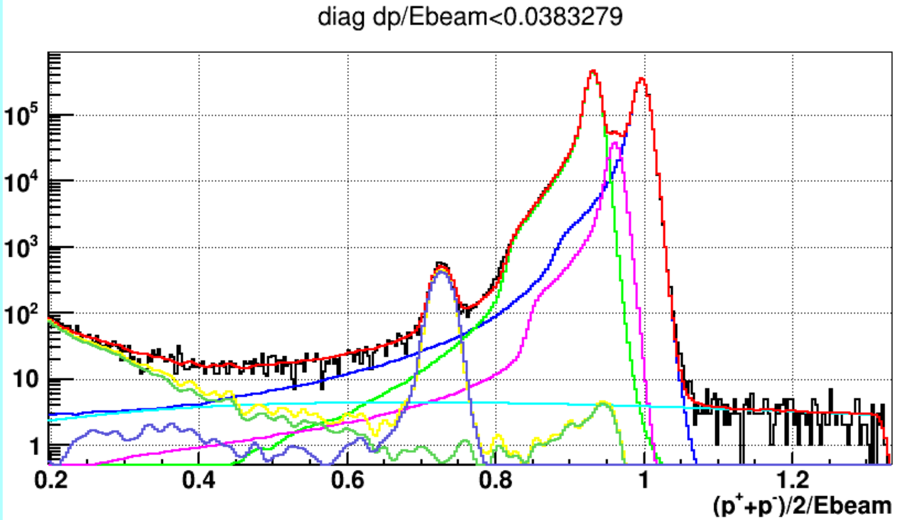
Question 27

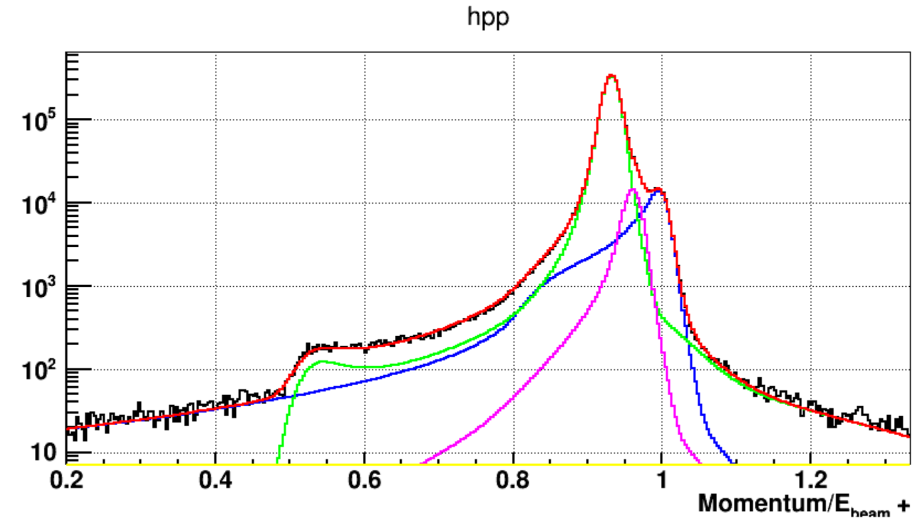
BabaYaga@NLO
e+e-, μ+μ-





MCGPJ with jet angles
e+e-, μ+μ-





Question 28

28. The problem is claimed to be partly cured by introducing an angular distribution for the photon jets. Is this physical? Wouldn't you expect different angles for each extra photon?

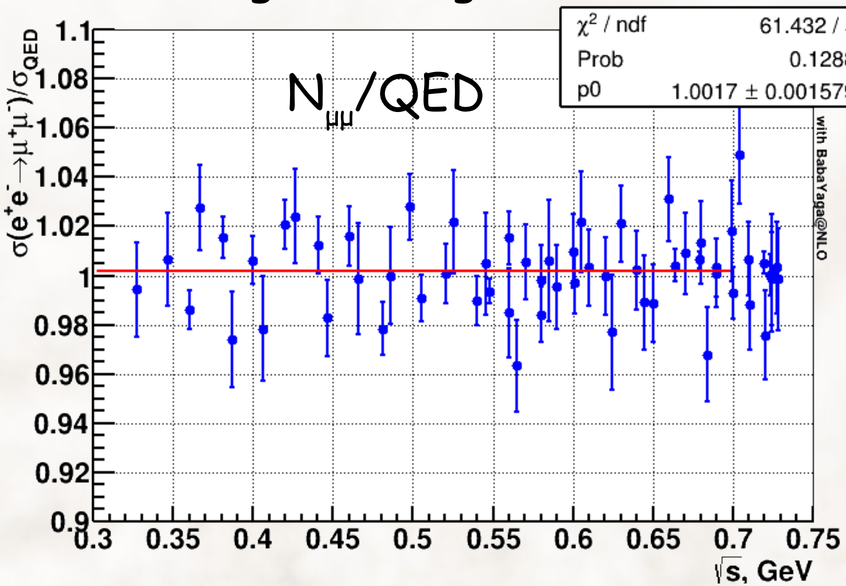
Yes, it is true. It is an approximation.
MCGPJ doesn't have separate extra photons:
There is only jets per lepton with summed energy according to the structure function, or one hard photon on large angle.

Probably this is the reason why BabaYaga@NLO works better:
For momentum-based separation, important difference comes from soft photons radiation region (when momenta of e+e- and $\pi^+\pi^-$ start to be close) – many photon radiation plays role.
Looks like BaBaYaga@NLO approach with iterative photons generation gives better result to describe this soft region.

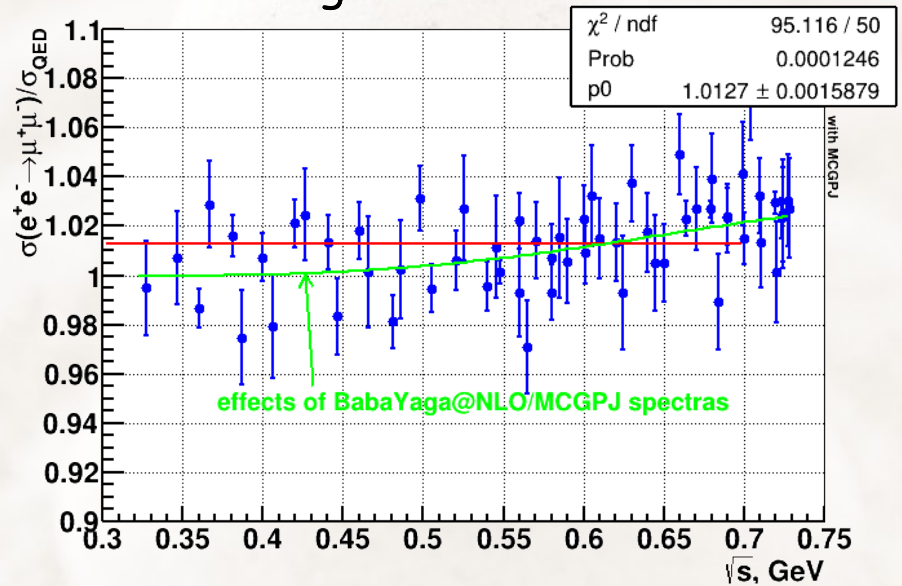
Question 29

29. It seems to affect measurement only above 0.75 GeV for pions, but above 0.4 GeV for muons (Fig. 20, also 1.3% difference quoted p. 36, 10 x larger than the statistical accuracy). In Fig. 30 the agreement is with BabaYaga. Yet MCGPJ is used for pions. Please clarify.

Using BabaYaga@NLO



Using MCGPJ



I was tried to answer for same question on Slide 49 (questioon 26):

The effect in the momentum-based separation comes when peaks of π+π- and e+e- start to be close. Momentum peak of π+π- stay on tail of e+e- momentum distribution and description of this e+e- tail plays role. For example effect on the $N_{\mu\mu}/N_{ee}$ ratio from momentum distribution of μμ itself is 1./4-1./3 less than from e+e-. Same can be expected for π+π-.

Also I tried to use π+π- momentum distribution from Phokhara for PDF construction (next few slides)

→ effect only 0.03% of $|F_\pi|^2$ on Ebeam = 391.36 MeV point.

Questions 30, 32 (from short list)

Question 30: How can you justify a 0.2% error for the ππ mode in MCGPJ given the large uncertainties seen for the Bhabha mode?

Question 32: The RC are large +8% at 0.9 GeV and -9% at 0.7 GeV. What is the uncertainty specific to this analysis, from the used generators. The number 0.2% quoted is for the integrated cross sections ('declared' by MCGPJ authors) , but apparently not listed in Table 2. Also what about NLO+HO differential cross sections? Need to be clarified.

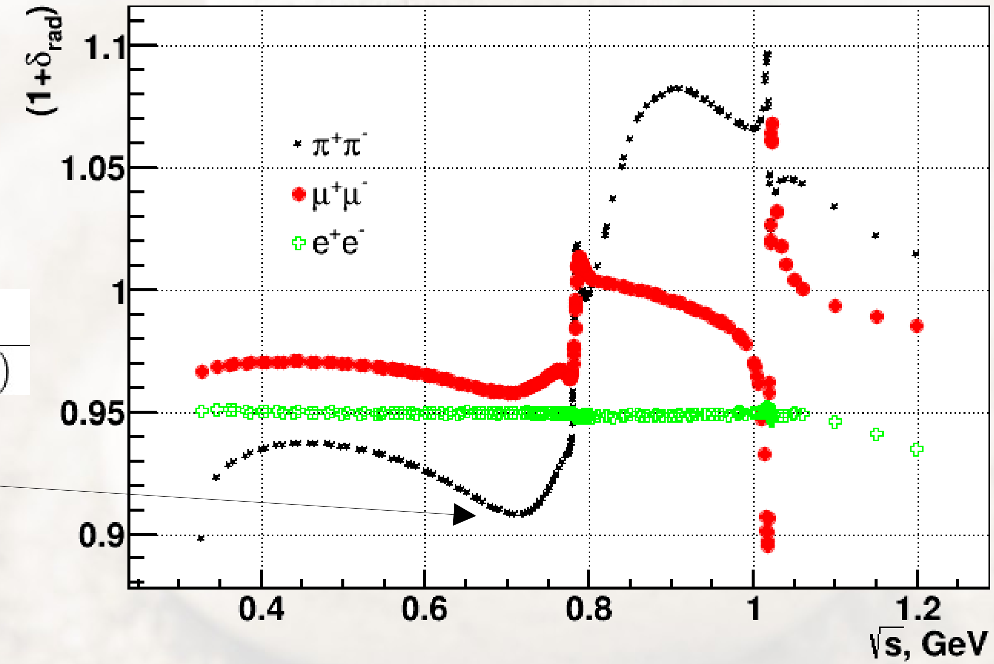
N.B. Integrated cross section in Bhabha mode was always consistent between generators at ~ < 0.1%

0.2% from MCGPJ is listed in Systematics Table 2:

Contribution
$0.2\%\ (\pi^+\pi^-) \oplus 0.2\%\ (F_\pi, \sqrt{s} > 0.74\ \text{GeV}) \oplus 0.1\%\ (e^+e^-)$

+8%/-9% wave comes from F_π and ISR

Uncertainty from different F_π parametrizations is second part in radiative correction uncertainty



Differential cross section doesn't affect energy deposition-based separation.

Looking on Nμμ/Nee in momentum-based separation, the effect from ππ spectra probably is smaller than from e+e- spectra (0.1-0.2% at ρ)

ππ generator

For ππ mode

Unfortunately only MCGPJ available with declared 0.2% precision (for energy scan experiments)

Phokara and BabaYaga 3.5 are incomplete at NLO level for energy scan mode: there is no FSR

Very desirable to have new precise generator with above sQED which will cover ISR up to $E\gamma=0$

The table with applied radiative corrections in this analysis is part of arXiv submission, It will be useful for cross-checks if new generators will be appeared.

