# PWG review:
# Measurements of Kaon Anti-flow in the High Baryon Density Region from Au + Au Collisions at √sNN = 3 - 3.9 GeV

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**2024/4/15**

# Suggestions

**Analysis Note:**

Major:
-- As seen in Fig 59 and 60, the kaon-minus v1 have a discontinuity at around y_cm ~ -0.7. It is also part of your main figure in paper. What I remember you have mentioned in some presentation that there was some issue in mass-square in eToF. Was it related and can you please remind me again how it was solved/addressed?

-- page-46: Fig. 65: The proton v1 has a bump structure at around ycm ~ -0.4. How do you understand it? Do you see a same structure for lambda?

-- page-11: You should have purity numbers for all your PIDs, please add them in your note. Note that there was a recent presentation from Cameron Racz on the effect of proton purity on v3. Can this affect your analysis?

-- page-24: Fig. 24: Can you add a few more example figures for your v1 versus invariant mass for kshort and lambda, may be in the Appendix section of AN? Please also add discussion how is v1 the background shape is considered. Have you varied v1^background to get systematic on v1 for kshort and lambda?

-- As seen in Fig 59 and 60, the kaon-minus v1 have a discontinuity at around y_cm ~ -0.7. It is also part of your main figure in paper. What I remember you have mentioned in some presentation that there was some issue in mass-square in eToF. Was it related and can you please remind me again how it was solved/addressed?

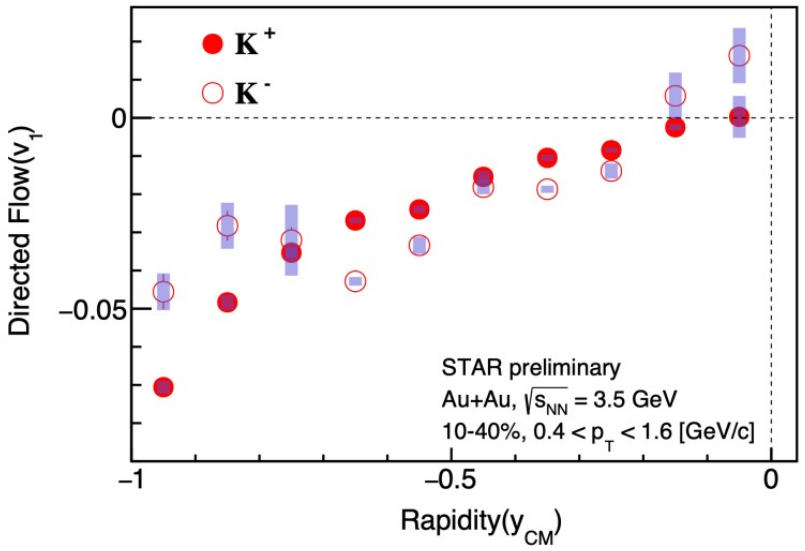
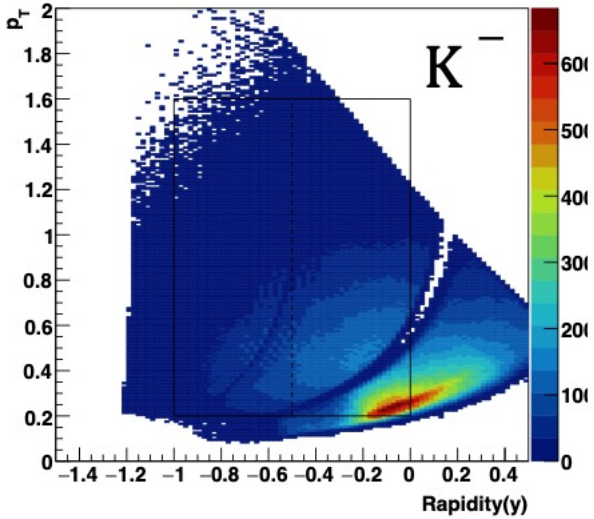



FIG. 60. $v_1$ of kaons as function of rapidity at $\sqrt{s_{NN}} = 3.5$ GeV.




The eTOF issue is solved with a additional cut to reduce mismatches between TPC and eTOF. Please look at slides in the backup.

I think the discontinuity is not related with eTOF.
As we can see in the acceptance plot, y [-1, -0.5] is covered by BTOF.

The K- v1 going up in the forward rapidity is also observed at published 3 GeV and 3.2, 3.9 GeV. It might be the common behavior.

-- page-46: Fig. 65: The proton v1 has a bump structure at around ycm ~ -0.4. How do you understand it? Do you see a same structure for lambda?

