Centrality issues in asymmetric collisions: direct photons to the rescue?

G. David
Stony Brook University
(but speaking as a private person)

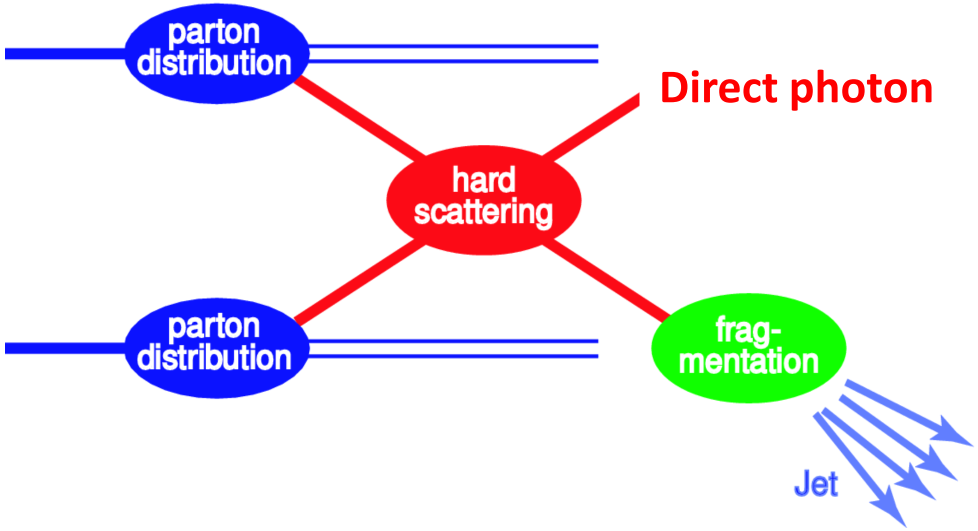
(Some words on *ideomorph[ic]* science, a.k.a.
"if you have a hammer, all problems look like a nail"
and a reasonable-looking prescription to weed it out)

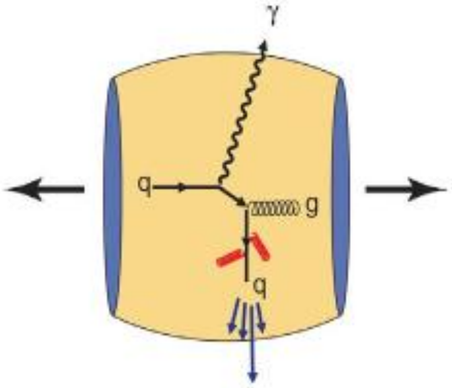
Why centrality in heavy ion collisions?

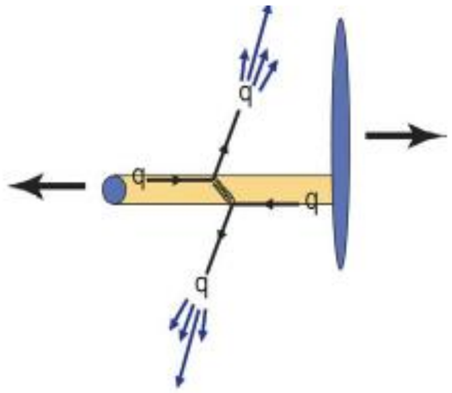
Because ignoring it would be foolish – centrality dependence hides almost all new physics we are after
For this talk I pick one the most rich and informative observable: the ***nuclear modification factor***
I'll restrict myself to η ~ 0 and observed multiplicity at high η.

$$R_{\rm AB}(p_{\rm T}) = \frac{(N^{\rm AB}_{\rm inel})^{-1}\,{\rm d}N^{\rm A+B}_{x}/{\rm d}p_{\rm T}}{\langle T_{\rm AB}\rangle_{\rm f}\,{\rm d}\sigma^{\rm pp}_{x}/{\rm d}p_{\rm T}}$$

$$R_{pA} = \frac{1}{\langle N_{\rm coll}\rangle}\,\frac{dN^{pA}_{\rm jet}/dp_{\perp}dy}{dN^{pp}_{\rm jet}/dp_{\perp}dy}$$







What does – and doesn't – change if a medium if formed in the collision?

Of course Nature doesn't tell you directly, how close the impact was – you have to guess, infer it from some observable

Collision geometry vs. event activity

Ann.Rev.Nucl.Part.Sci.57:205-243,2007
(arXiv:nucl-ex/0701025)

Geometry is *convenient* for theorists to describe nuclear density, nPDFs, bulk phenomena, path lengths, initial fluctuations… etc.