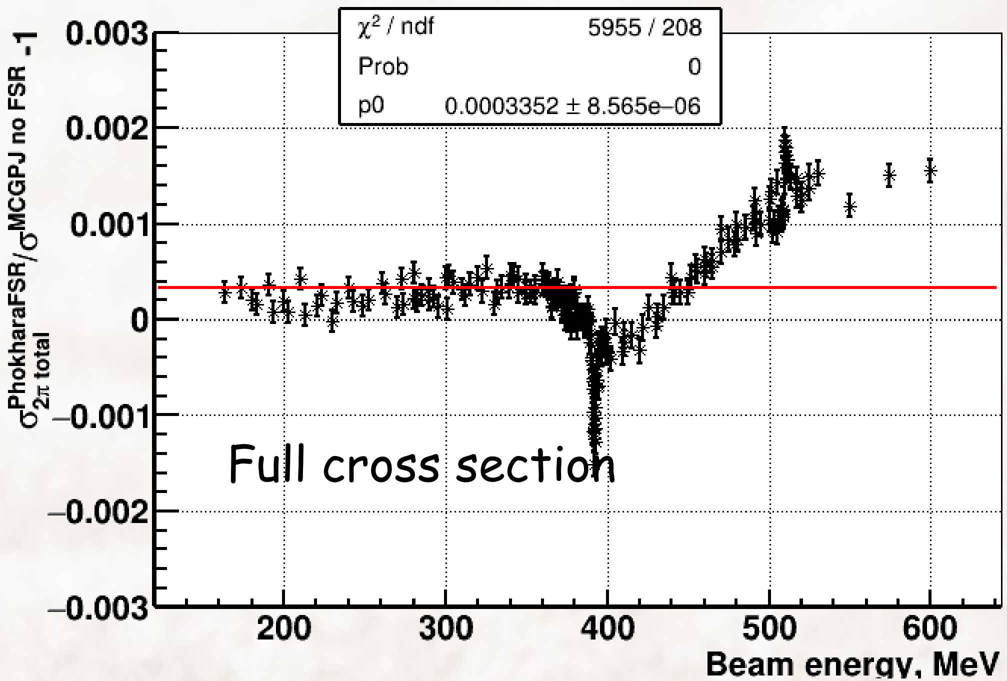
Some cross checks to compare MCGPJ/Phokara were performed

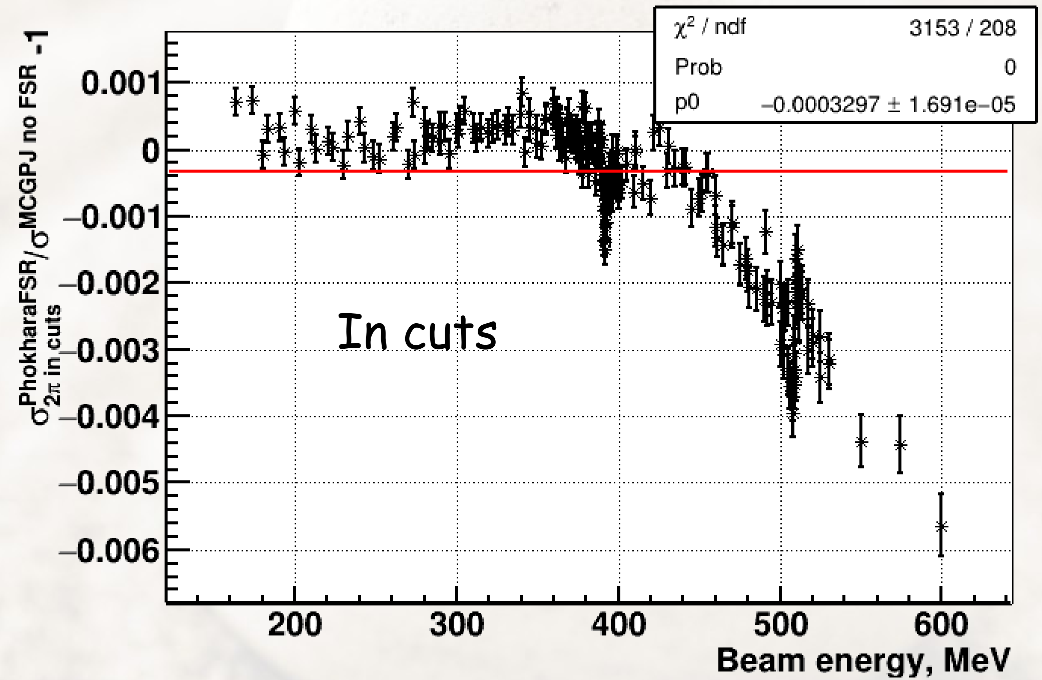
At E_{beam} 391.48 MeV point: If to use Phokara momentum spectra for ππ PDF instead of MCGPJ → 0.03% difference on F_π

MCGPJ/Phokara

ISR and Fπ cross check

MCGPJ with FSR off,
Phokara 10 with same |Fπ| as in MCGPJ, additional VP off

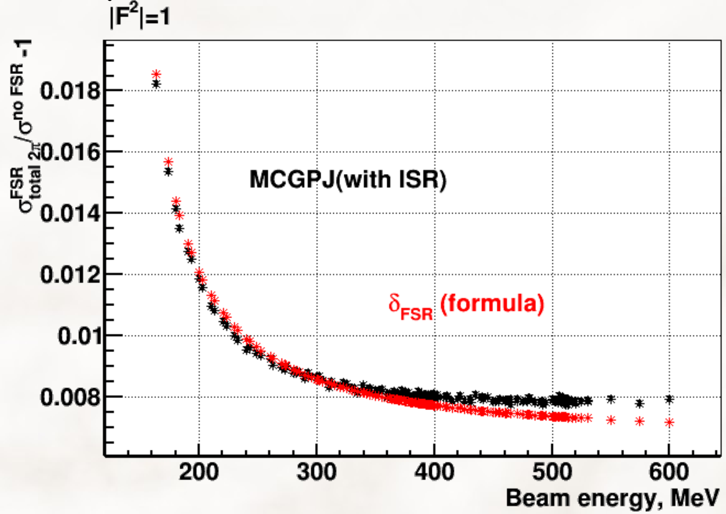


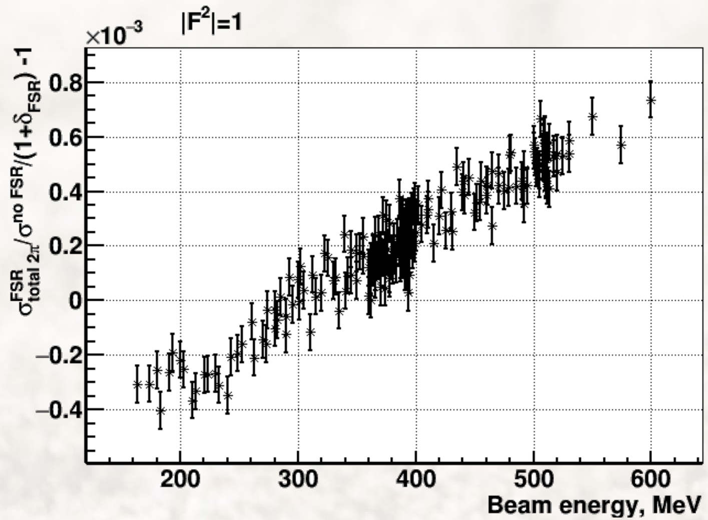


Cross section is consistent at ~0.05% at ρ-peak
(at phi ~ 0.25%)

MCGPJ FSR contribution

With Fpi=1 FSR is consistent with analytical formula at < 0.05%

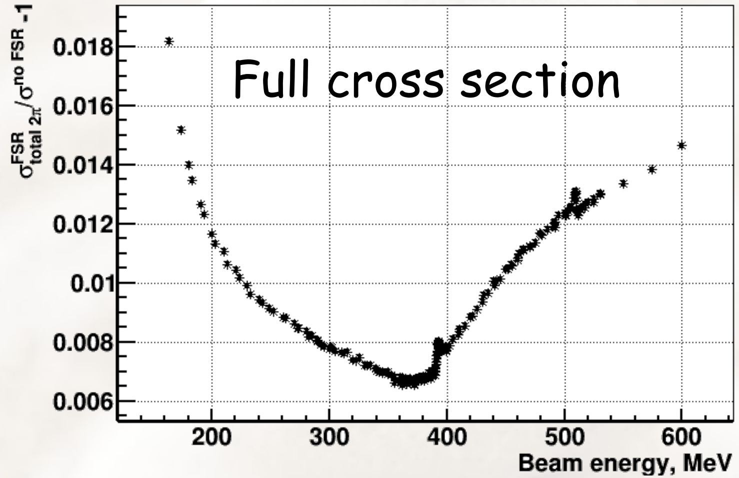


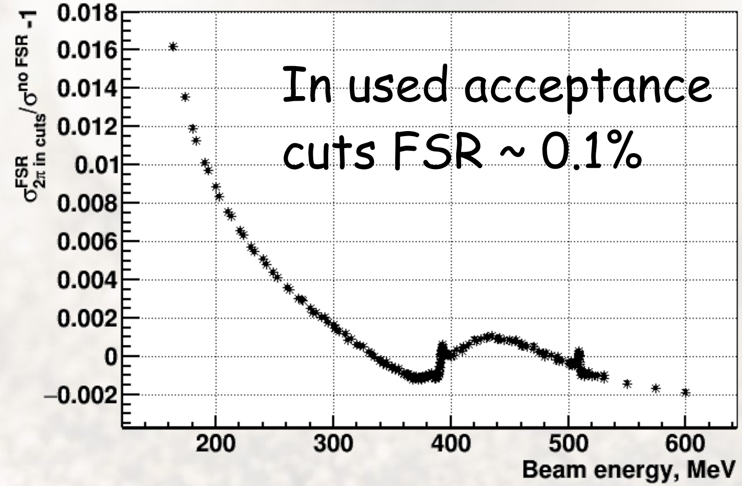


$$\frac{\sigma^{FSR}}{\sigma^{no\,FSR}} - 1$$

With full formfactor behaviour it is different because of ISR return.

Looks reasonable





Question 31

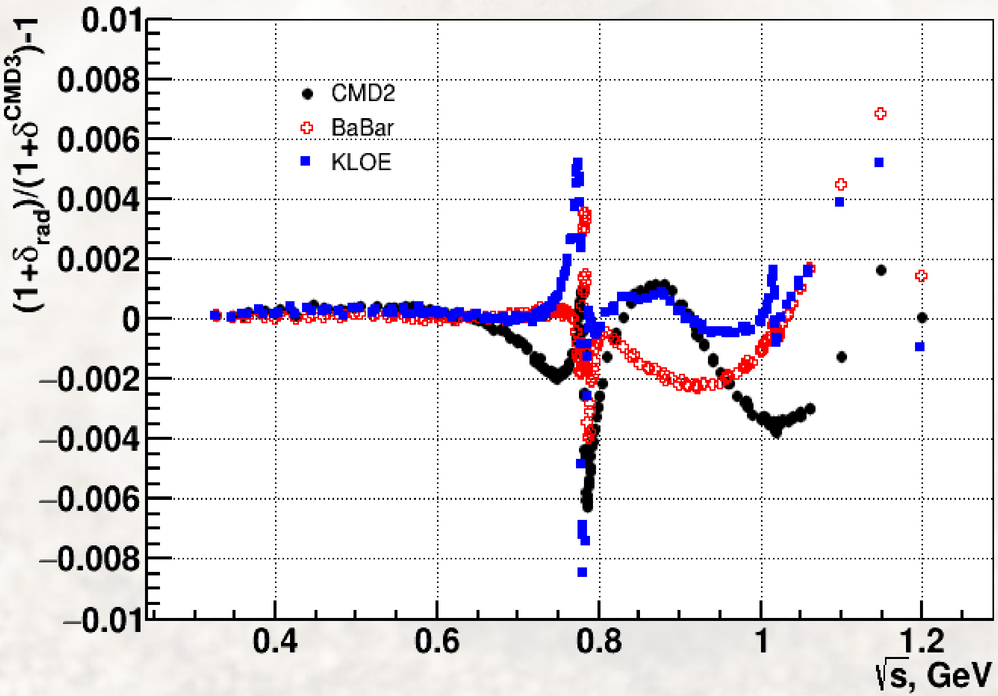
31. Why quote a systematic uncertainty on the RC only from form factor parametrizations in other experiments, since the iterative procedure uses the CMD-3 data and so should be self-consistent?