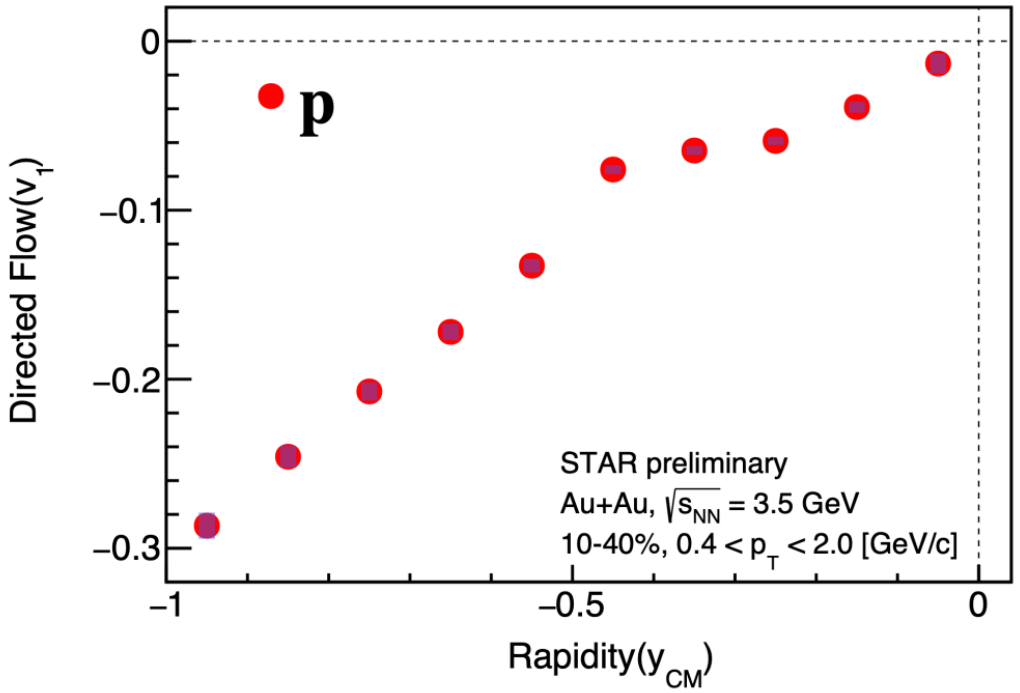
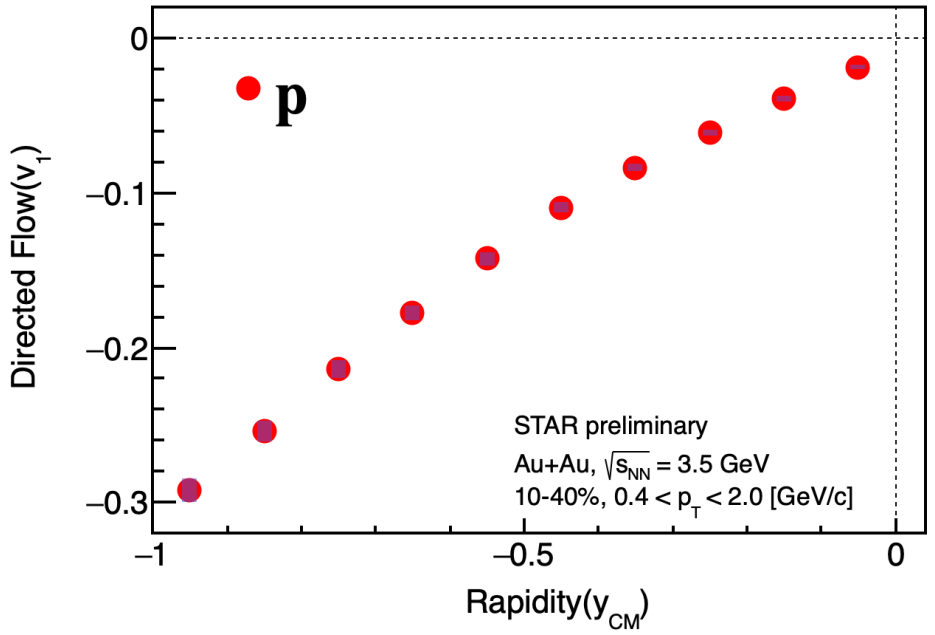



FIG. 65. $v_1$ of proton as function of rapidity at $\sqrt{s_{NN}} = 3.5$ GeV.