Experimentally it is *inferred* from some observable reflecting average interactions (event activity)

The usual tool to make the connection is the Glauber-model (and its extensions)

"In heavy ion collisions, we manipulate the fact that the majority of the initial state nucleon-nucleon collisions will be analogous to minimum bias p+p collisions…"

3.1 Methodology

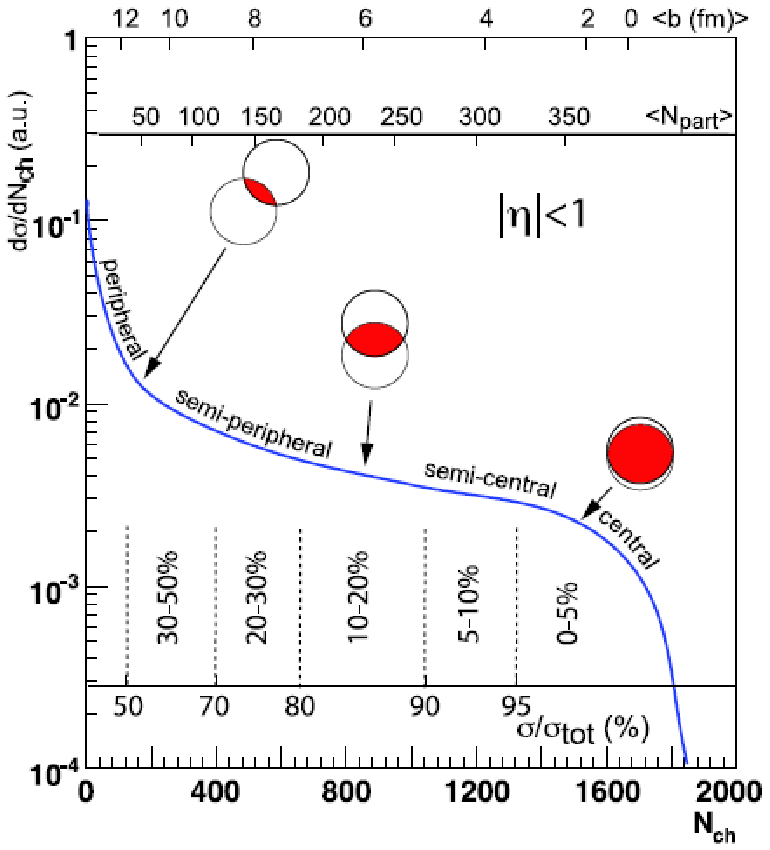


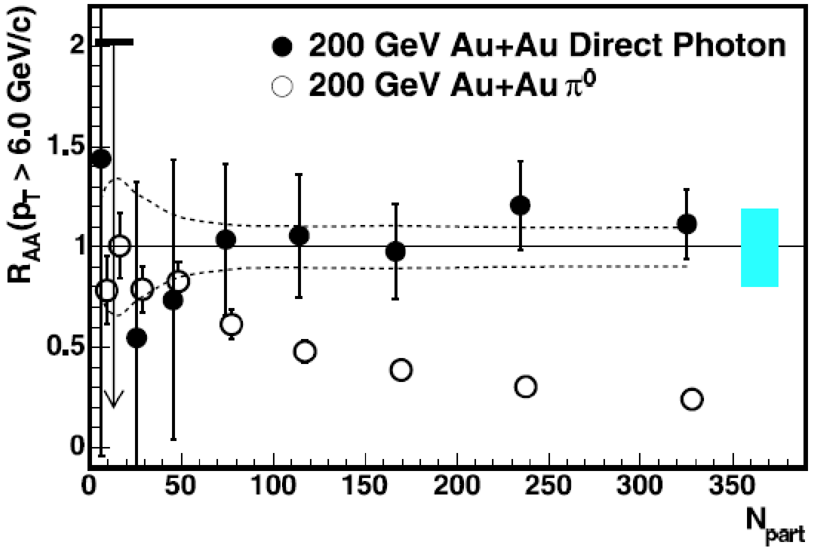
Figure 8: A cartoon example of the correlation of the final state observable N_{ch} with Glauber calculated quantities (b, N_{part}). The plotted distribution and various values are illustrative and not actual measurements (T. Ullrich, private communication).

Glauber model

Eikonal approach
Number of participants and collisions
(*what are the degrees of freedom?*)
Convolve response (e.g. multiplicity) observed for a single collision ***n** times*
Experimentally: take percentages of the observed total distribution → centrality
Works great in large-on-large collisions (A+A)

Nuclear modification factor:

$$R_{\mathrm{AB}}(p_{\mathrm{T}}) = \frac{(N_{\mathrm{inel}}^{\mathrm{AB}})^{-1}\,\mathrm{d}N_x^{\mathrm{A+B}}/\mathrm{d}p_{\mathrm{T}}}{\langle T_{\mathrm{AB}}\rangle_{\mathrm{f}}\,\mathrm{d}\sigma_x^{\mathrm{pp}}/\mathrm{d}p_{\mathrm{T}}}$$