Not only, it is also with CMD3 form factor parametrization. The plot on radiative correction is shown as relative to CMD-3 case.

The radiative correction itself is taken from CMD-3 parametrization – so it is self-consisten.

Yes, quoted systematic uncertainty is estimated by looking on different datasets (like theoretical view above different experimental measurement)

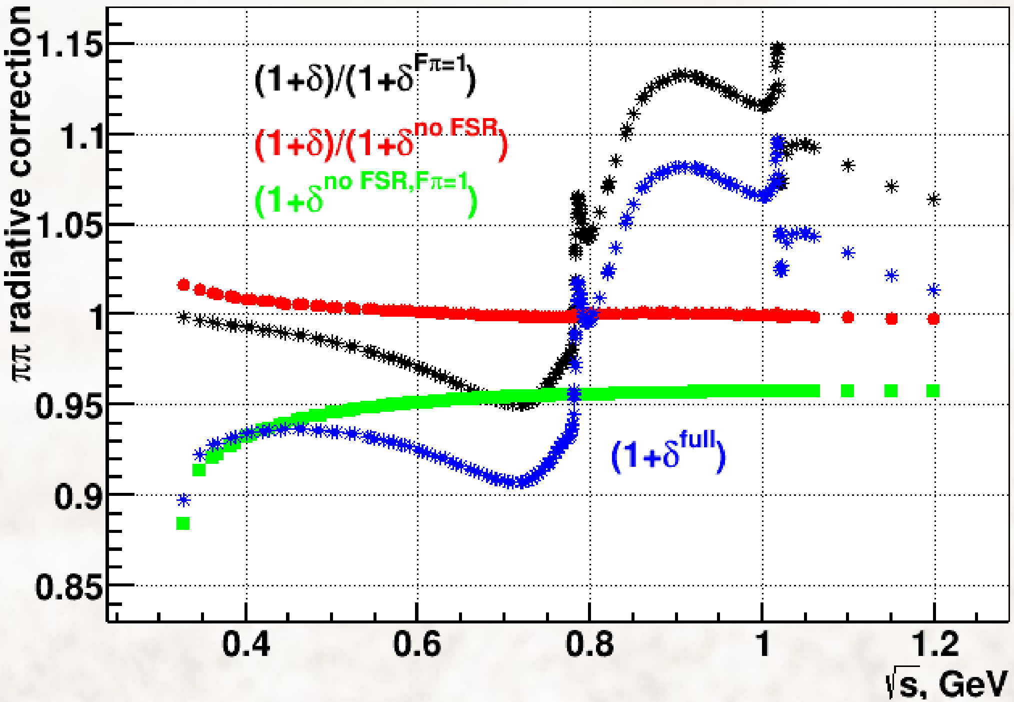
Effect on 2π radiative correction from different $|F|^2_\pi$ parametrizations (over different datasets)

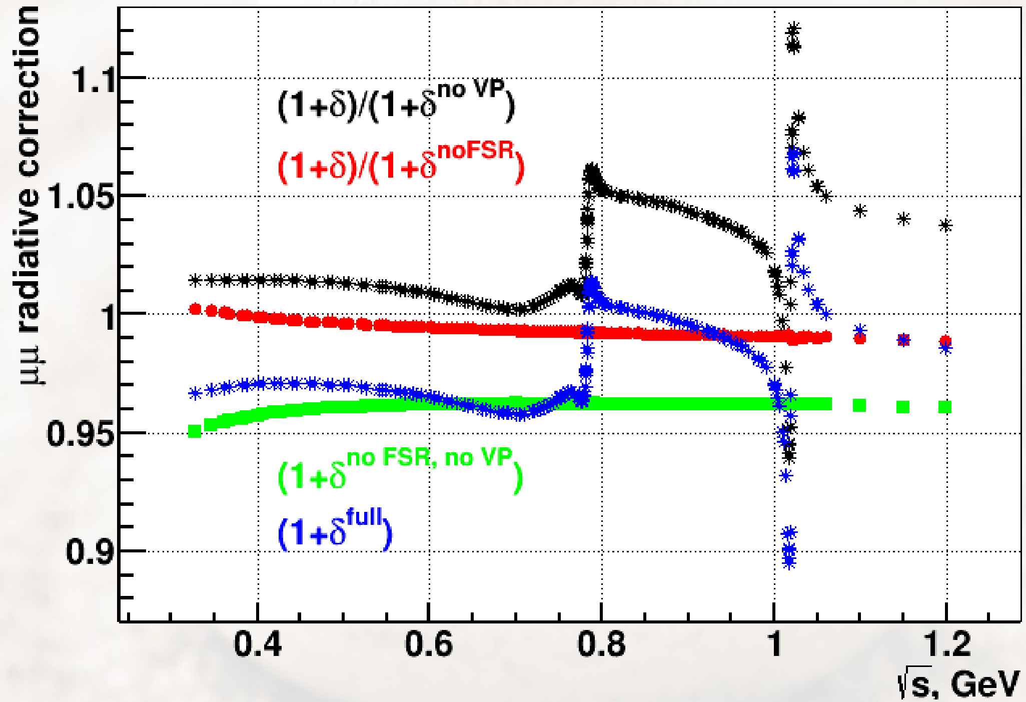


Question 33

33. In Fig 21 would be possible to distinguish the different sources of RC (ISR, FSR and VP) for the three sample (ee, μμ, ππ) also when Babayaga@NLO and MCJPG are used (for ee, μμ)?

Using MCGPJ





For e+e- it is no separate formulas without FSR in the MCGPJ generator.
Effect from the VP is much smaller as t-channel dominated.

General procedure

Question 34

34. In Fig. 2 the BaBar statistical uncertainties within the enhanced bin sizes are probably computed using the published statistical covariance matrix, accounting for the (anti-)correlations between the original bins. Can you please confirm?
If that's the case, please note that this matrix includes both data and MC stat uncertainties (e.g. from the unfolding), including also the uncertainty from MuMu luminosity, from background subtraction and from data/MC corrections.
Do the values of the statistical uncertainties for the other experiments include the statistical uncertainties from the background subtraction and from the other corrections that are being applied?

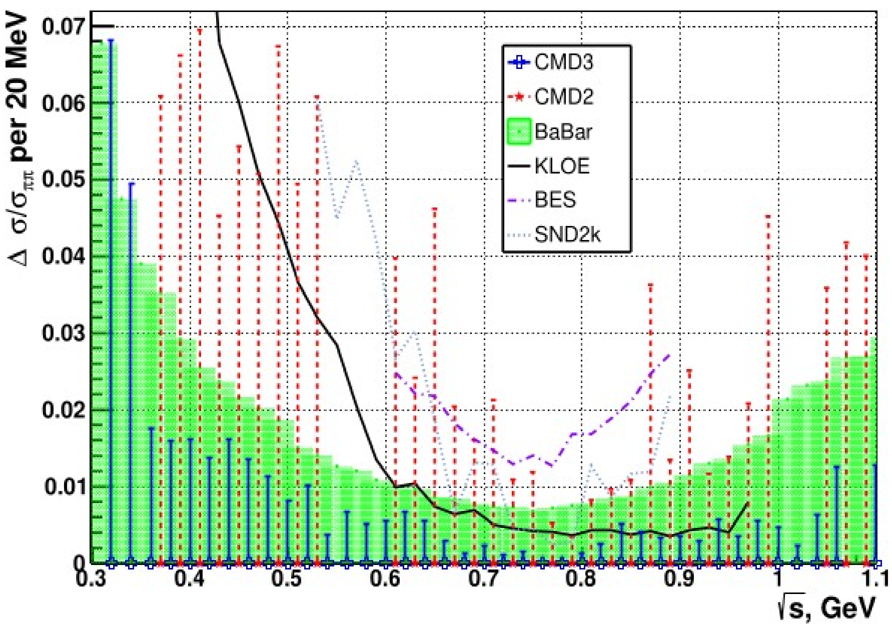


Figure 2: Relative statistical precision of $|F_\pi|^2$ from the CMD-3 data in comparison with the CMD-2, BABAR, KLOE, BESIII and SND@VEPP-2000 results. Integrated statistic over 20 MeV bin is shown.

Yes, it is taken as sum over the published statistical covariance matrix.
Same things are for KLOE and now BES.
(BES in arxiv version occasionally was using diagonal values from last paper – but it was assumed previous publication without cov matrix)

CMD2,CMD3 and SND2k are using statistical uncertainties of $|F_\pi|^2$
So this is full Luminosity+Analysis related stat. erros as it was mentioned in the caption.

P.S. What is addition to pure luminosity statistics? Is it quite different?

Question 35

35. The fact that the value for Nππ/Nee is consistent within 0.2% between angular fit, momentum and energy separation does not automatically imply that the systematic uncertainty is <0.2% if there are correlations between the methods (for ex. angular distribution and momentum separation, both using only tracks). Please justify.

Sure, even two uncorrelated statistical values can be statistically at same side

The Logic is different: Possible biases are checked on full MC → systematic biases are estimated independently per each separation method.
Comparison of different methods gives the additional cross-check and ensure us, at least at central region, that 0.2% systematic uncertainty estimation is safe.

Question 36

36. Since 0.2% is quoted for the range 0.381 - 1 GeV can the test be repeated below/above the restricted 0.7 - 0.82 GeV range used in the test?