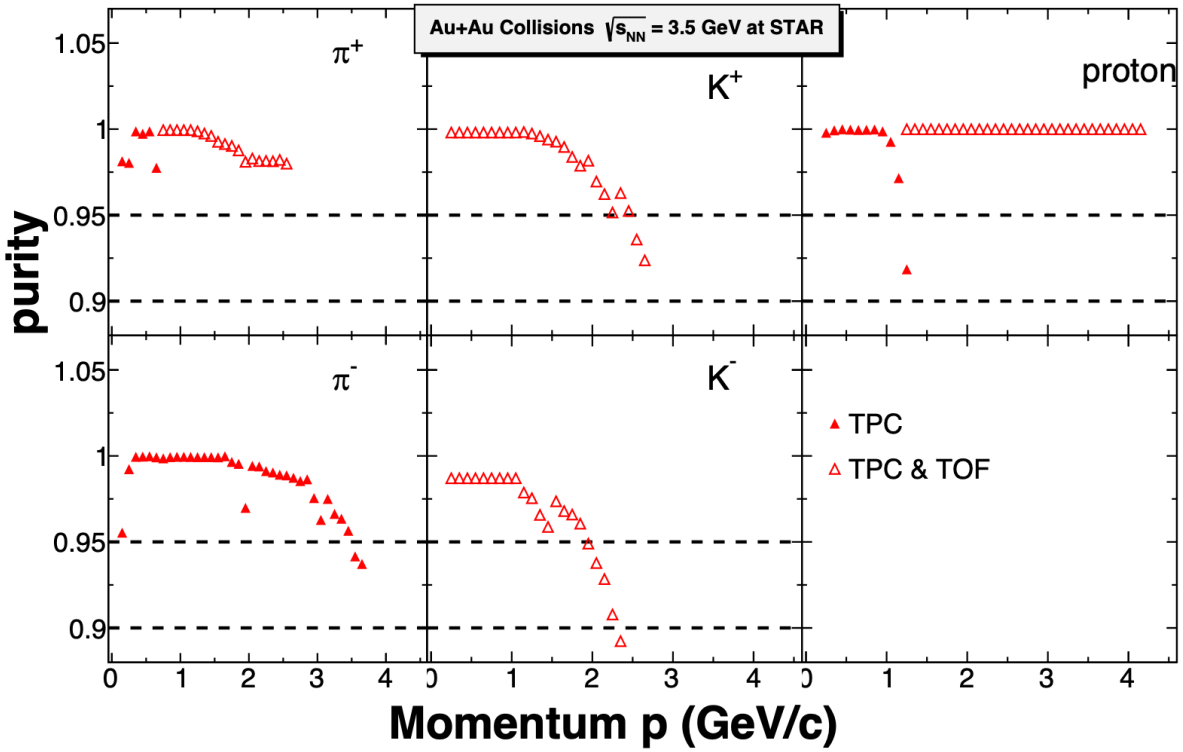
NEW




Sorry for the misleading, cause I write the note for a long time. Fig. 65 is old plot I didn't update.

The bump issue is solved. It is caused by efficiency correction.
I used to apply TPC+TOF efficiency within pT[0.4, 2.0], it's not right since we only use TPC at low pT(pT < 1.2).
I corrected it applying TPC efficiency only at low pT, and the bump is gone.

-- page-11: You should have purity numbers for all your PIDs, please add them in your note. Note that there was a recent presentation from Cameron Racz on the effect of proton purity on v3. Can this affect your analysis?




Yes, we have, already updated it.

For Cameron's study, is this one?
https://drupal.star.bnl.gov/STAR/system/files/cracz_FCV_240410.pdf
If I understand correctly, the conclusion he got is: purity effect on proton v3 is arising from the statistics. And we also use p-dependence nSigma cut to improve statistics like he did.
Another conclusion he got is: we should stay with the larger statistic production P23id which is without eTOF. While in v1 analysis, we have to use eTOF corresponding to the less statistic production P23ie, cause we need to eTOF to cover the mid-rapidity region.

-- page-24: Fig. 24: Can you add a few more example figures for your v1 versus invariant mass for kshort and lambda, may be in the Appendix section of AN? Please also add discussion how is v1 the background shape is considered. Have you varied v1^background to get systematic on v1 for kshort and lambda?