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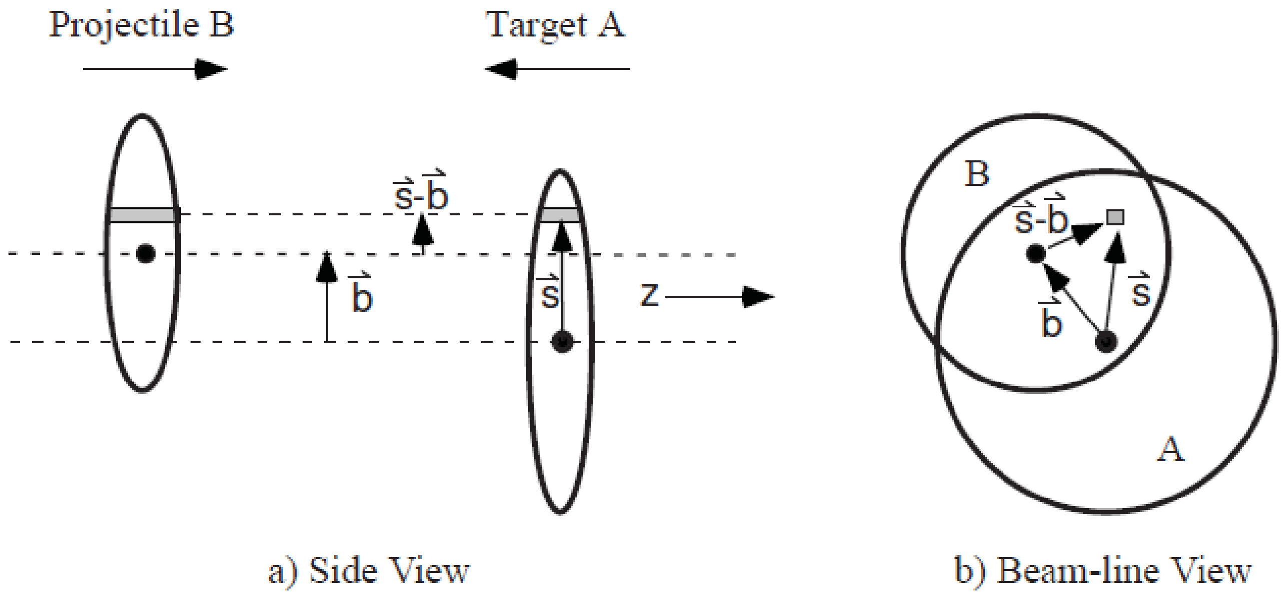
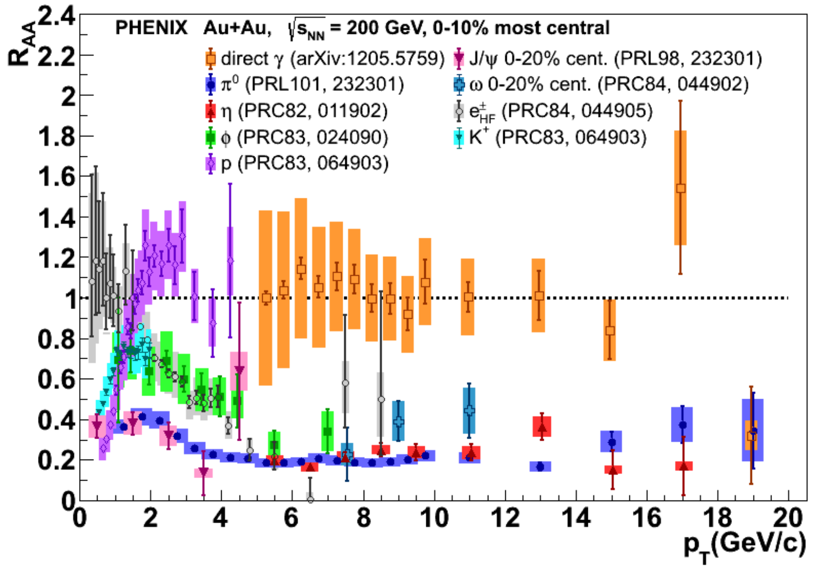


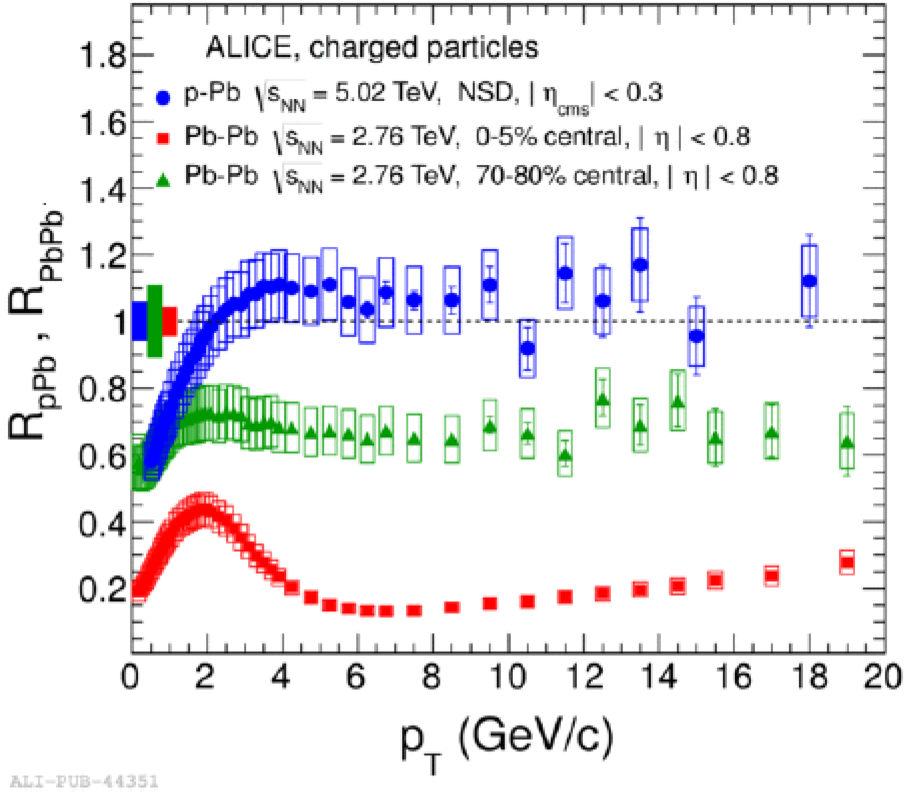
Glauber (1959): "…the approximate wave function (74) is only adequate for the treatment of small-angle scattering. It does not contain, in general, a correct estimate of the Fourier-amplitudes corresponding to large momentum transfer"