For sum of $\sqrt{s}$ = 0.7 – 0.82 GeV points

by momenta in DCH: $N_{\pi\pi} / N_{ee}$ = 1.0193 +- 0.00030

by energies in LXe $\Delta N_{\pi\pi} / N_{ee}$ = -0.09 +- 0.024%

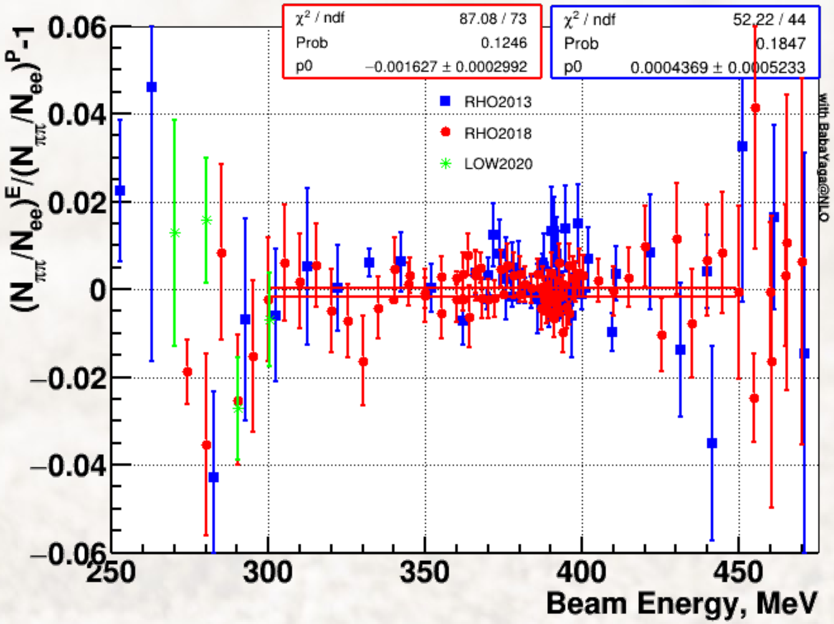
from theta with free δA: = -0.20 +- 0.12%

with fixed δA=0: = +0.21 +- 0.07%

consistency at ~ 0.2%

Common stat from √N: 0.026%

Taken as central most important energy region on peak of ρ



Point by point average $\Delta N_{\pi\pi} / N_{ee}$ (E/P -1) at

0.82-0.95 GeV	0.06±0.23%
0.7 -0.82	-0.116±0.027%
0.6 -0.7	-0.09±0.09%

N.B. mometum-based separation corrected for bias according to MC

Question 36

Average number of events:

0.82-0.95 GeV

P-based $N_{\pi\pi} / N_{ee}$ = 0.27808 ± 0.00077

E-based $\Delta N_{\pi\pi} / N_{ee}$ = +0.72 ± 0.29%

θ +2.06 ± 0.69%

0.6 -0.7 GeV

P-based 0.41816 ± 0.00033

E-based -0.31 ± 0.12%

θ +0.47 ± 0.52%

Momentum-based is biased in this range,
Should be compared E/θ

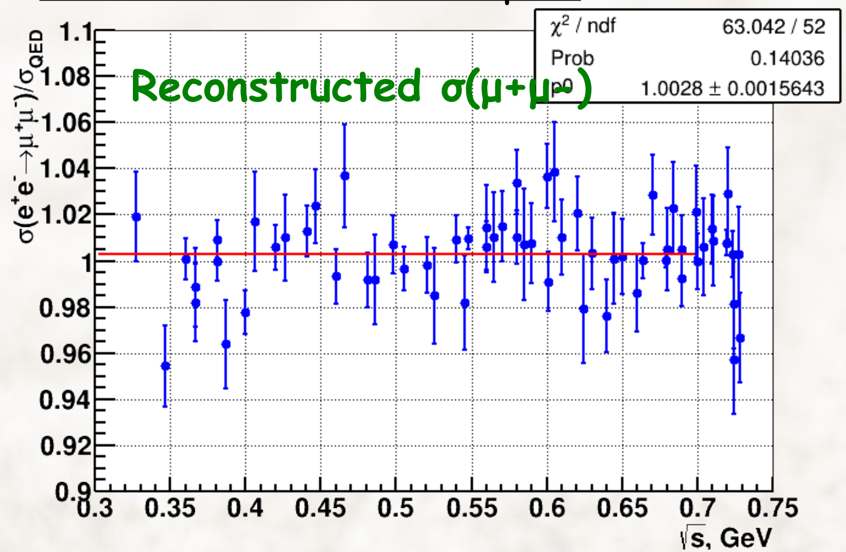
Questions 37,38

37. In Fig. 28 right there seem to be some structures (i.e. oscillations) in the shape of sigma(mu mu) / QED, while the chi2 fit is also not sensitive to these structures (i.e. one can obtain a good chi2/ndof even in presence of significant oscillations on sub-ranges). Has the significance of these oscillations been quantified and has their source been studied? It seems the fit in this figure is also not performed on the full sqrt(s) range.
38. What is the difference between the sigma(mu mu) / QED plot in Fig. 30 and the one in Fig. 28 right ? The above-mentioned oscillations seem to be absent in Fig. 30.

<u>Full analysis workflow was checked on mixed full MC data samples</u>

<u>From data</u>

Fig. 28



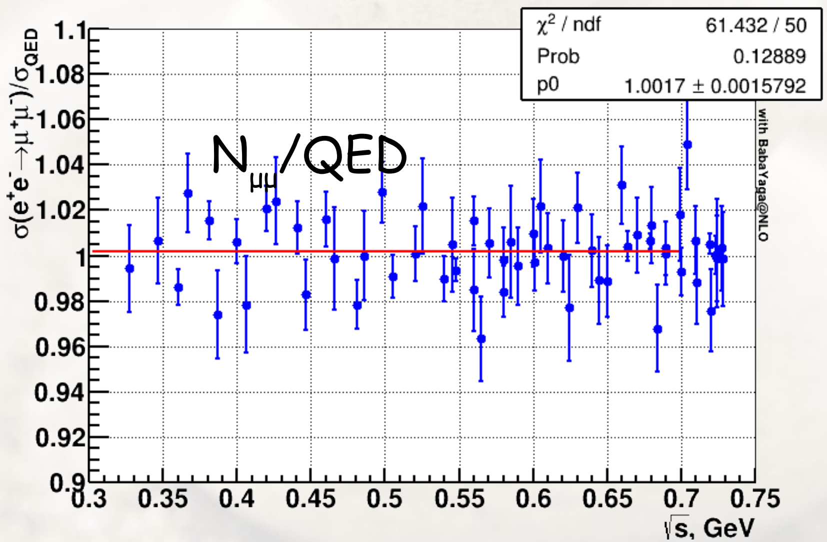


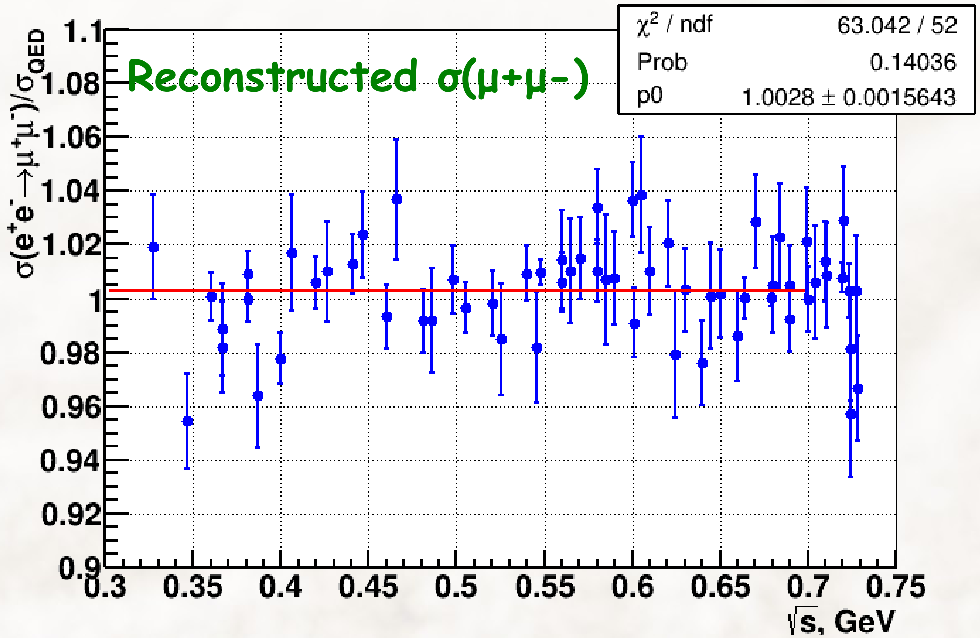
Fig. 30

Fit in 0.55-0.62 GeV: 1.0138 ± 0.0043
Can be just statistical fluctuation

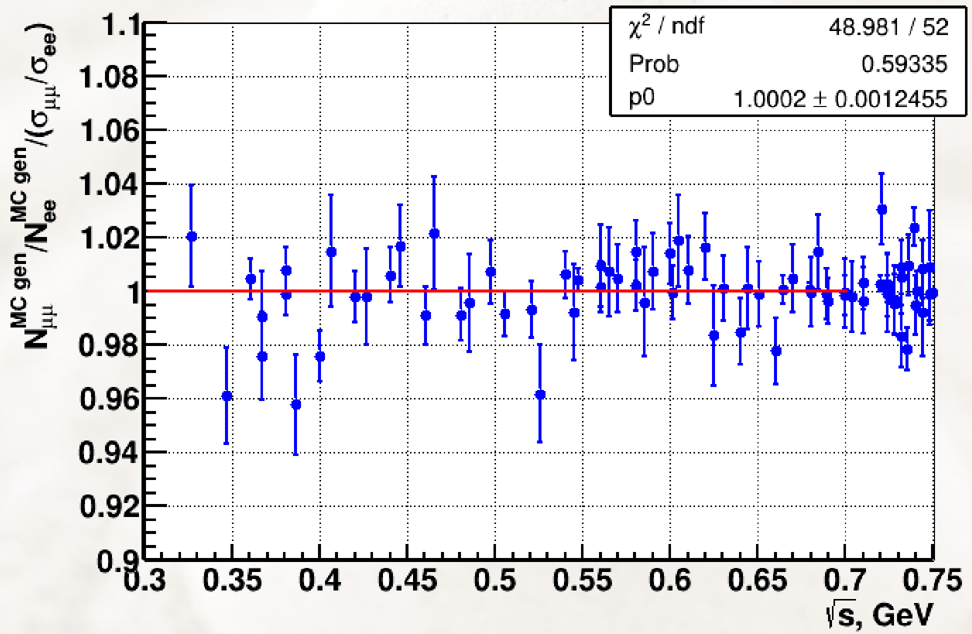
Yes, fit performed up 0.7 GeV as I limit itself on this energy, above of this it is shown for the demonstrative purpose

Question 37,38

Full analysis workflow was checked on mixed full MC data samples



Number of events on the generator level in selected cuts



Fit in 0.55-0.62 GeV: 1.0138 ± 0.0043

Can be just statistical fluctuation

Fit in 0.55-0.62 GeV: 1.0062 ± 0.0035

Maybe also can be seen some oscillation...

Question 39

39. Would it be possible to have a prescription for the treatment of the systematic uncertainties in phenomenological studies? Are all the sources to be treated as independent between each-other, while each of them is fully correlated across the phase-space?

We assume that the systematic error is 100% correlated between points.

The biggest effect comes from the detector related source (fiducial volume).
This can depend on detector conditions with time. Further, the corresponding value of the systematic error can be the same (in the correlated way) between different energy range regions or it can be somehow different.
We cannot guarantee one of these scenario.

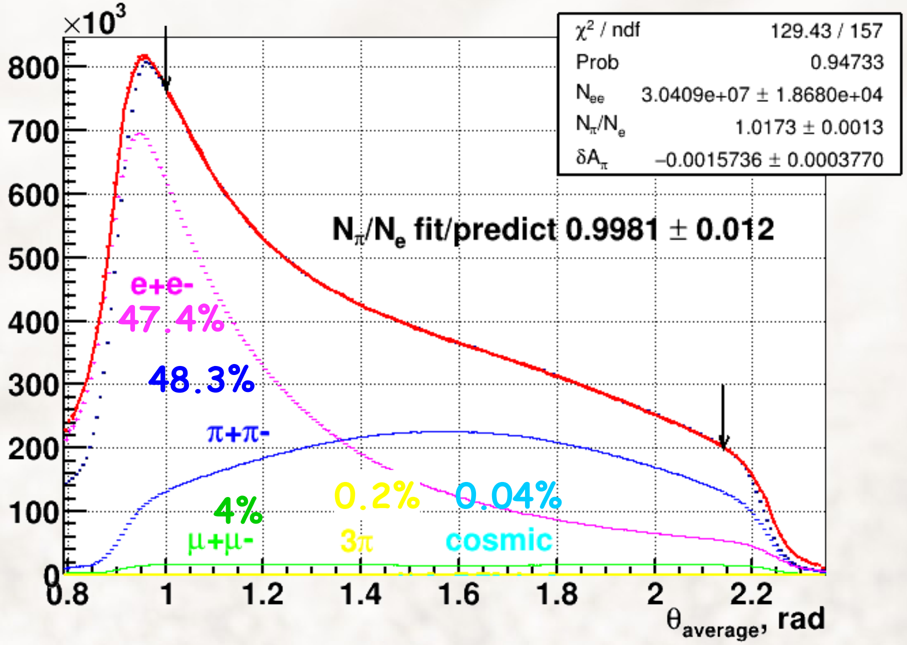
In my opinion the best way will be to treat systematic uncertainty in most worst case, which should depend on a particular application.
For example, for the a_μ integral: the total systematic errors should be treated as 100% correlated (gives biggest contribution to the systematic uncertainty of the a_μ value).
But it should be also considered possibilities (for systematic studies of final value) with reduced correlation between lower/middle/upper energy ranges.
By other words, the uncertainty of error correlation should be also taken into account into each specific application.