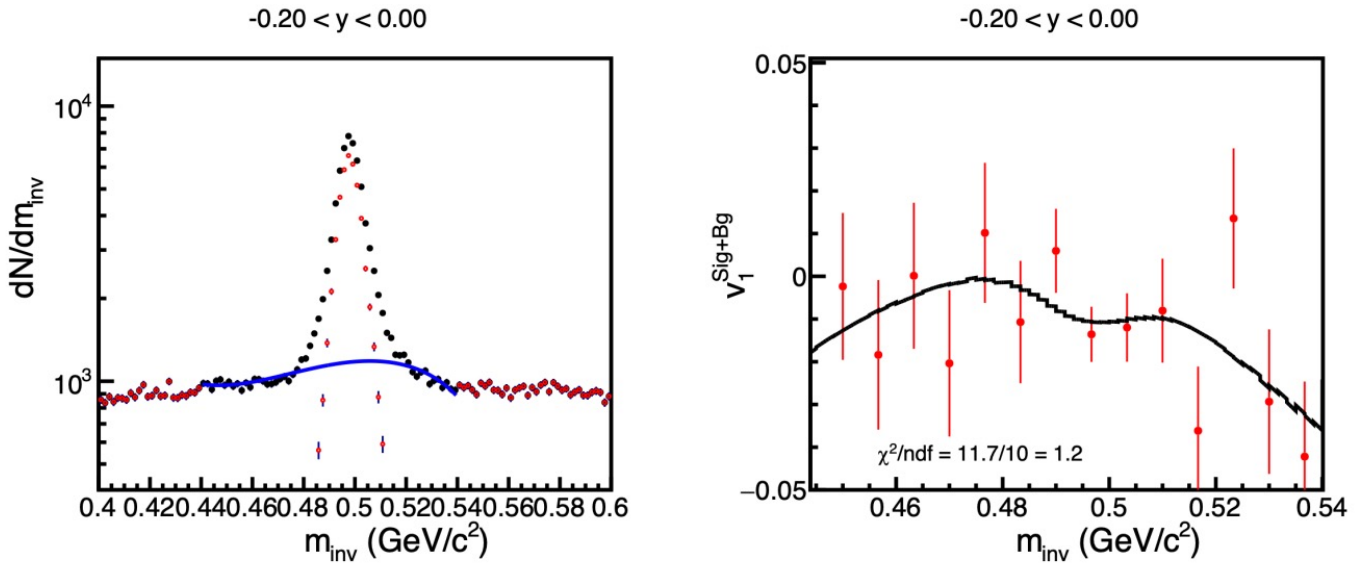



FIG. 24. Invariant mass distribution of $K_S^0$ in 10-40% centrality at $\sqrt{s_{NN}} = 3.5$ GeV.

Yes, we have updated other rapidity bin as examples in the appendix.

The v1 background is taken as the second order polynomial function.

No, we didn't vary the v1^BG as systematic source.

# Suggestions

**Paper draft:**

Major comments:
-- If I understand correctly, the main selling point of this paper is the observation of anti-flow for pions and kaons at low pT. This manuscript do not rule out the kaon potential causing anti-flow for kaons, but concluded that it could also be driven by nuclear shadowing.

abstract: If it is a first time simultaneous observation of anti-flow of pions/kaons we could probably stress explicitly in the abstract in a data driven way (what you did around #l 188-190) and then mention it is _supported_ by JAM.
l#16: It is concluded --> It is supported, since you haven't ruled out kaon potential picture.
Is there a way in JAM to demonstrate the sign change in pions/kaons _solely_ from nuclear shadowing, so that you can rule out one picture. That might make this paper sending a stronger message.

-- Compared to our 3 GeV PLB paper (2108.00908), what information it can bring in terms of baryonic mean-field. Because now we have p/lambda v1 for four different beam energies. It could be added in the conclusion, eg what compressibility values are used.

-- Please double check the Kaon data points at 3.5 GeV as I mentioned above.

# Suggestions

-- I remember there was some discrepancies in two versions of JAM. Which
version are you using? And it should be pointed out in the manuscript if
results from JAM (version dependent) changed compared to our previous
publication.

-- You would need a few supplemental figures to discuss on the event plane and its resolutions.

-- You could also think about reporting v1 versus pt in your supplemental materials.

-- If I understand correctly, the main selling point of this paper is the observation of anti-flow for pions and kaons at low pT. This manuscript do not rule out the kaon potential causing anti-flow for kaons, but concluded that it could also be driven by nuclear shadowing.

Yes, this work can not deny the possible kaon potential contribution to anti-flow.
We want to emphasis spectator shadowing effect on anti-flow. And we concluded that kaon potential is not unique to kaon anti-flow.