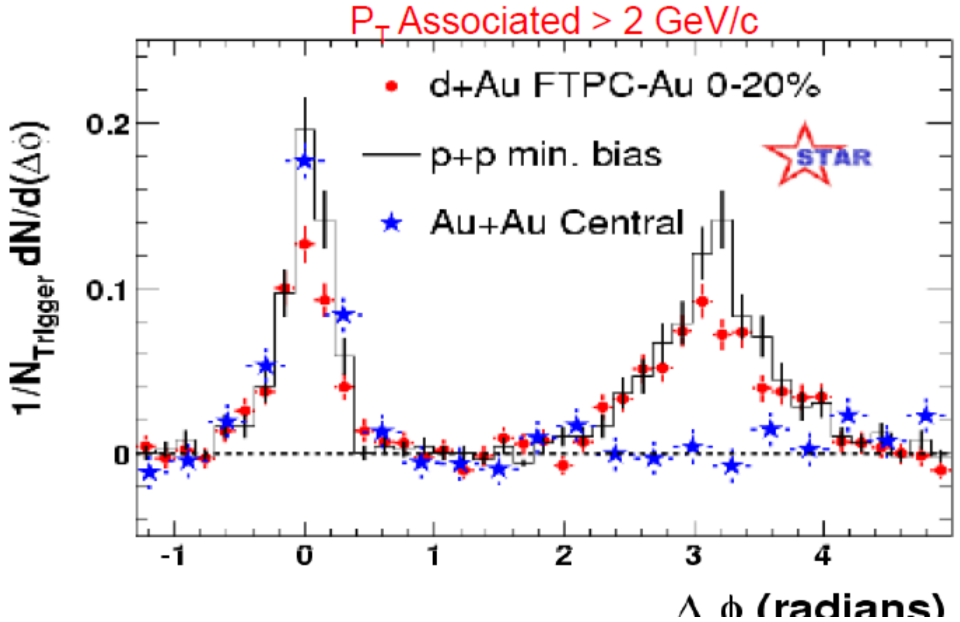
In A+A all this works like a charm…



…meaning: as expected

Hard and soft processes factorize

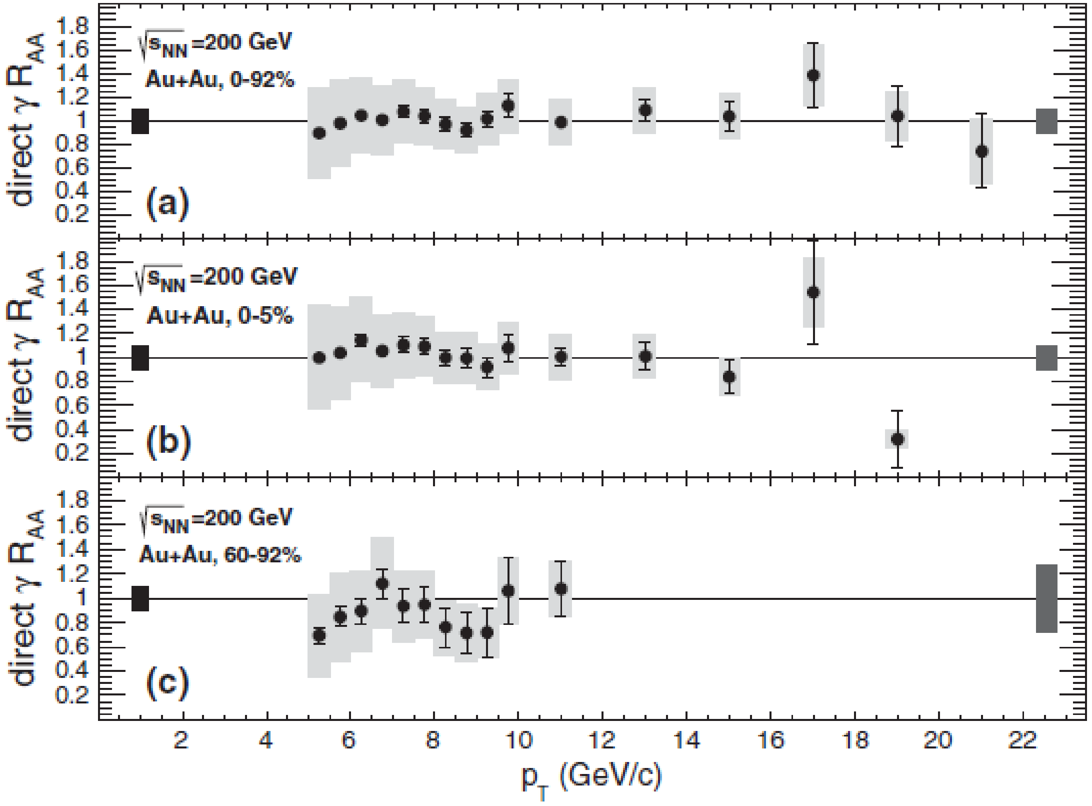




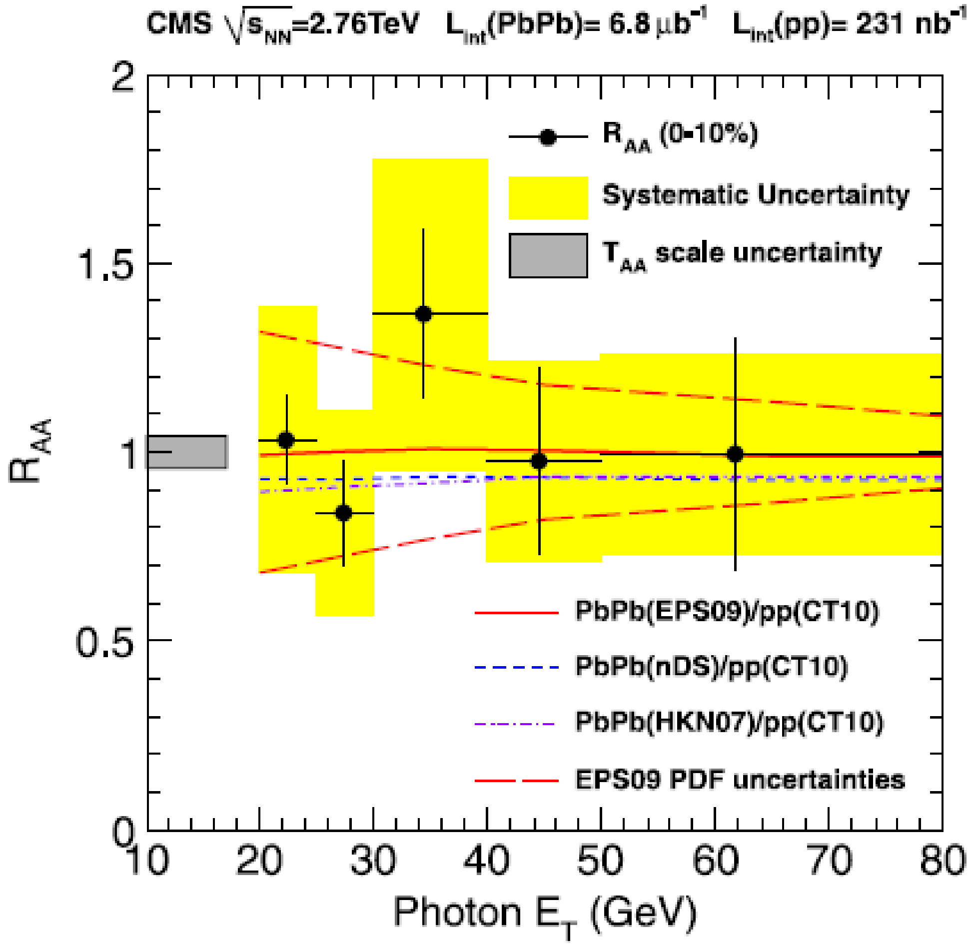
High pT (isolated) photons are immune to the medium