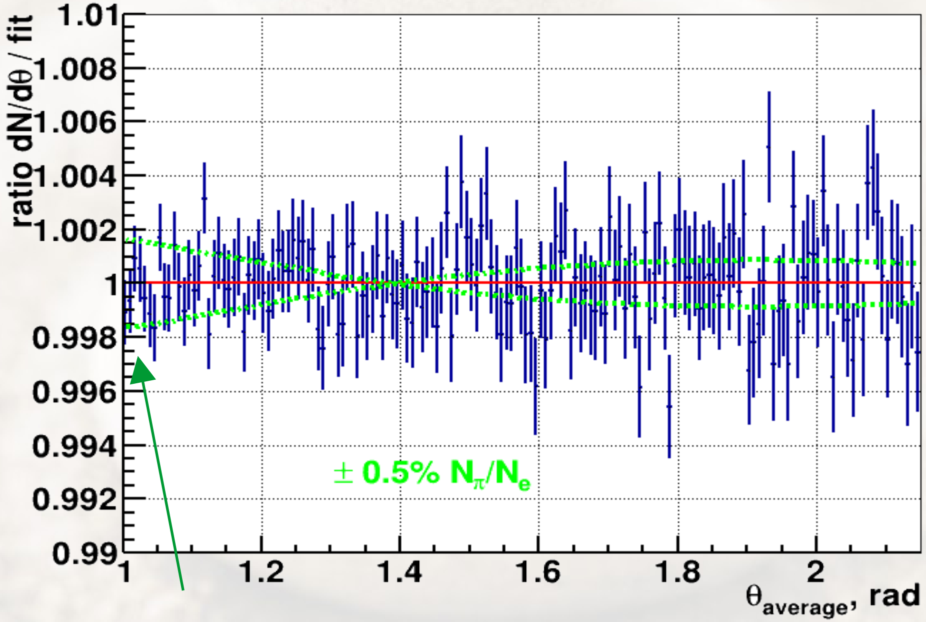
40. Section 6.2: What would be the agreement if one uses only MC samples (without additional data/MC corrections)?

(Section 6.2. Particle separation based on polar angle distribution)

All point at √s = 0.7-0.82 GeV

Fit by θ distribution



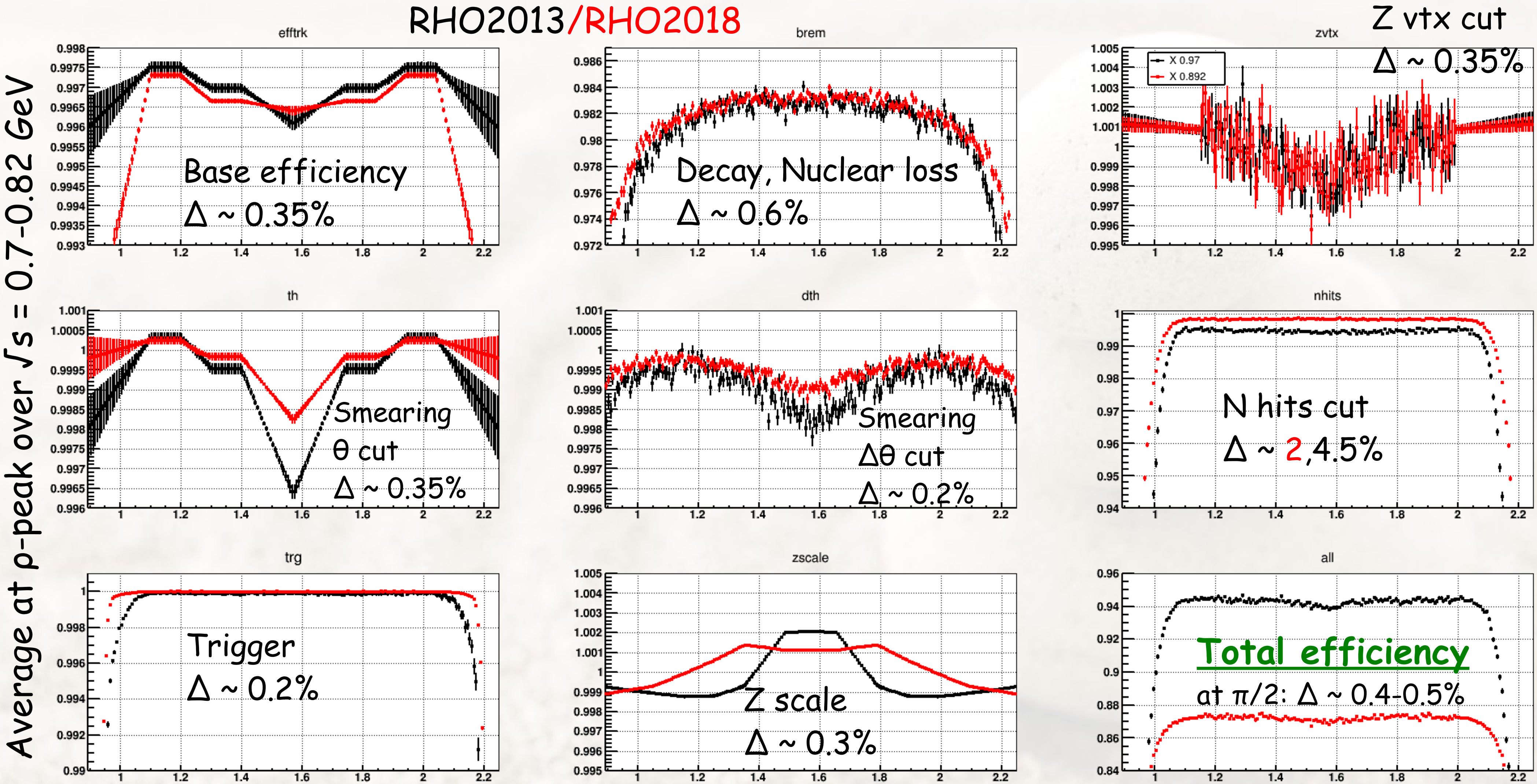


No issue in accounted efficiency at θ = 1 rad

Nμμ/Nee fixed

3π, cosmic from P - separation

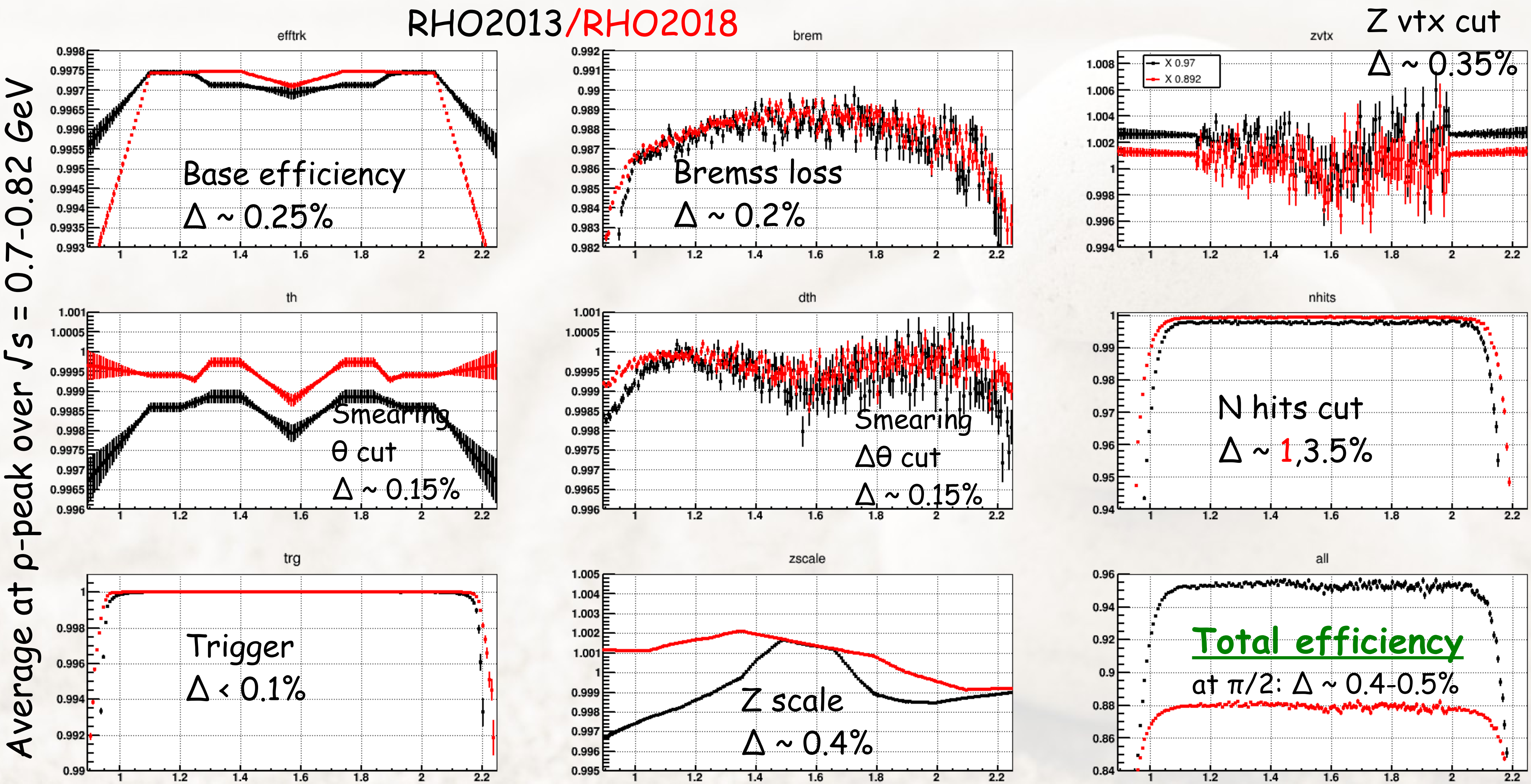
π+π- efficiency vs θ polar angle



Two features: edge behaviour – tracks go out throw DCH endplate (less N hits)

and at θ = π/2 - amplitude on wires smaller (charge screening effect) , σθ – x2 higher (to θ = 1rad)

e+e- efficiency vs θ polar angle

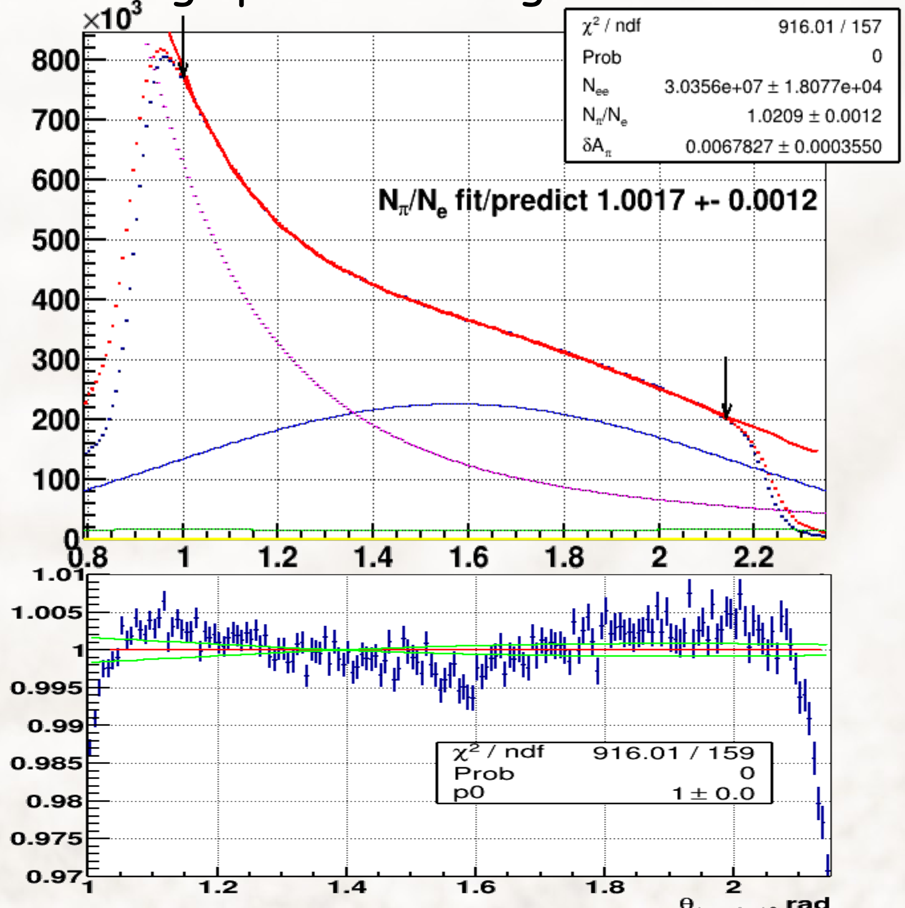


Two features: edge behaviour – tracks go out throw DCH endplate (less N hits)