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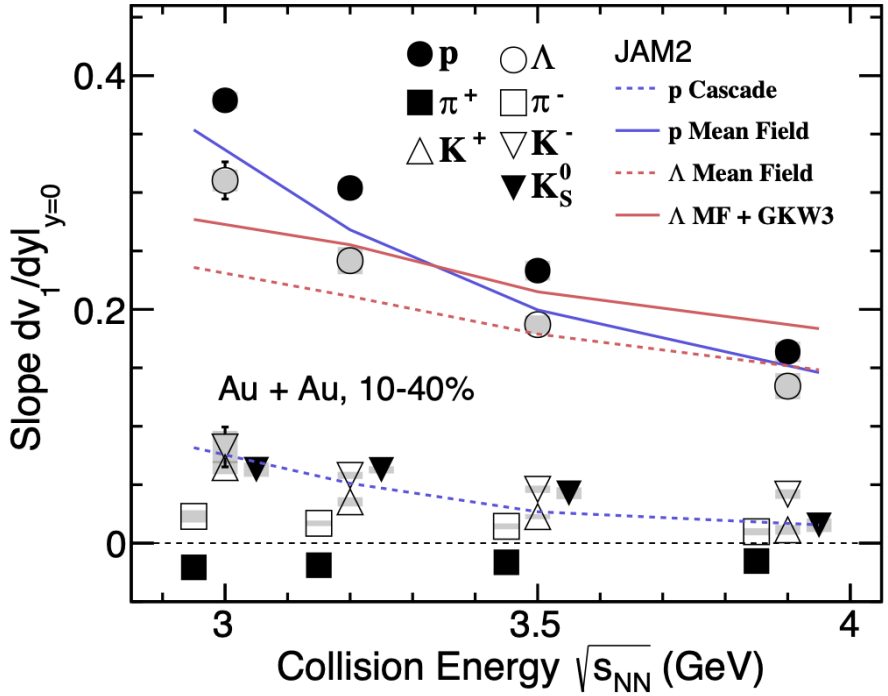
l#16: It is concluded --> It is supported, since you haven't ruled out kaon potential picture.
Is there a way in JAM to demonstrate the sign change in pions/kaons _solely_ from nuclear shadowing, so that you can rule out one picture.
That might make this paper sending a stronger message.

It’s not the first time to observe kaon anti-flow at low pT, because the K0s anti-flow observed by E895 is also at low pT (pT < 0.7).
I think the JAM can not to do so. The sigh change is not only related with shadowing effect, but also tilted expansion etc. And JAM can not describe dv1/dy vs. pT very well, there might be some room for kaon potential.

Other suggestions are implemented to the abstract.

-- Compared to our 3 GeV PLB paper (2108.00908), what information it can bring in terms of baryonic mean-field. Because now we have p/lambda v1
for four different beam energies. It could be added in the conclusion,
eg what compressibility values are used.




The nucleon incompressibility we used is kappa = 210 MeV (soft EoS).
The new thing we can discuss might be the lambda potential in the high baryon density region.
As we can see in the plot above, red solid line (MF + lambda potential) can describe lambda v1 better than the red dashed line(MF only) at 3 and 3.2 GeV.
But we PAs think lambda potential is not mature in model calculation, it would distract attention of people from kaon anti-flow.

-- I remember there was some discrepancies in two versions of JAM. Which version are you using? And it should be pointed out in the manuscript if results from JAM (version dependent) changed compared to our previous publication.

JAM version 2 we used in this work.
Cascade mod in JAM 1 and 2, they should be consistent.

The mean-field mode in JAM 2 we used is RQMDv/MS2,
Which is not include in JAM 1.
We have pointed it out in the caption of Fig. 2.

-- You would need a few supplemental figures to discuss on the event plane and its resolutions.

-- You could also think about reporting v1 versus pt in your supplemental materials.

In the paper or analysis note?

If in the paper, it's limited to add the 5$^{th}$ figure.
We have include event plane and resolution figures in the analysis note.
I have updated v1 vs pT in the analysis note.

# Suggestions

**Minor suggestions:**

-- Please check notations, sometimes you use "protons" some times "p".
For instance l#77