In A+A collisions, while hadrons are strongly suppressed, and in a p_T-dependent way, photons appear to be unaffected

PHENIX PRL 109, 152302 (2012)



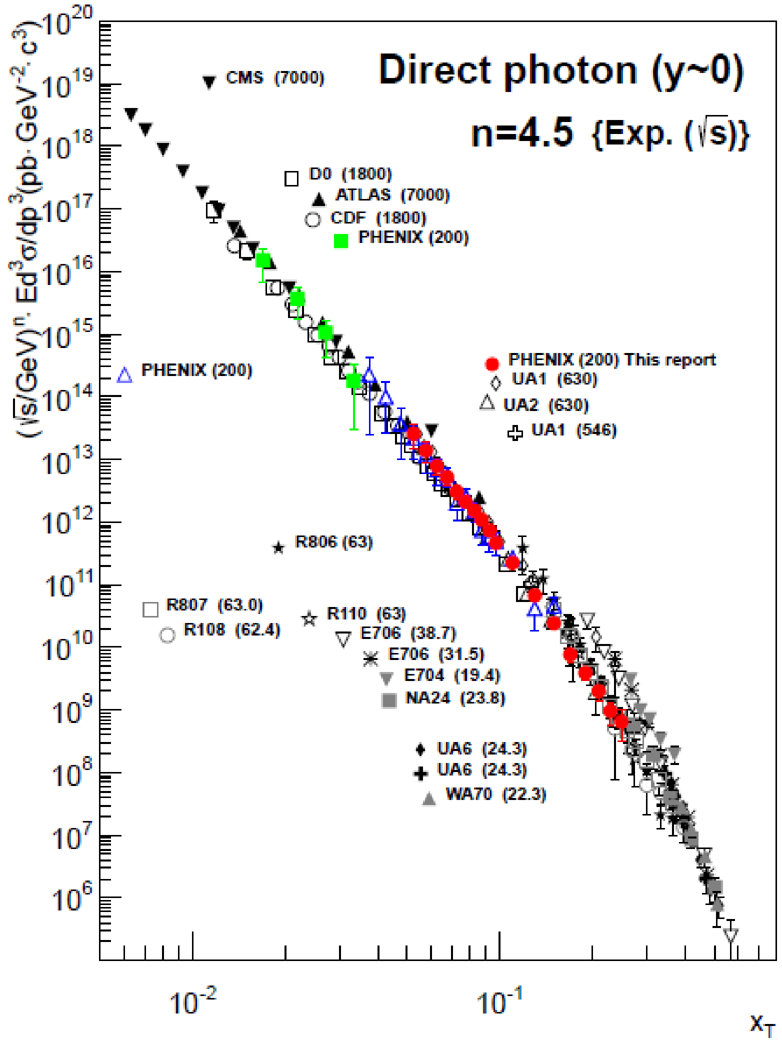
CMS PLB 710 (2012) 256
isolated photon, PbPb 0-10% centrality