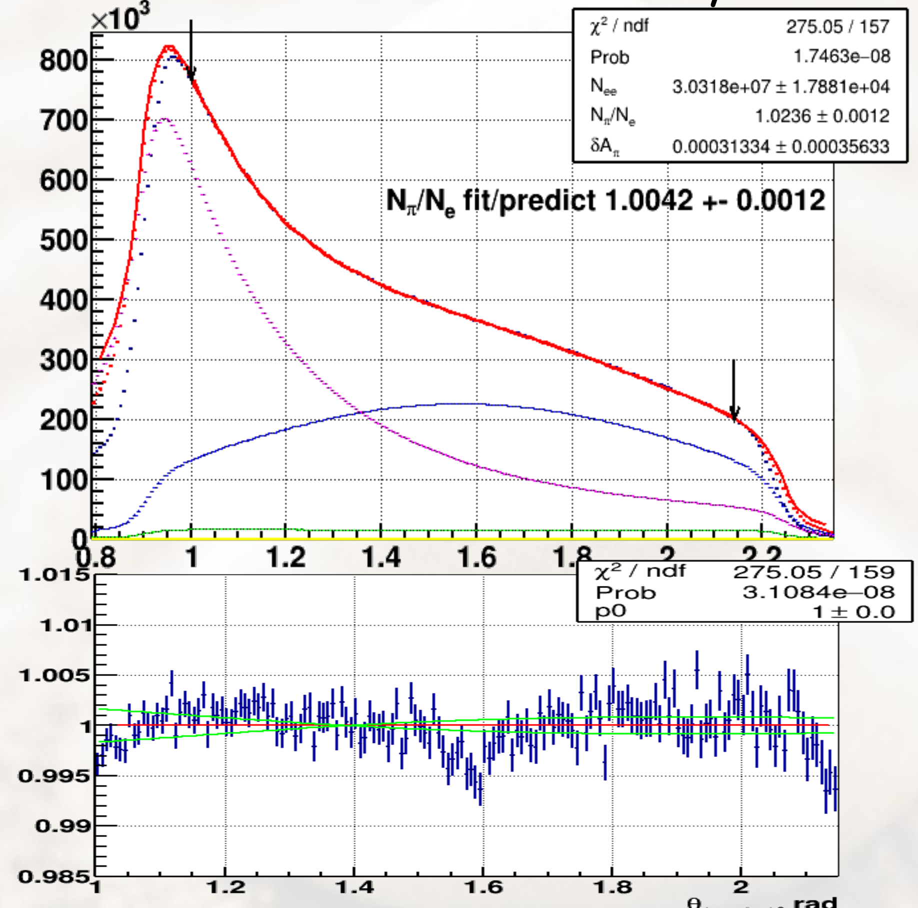
and at θ = π/2 - amplitude on wires smaller (charge screening effect) , σθ – x2 higher (to θ = 1rad)

Theta distribution fit

Using spectra from generators



Generators + Nhit efficiency



	Using spectra from generators	Generators + Nhit efficiency
RHO2013:	1.41 +- 0.23 %	0.32 +- 0.23 %
RHO2018:	-0.29 +- 0.14 %	0.42 +- 0.14 %
Sum:	0.17 +- 0.12 %	0.43 +- 0.12 %

Relative to the mometum-based separation

Effect of efficiency on $N_{\pi\pi}/N_{ee}$~ 0.5-2. %

Question 41

41. Is there any way to check the correctness of the corrections obtained from the fit to the PDF vs the ones obtained by the fit in the angle? (It's unclear to me what the corrections from PDF refer to, and therefore having a dependence on the angle could be useful to clarify that)

? question related to Section 6.2. Particle separation based on polar angle distribution

Probably it is some misunderstanding here in the difference between ISR analysis and this one (what is the leading order effect in each analyses).
If I'm not mistake: in the ISR analysis case all parts of momentum distributions are important and corrections to it in the first order are the efficiency corrections, right?
In this analysis, most of the events are placed in corresponding collinear peaks.
And when we speak about momentum-based separation, to construct the PDF, the addition to initial spectra from generators are the empirical function for the detector effect – which are mainly resolution + some specific part related to pion decay spectra, bremsstrahlung momentum loss spectrum. And then after the likelihood fit, the efficiency correction (one number per process) is applied for the Nππ/Nee ratio.

And in case of the fit of angle spectrum:
Yes, the corrections (as shown 2 slides before) are the mainly efficiency + some θ resolution in dependence on θ-angle. For the momentum-based and energy deposition based separation, this efficiency is used as integrated to single number and applied after to calculate the $|F_\pi|^2$ value.
N.B. angle distribution gives good possibility to cross check that N_{hit} inefficiency is accounted properly, otherwise it will be a sharp drop at θ~1rad in the residual of data over the fitted function.
Others are not very much connected for PDFs in momentum and angles.

Question 42

42. DM2 results above 1.35 GeV are used to constrain the high mass part with ρ(1420) and ρ(1700). DM2 results strongly disagree with BABAR in this region with a large negative interference not seen in DM2 because of μμ background. Does it affect the fit in the ρ region?

Yes, the BABAR is more precise compared to DM2!
It was used less precise data from the DM2 and CMD-2 for purpose of not over constrain the form factor fit by precise BABAR measurement.
Just roughly introduce ρ(1700) in the fit and give more freedom for CMD-3 data itself.

The effect of using BABAR data (>1.2 GeV) instead of DM2,CMD-2 was studied for the parameters of $\varphi \to \pi^+\pi^-$ decay, and it is included in systematic uncertainties of this parameters.

Probably combined fit should be with using systematic uncertainties, etc....
Now it is done using only statistical errors..

Fit with CMD2,DM2 or BABAR

Constrained fit with M,Γ of φ and ω from PDG

Parameter	with CMD-2 and DM2	with BABAR	PDG(2022)
m_ρ, MeV	775.39 ± 0.1	774.98 ± 0.08	775.26 ± 0.23
Γ_ρ, MeV	148.73 ± 0.17	148.71 ± 0.16	147.4 ± 0.8
m_ω, MeV	782.44 ± 0.03	782.4 ± 0.03	782.66 ± 0.13
Γ_ω, MeV	8.59 ± 0.06	8.54 ± 0.06	8.68 ± 0.13
$\mathcal{B}_{\omega\to\pi^+\pi^-}\mathcal{B}_{\omega\to e^+e^-}$, 10^{-6}	1.203 ± 0.01	1.189 ± 0.009	1.28 ± 0.05
$\arg(\delta_\omega)$, rad	0.169 ± 0.014	0.126 ± 0.008	
m_ϕ, MeV	1019.465 ± 0.016	1019.467 ± 0.016	1019.461 ± 0.016
Γ_ϕ, MeV	4.25 ± 0.013	4.25 ± 0.013	4.249 ± 0.013
$\mathcal{B}_{\phi\to\pi^+\pi^-}\mathcal{B}_{\phi\to e^+e^-}$, 10^{-8}	3.51 ± 0.22	3.52 ± 0.22	2.2 ± 0.4
$\arg(\tilde{\delta}_\phi)$, rad	2.767 ± 0.024	2.794 ± 0.022	
$\|a_{cont}\|$	0.0969 ± 0.0107	0.0928 ± 0.0181	
$\arg(a_{cont})$, rad	2.348 ± 0.309	3.362 ± 0.09	
m'_ρ, MeV	1226.22 ± 24.76	1479.52 ± 12.9	1465 ± 25
Γ'_ρ, MeV	272.97 ± 45.53	339.88 ± 25.57	$400. \pm 60$
m''_ρ, MeV	1604.66 ± 30.8	1876.97 ± 19.47	1720 ± 20
Γ''_ρ, MeV	249.39 ± 52.24	228.71 ± 38.51	$250. \pm 100$
$\|a'_\rho\|$	0.3589 ± 0.0693	0.1674 ± 0.0162	
$\|a''_\rho\|$	0.1042 ± 0.031	0.0782 ± 0.013	
$\arg(a'_\rho)$, rad	-1.831 ± 0.07	-1.722 ± 0.073	
$\arg(a''_\rho)$, rad	3.384 ± 0.234	3.573 ± 0.098	
χ^2/ndf	288.87 / 240	296.99 / 231	

Question 43 (from short list)

Since it is only mentioned without any detail in the conclusion, can you clarify how the blinding of the results was achieved?

It was not "fully" blinding way.
The analysis was driven by self-consistency checks without comparing with others and by list of effects which should be checked giving effects ~0.1% .
The main blocking difficulties were:

Consistency between momentum/energy deposition-based separations
(initial version of Energy based method (with LXe+CsI total energy) was having bias even on full MC data)
Discrepancy in angle distribution

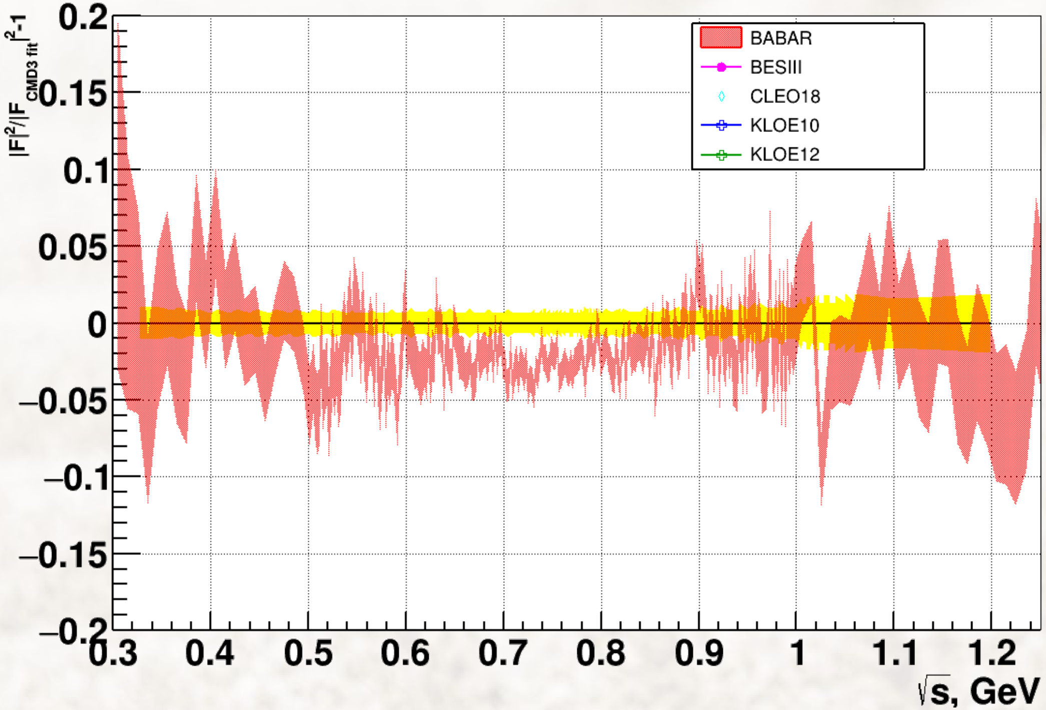
The detailed comparison with previous experiments appeared only at final stage,
when it was performed accurate fitting of final measurement, iterative recalculation of radiative correction with CMD-3 form factor parametrization, with different parametrization over different experiments, etc

The collaboration was blinded to the last moment, the day before of the public institute seminar:
The discussions on all steps of the analysis over many years in local collaboration meetings, the paper preparation, the discussion on the systematic contribution (with all effects and problems involved) were without looking on final formfactor and comparison with others.