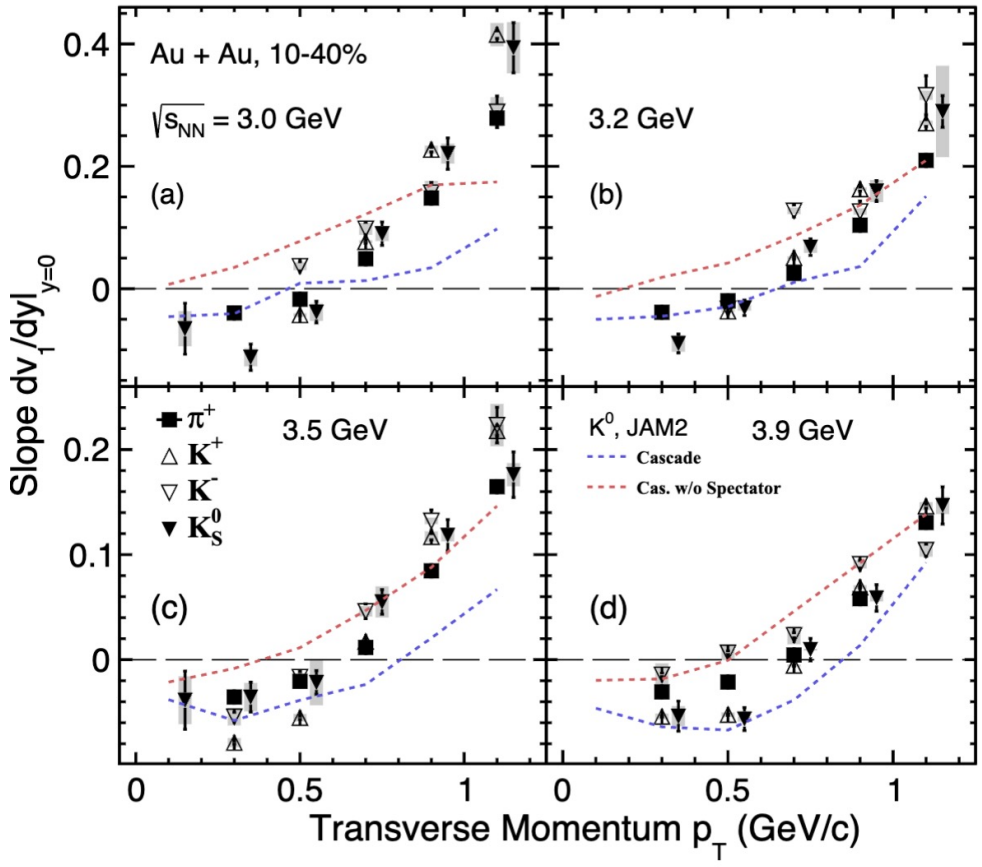
-- Fig. 4: You should mention rapidity coverages inside the figures,
although you are presenting dv1/dy
You could draw pi+ as markers, not to confuse with models. You can play
with color coding.

-- l#196: w/O spectator, have you turned off spectator interactions?
Sorry for my ignorance, but I wanted to understand how it was done in JAM.

-- Please check notations, sometimes you use "protons" some times "p".
For instance l#77

The suggestion is implemented in the paper draft.
The notation is unified with “protons”.

-- Fig. 4: You should mention rapidity coverages inside the figures, although you are presenting dv1/dy
You could draw pi+ as markers, not to confuse with models. You can play with color coding.