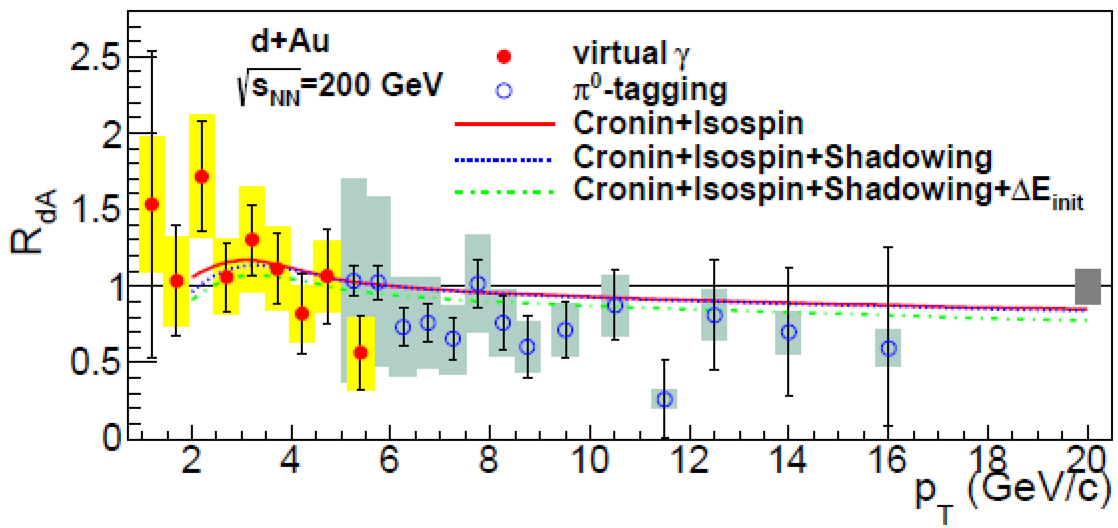
Same holds for Z, W at LHC

High pT photons are well known in p+p and unchanged even in p+A

PRD 86 072008



PRC 87, 054904 (2013)



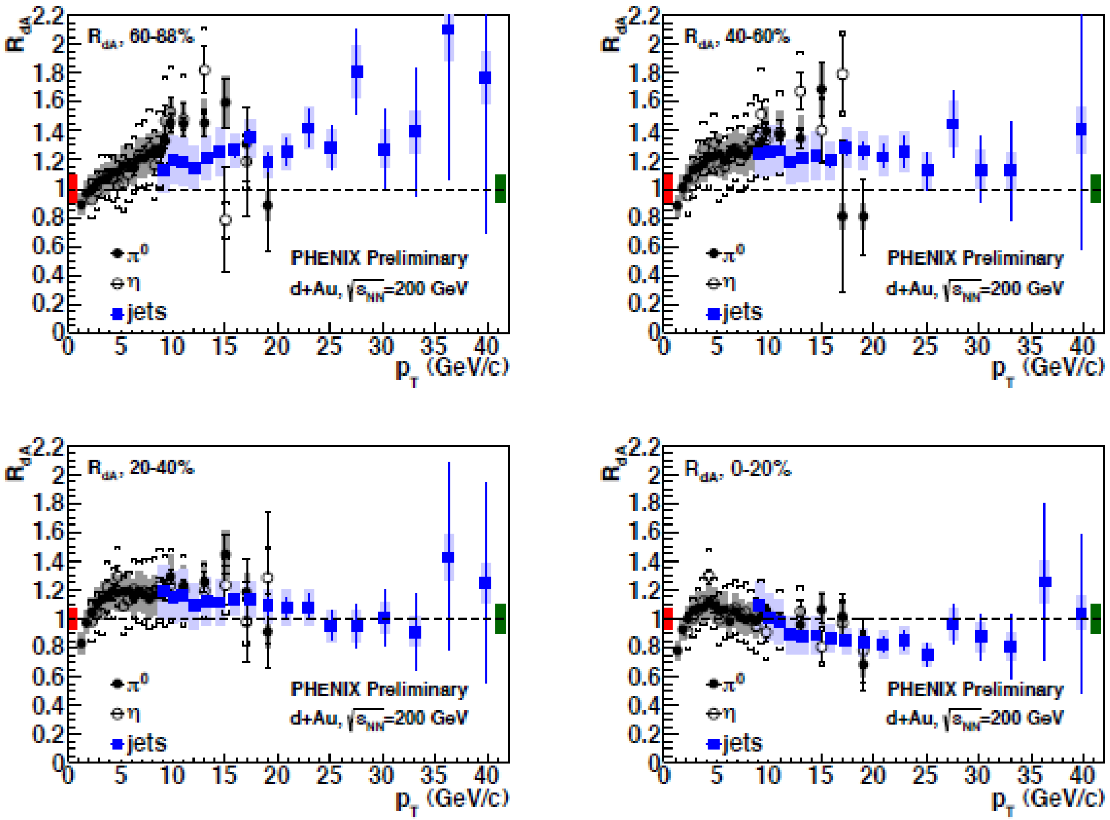
Watch out for the slight deviation from unity due to the isospin effect