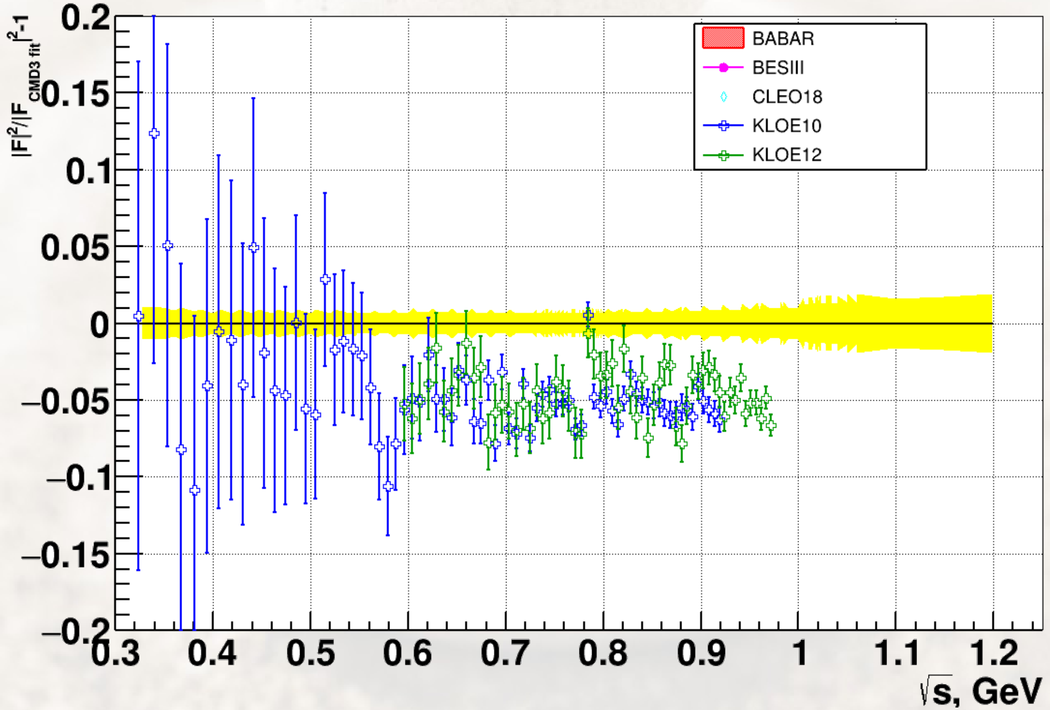
Comparison to other experiments

44. There is the same trend for CMD-3/BABAR or /CMD2 or /SND: CMD-3 excess of up to 5% around 0.7 ± 0.1 GeV (left side of ρ peak), excess extending to the highest energies for CMD-3/KLOE. But hard to distinguish the different contributions in Figs. 34-35. Plot separately CMD-3 fit/all other experiments to better assess the discrepancies.

BABAR



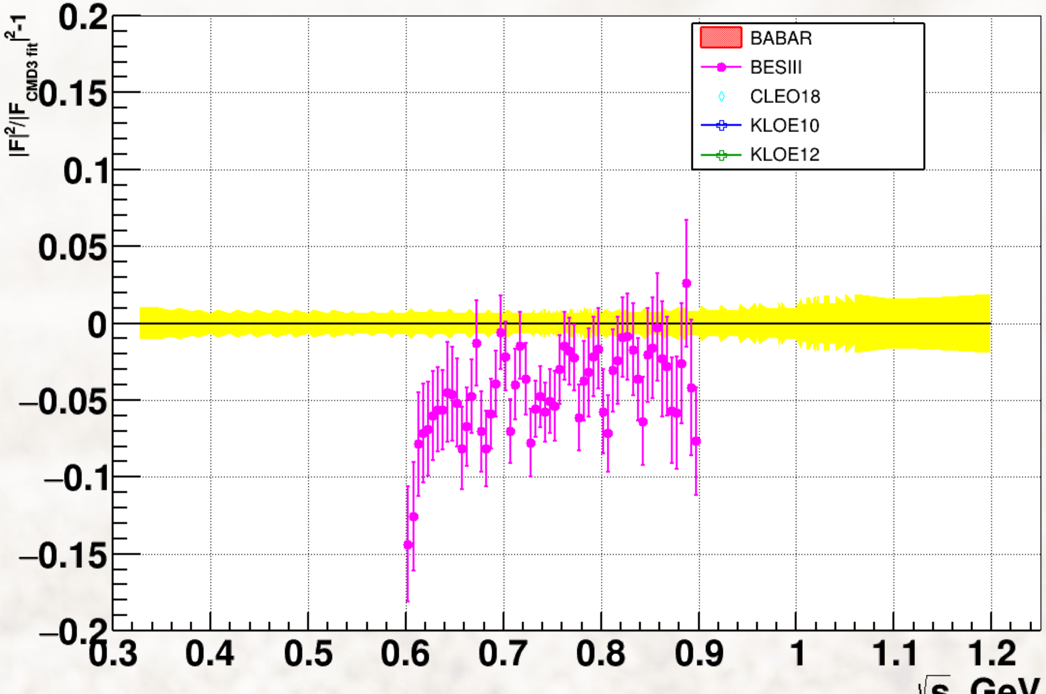
KLOE



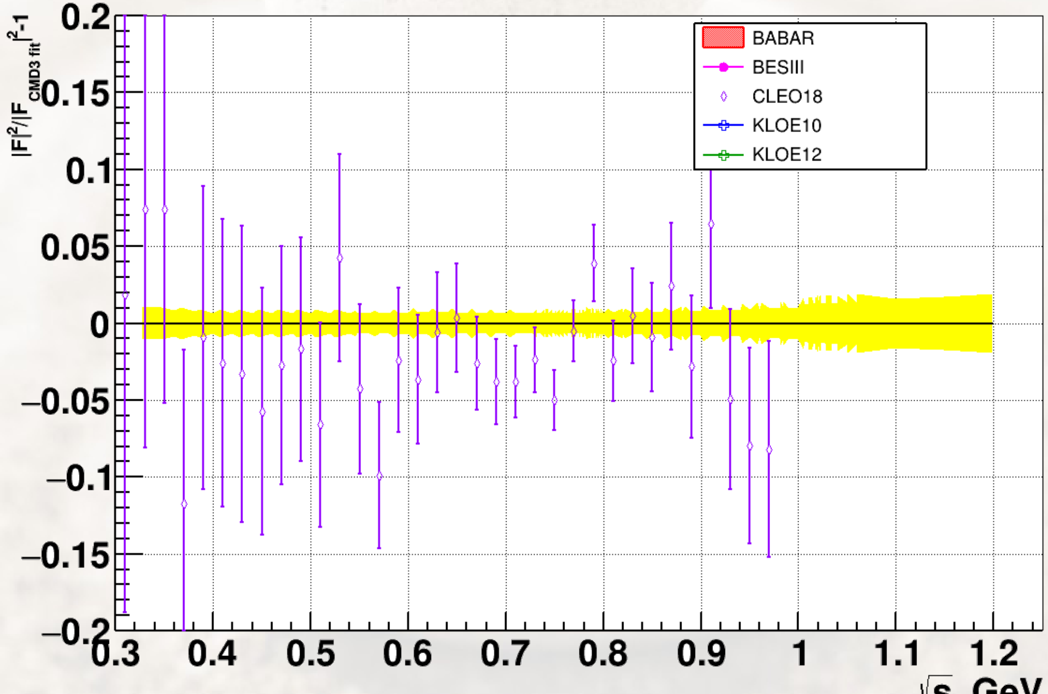
Question 44

44. There is the same trend for CMD-3/BABAR or /CMD2 or /SND: CMD-3 excess of up to 5% around 0.7 ± 0.1 GeV (left side of ρ peak), excess extending to the highest energies for CMD-3/KLOE. But hard to distinguish the different contributions in Figs. 34-35. Plot separately CMD-3 fit/all other experiments to better assess the discrepancies.