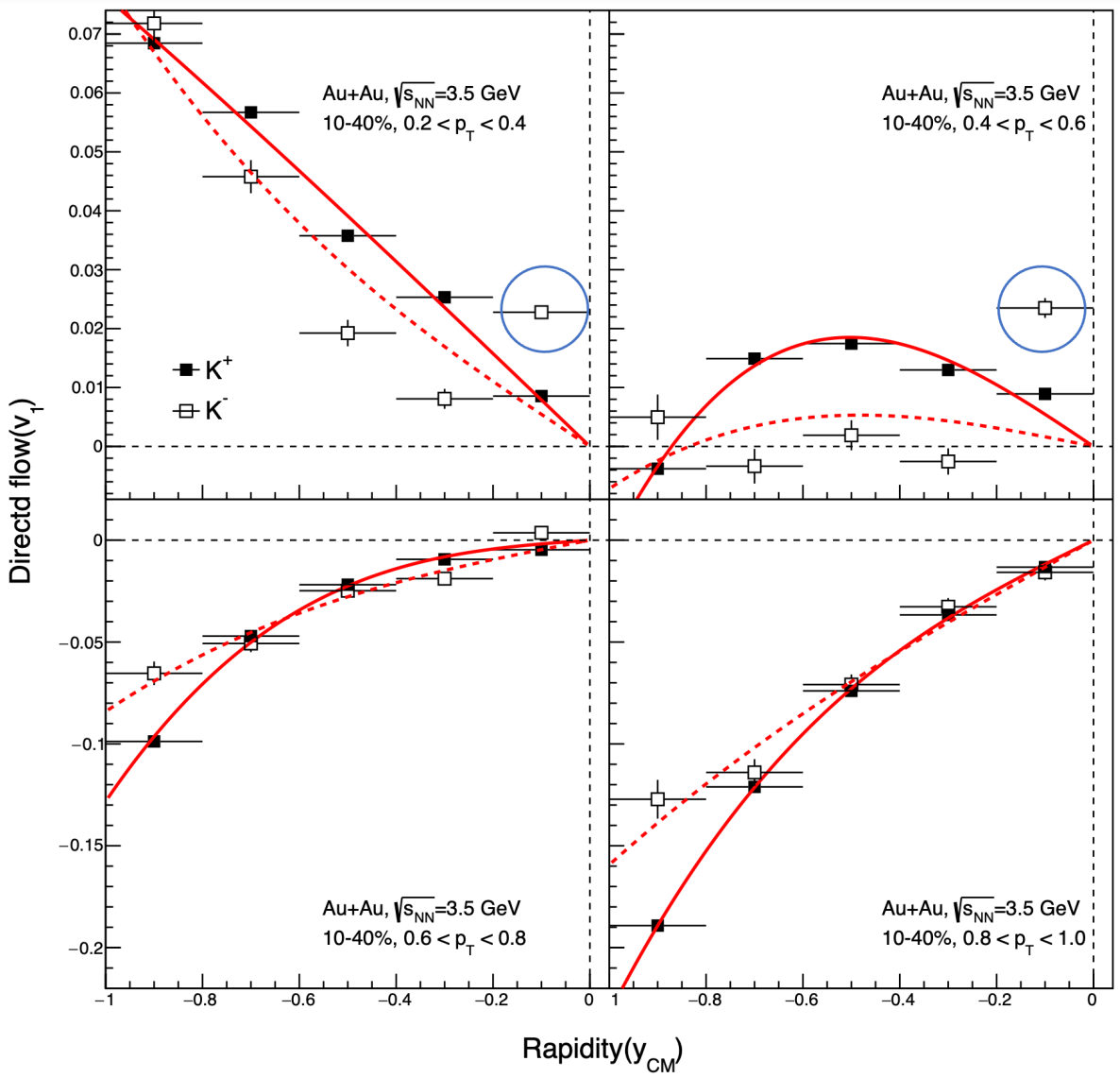

The rapidity coverages are [0.2, 0.4], [0.4, 0.6], [0.6, 0.8] …
How can we present it concisely? I have no idea so far.
We used to use maker to show pi+ v1, right top panels.
Four set of markers would gather in one plot, it's not easy to find kaon v1 what people interests. So we chose the solid black line to show pi+, it could be the "standard" line showing how strong the shadowing effect on meson v1.

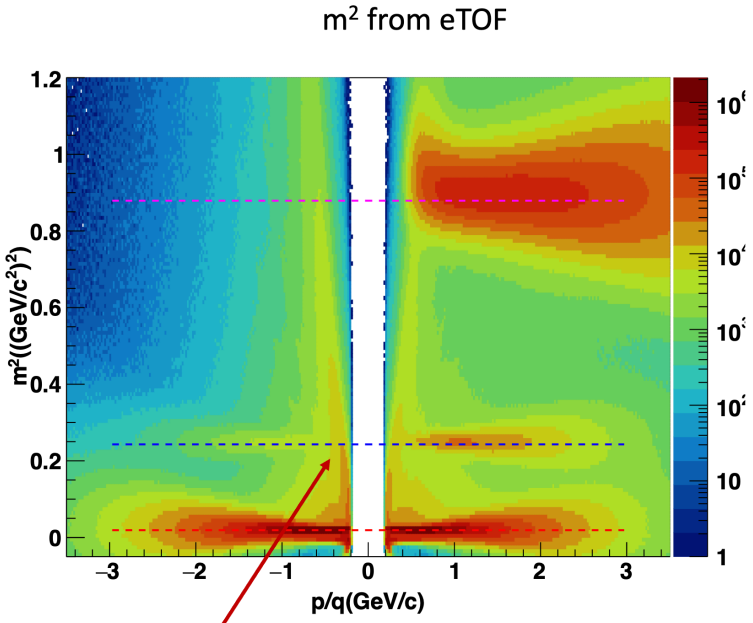
-- l#196: w/O spectator, have you turned off spectator interactions?
Sorry for my ignorance, but I wanted to understand how it was done in JAM.

Thank you, it’s a good question.
JAM provides one setup in the input file.
One just need add another command:  Cascade:removeSpectator = true
I think the spectator interactions is turned off.

# Backup

# K- Identification




eTOF expert Philipp:
“The vertical band is largely due to mismatches between TPC and eTOF.
In the production, we used very loose cuts on these distances to allow analysers to make their own choice”