All right, this is MB, but stay tuned!

Then why are we talking about this at all?

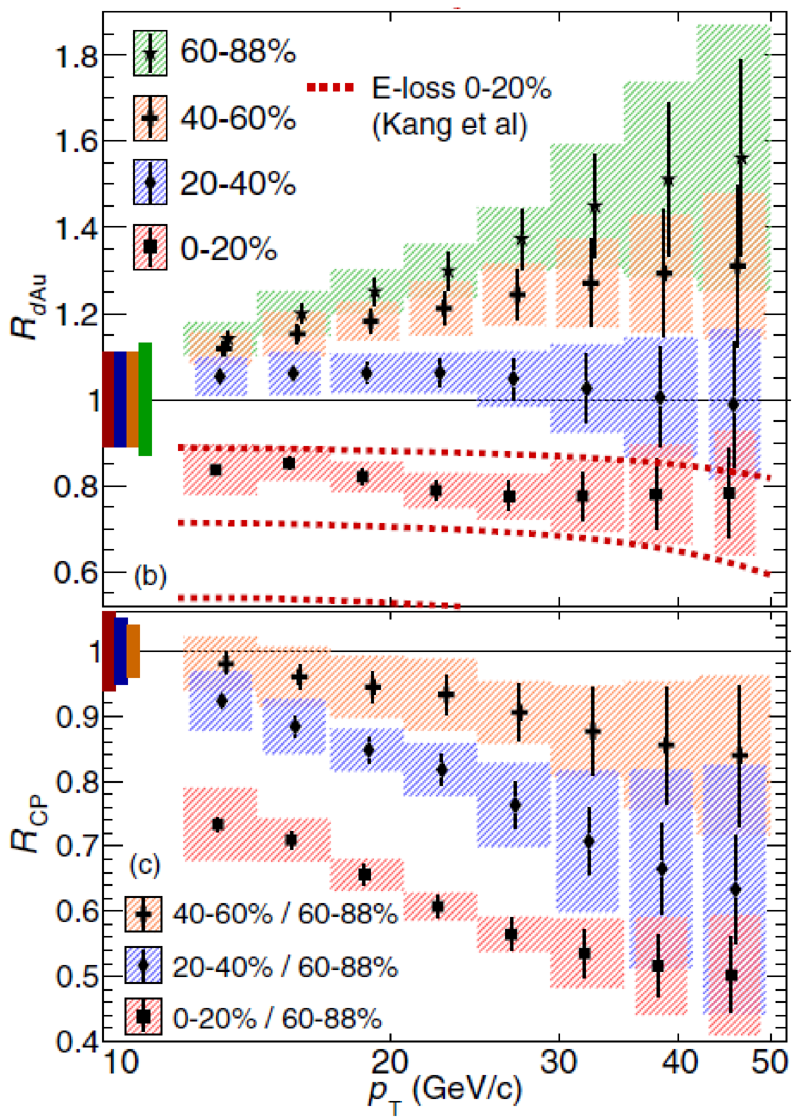
d+Au π^0, η, jets, 2012

Because of a dinner at QM’12, where 8 Hungarian theorists put me in their cross-hair: “you must be wrong”