BES

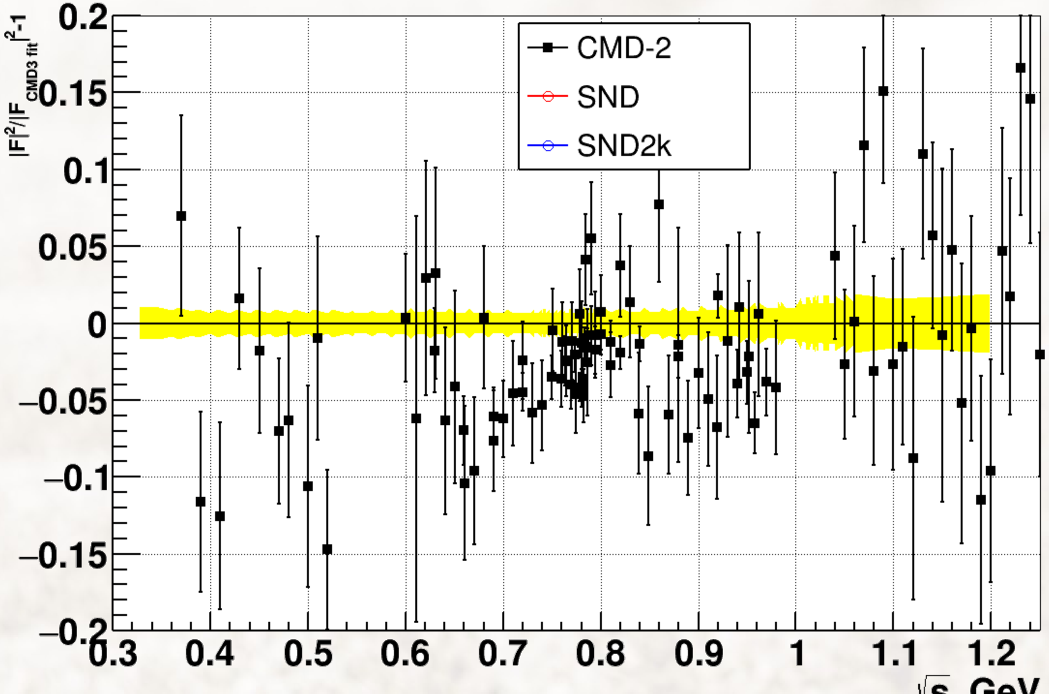


CLEO

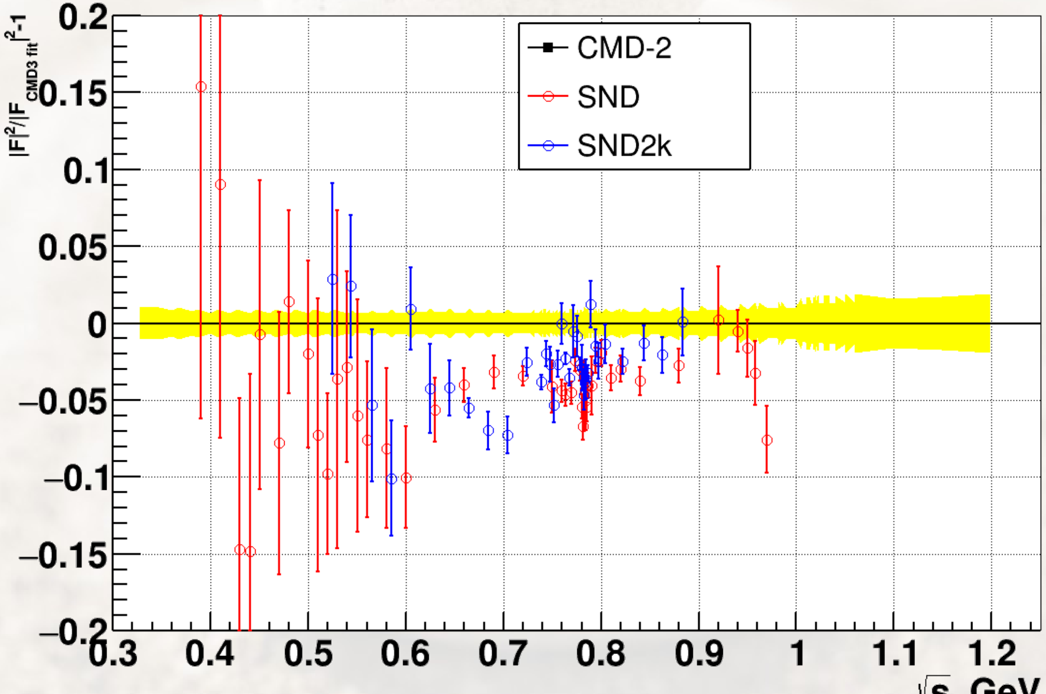


44. There is the same trend for CMD-3/BABAR or /CMD2 or /SND: CMD-3 excess of up to 5% around 0.7 ± 0.1 GeV (left side of ρ peak), excess extending to the highest energies for CMD-3/KLOE. But hard to distinguish the different contributions in Figs. 34-35. Plot separately CMD-3 fit/all other experiments to better assess the discrepancies.

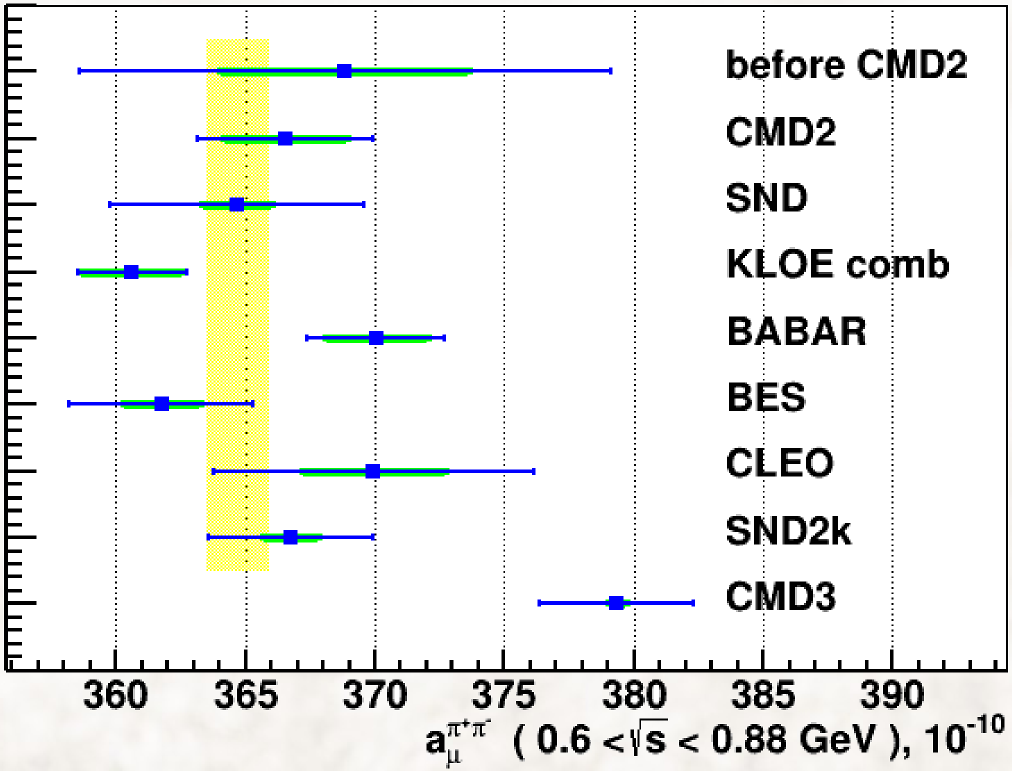
CMD-2



SND, SND@VEPP-2000



45. Comment on the fact that accuracy for a_μ[0.6-0.88] is similar for CMD-2 and CMD-3 despite much larger data set for the latter.



	$a_\mu^{\pi\pi,LO}$, 10^{-10}
before CMD2	368.8 ± 10.3
CMD2	366.5 ± 3.4
SND	364.7 ± 4.9
KLOE	360.6 ± 2.1
BABAR	370.1 ± 2.7
BES	361.8 ± 3.6
CLEO	370.0 ± 6.2
SND2k	366.7 ± 3.2
CMD3	379.3 ± 3.0

RHO2013	380.06 ± 0.61 ± 3.64
RHO2018	379.30 ± 0.33 ± 2.62 $\times 10^{-10}$
Sum	379.35 ± 0.30 ± 2.95

Systematic uncertainty 0.7% (RHO2018)
0.9% (RHO2013)

CMD-3 have conservative systematic uncertainties,
They are averaged between seasons in a_μ integral,
The systematic error is totally dominated over statistics now.

Question 46 (from short list)

The paper cannot avoid a study and a discussion concerning the CMD-2/CMD-3 strong discrepancy which are absent at the moment, despite similar detectors, analysis and group: outline the major differences in the detector and the analysis procedure, compare distributions, dig out where the problem occurs. seen for the Bhabha mode?

We don't know at the moment the source of difference between experiments.
In principal CMD-2/CMD-3 detectors are totally different:
CMD-3 allows to study systematics at higher statistical level.
New Drift Chamber, new LXe calorimeter(with tracking capabilities),
new electronics, new implementation of trigger system, ….
Peoples involved in analysis at ρ-peak are different (except exchanged experiences)

Question 47

47. More generally can an effort be made between CMD-3 and SND groups (the 2 scan experiments running at BINP) to understand their discrepancy? Maybe the institute can help to straighten out the embarrassing situation?

In fact they are two totally two different experiments, different data, different analysis….

Yes, we can profit that we still both running experiments, some future analysis can be performed by looking on specific components more carefully during data taking.
Published SND result is based only at this moment on ~1/10 of available statistics, more dedicated analysis of systematic sources can be performed on more precise level on the full statistic.

And indeed some of effects are common, and can be probably excluded from suspicion, for example, both experiments used the MCGPJ generator for the $\pi\pi$ radiative corrections.

Of course, the institute understand the situation, and hopefully some effort to strengthen detectors performance will be performed for future data taking.

Question 48 (from short list)

The central values of the K+K-, π+π-, ancillary 3π measurements all tend to be higher than other experiments at a similar level of 4%, which of course for the 2π channel looks most spectacular. Have possible common systematic effects across channels been investigated?

3π process is well consistent with others experiment (except CMD-2)

The common excesses in K+K- and π+π- to others experiment are seen, it could be correlated or could be not….

Possible common sources:

✗ Detector related:

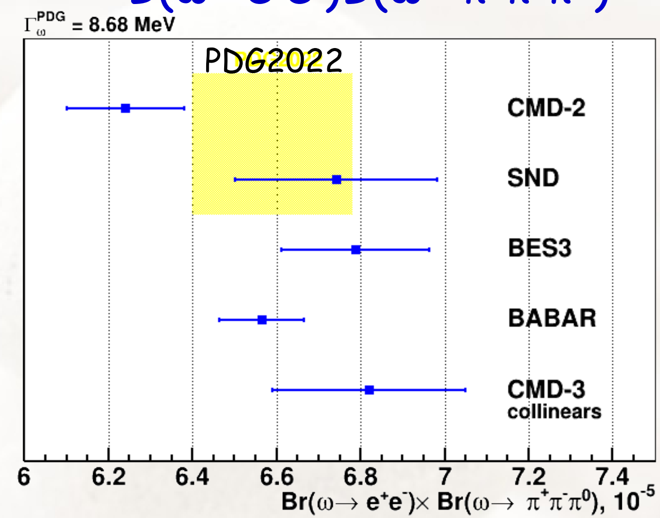
e+e- trigger efficiencies, tracker efficiencies, …. :

- ✗ not seen in Nμμ/Nee ratio
- ✗ effort to catch triggers TF vs CF correlations was performed
- ✗ not seen problems in angle distribution (if some resolution effect unaccounted…)

for future scans: new trigger system under commissioning, new DCH, ZC under consideration

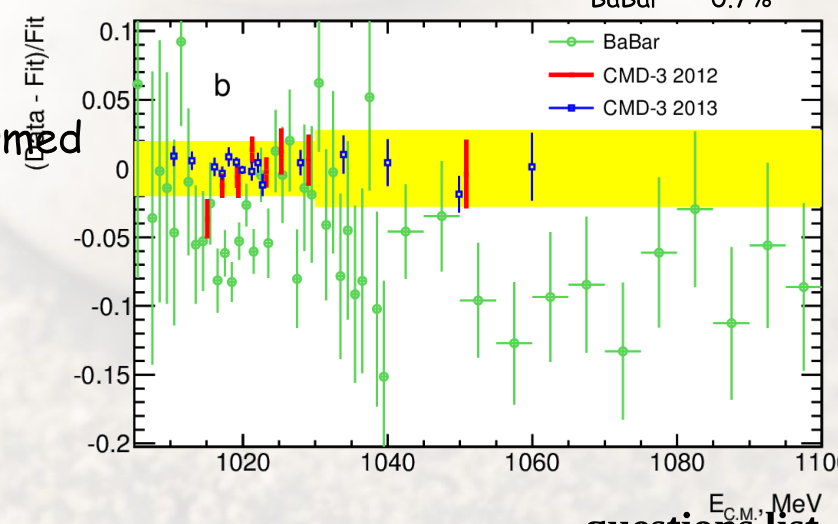
✗ Radiative correction for K+K-/π+π- from MCGPJ generator: discussed in previous slides

$B(\omega\to e^+e^-)B(\omega\to\pi^+\pi^-\pi^0)$



e+e- → K+K-

Systematics at φ
CMD-3 2%
BaBar 0.7%



Question 49 (from short list)

What are the plans for publishing this analysis: short/long papers?
Do you intend to perform additional checks before submitting to a journal?

Analysis is finished.

(in fact, analysis was finished about a year ago, since then it was form factor fitting, polishing, paper preparation, internal paper reviewing, ….)

many self consistency checks were already performed, further may be with a better detector

Current plans:

short paper is under preparation, final editing of the long paper was finished.
Still we plan to submit both versions to journals at same time

Future plans, other papers:

New ρ scans with improved detector and possibly some specific systematics checks are expected
Analysis at $\sqrt{s} > 1$ GeV is in progress by another person
(exploiting full shower profile information by neural network, as better separation is required at higher energies)
with same independent steps for efficiency determination, etc for formfactor evaluation
→ cross check between current and new analyses will be required at final stage

Question №50?