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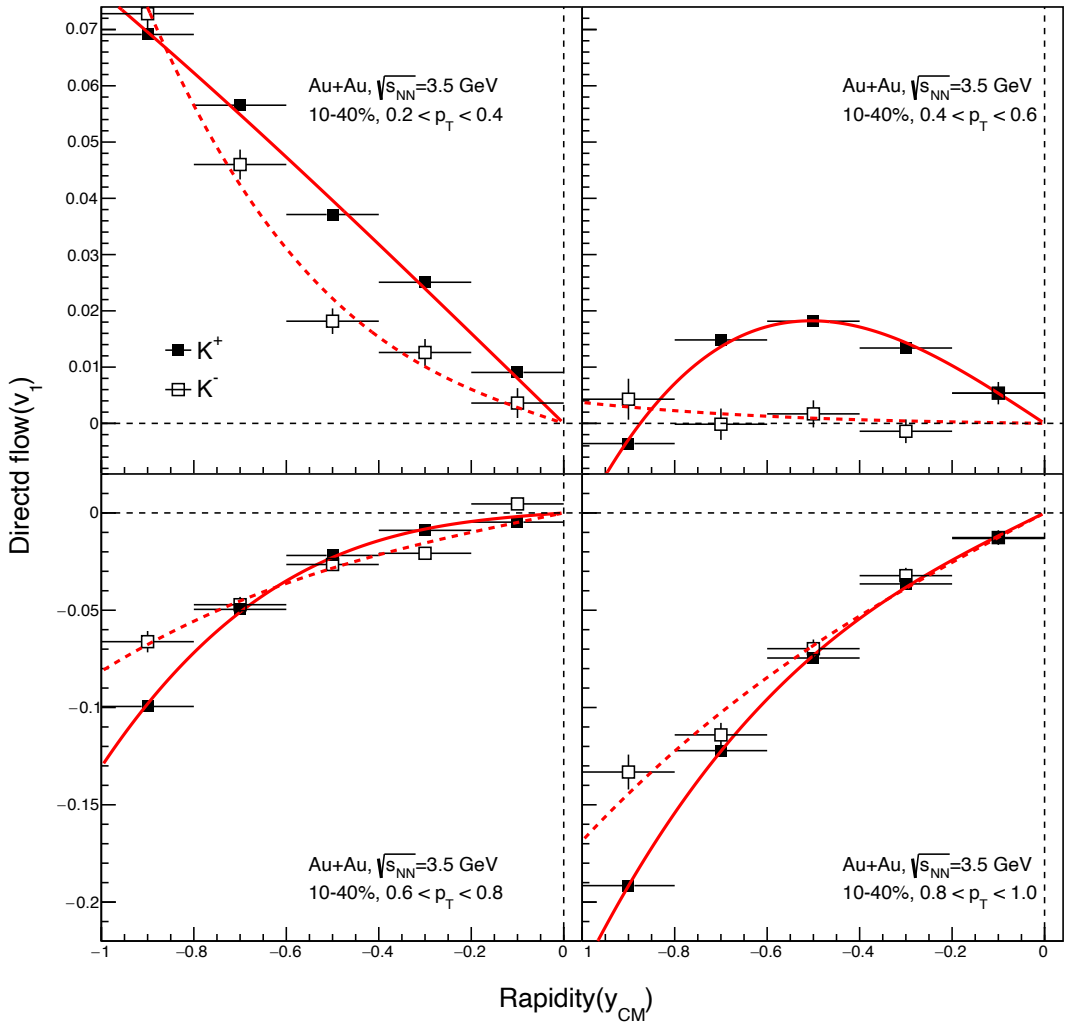


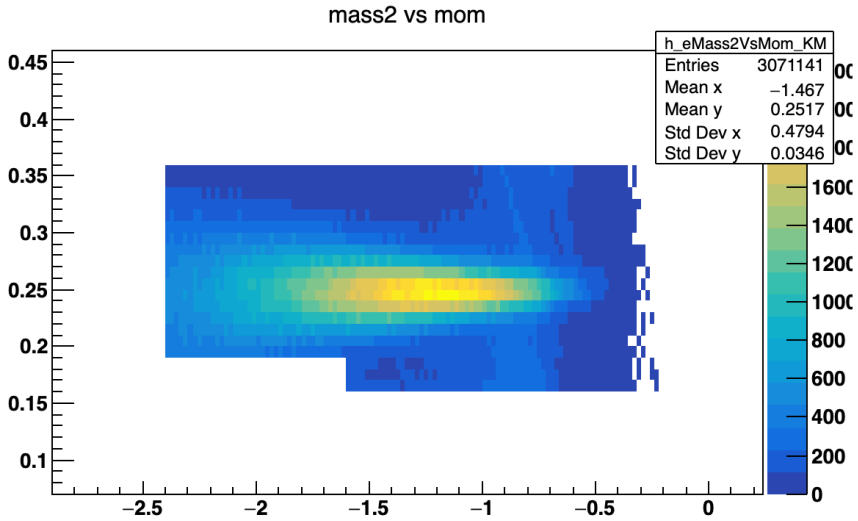


The issue could be solved with additional cut:
sqrt(deltaY^2+deltaX^2) < 1
(Where deltaX = etofPidTraits.deltaX())

After cut, the mass distribution looks clean, and v1 in the midrapidity drops.