Baldo Sahlmueller, QM’12

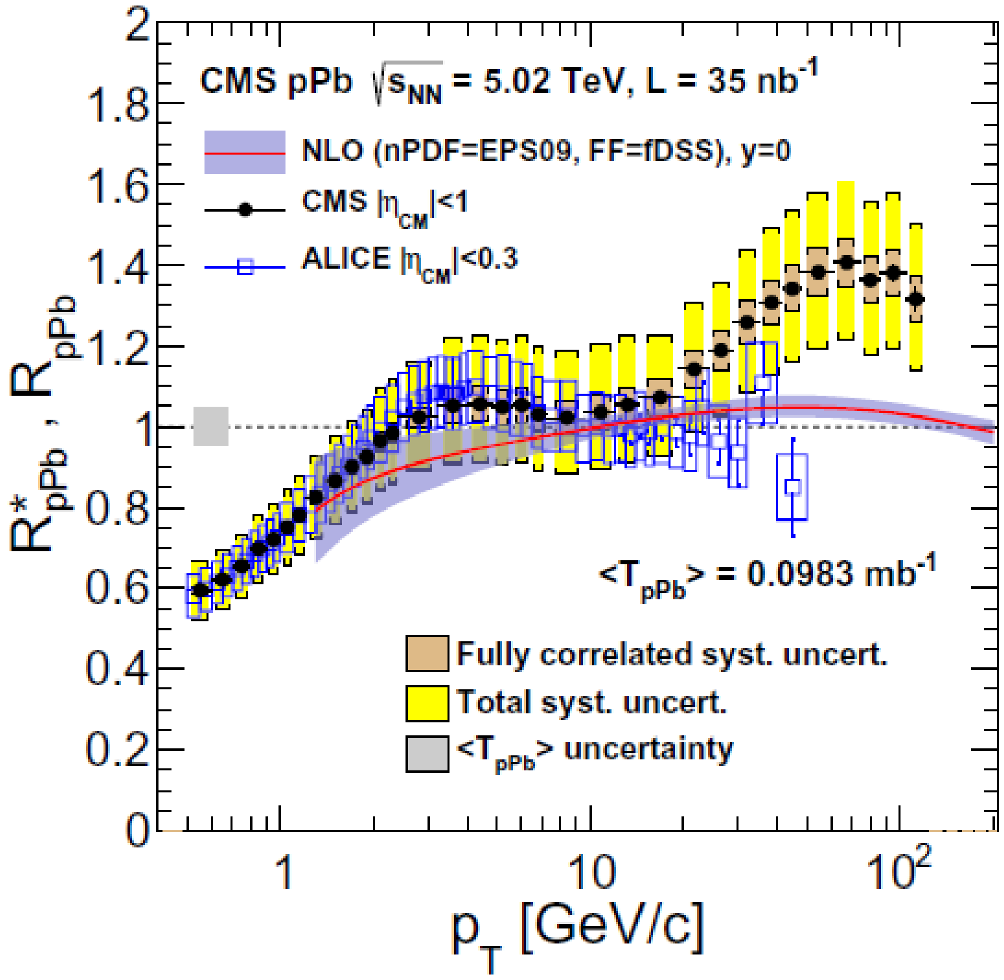
PHENIX PRL 116, 122301 (2016)

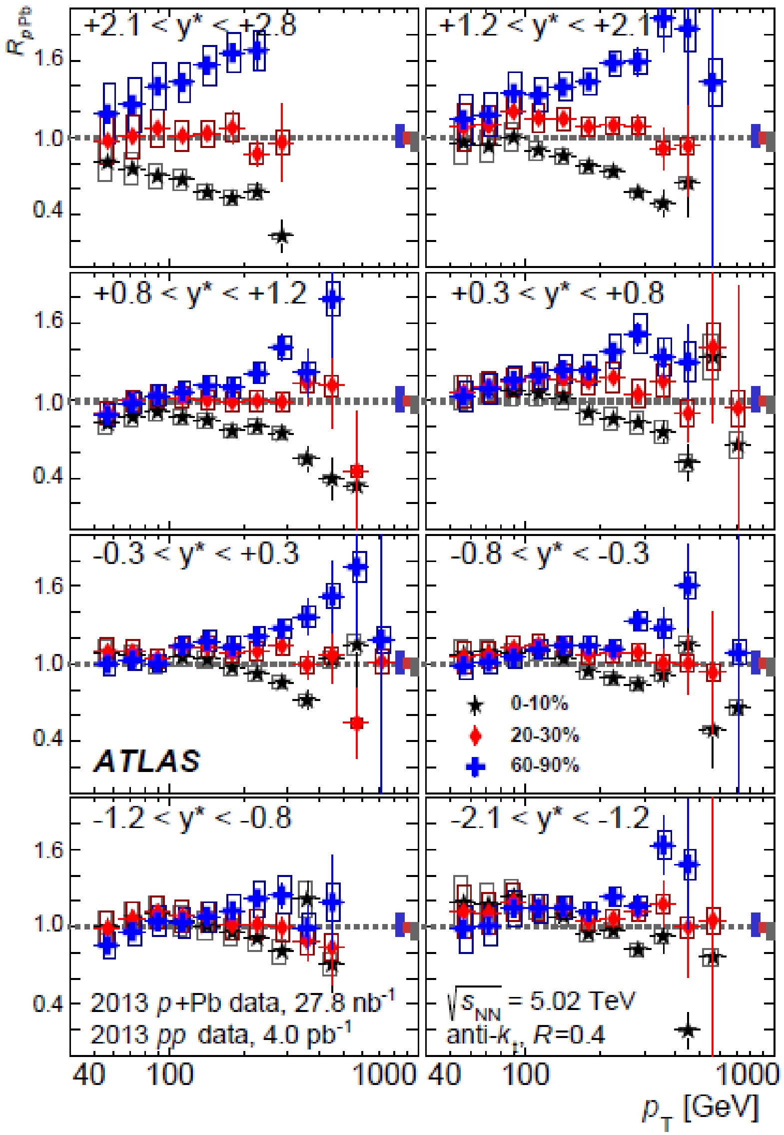


LHC – pick your favorite ☺

PLB 748 (2015) 392

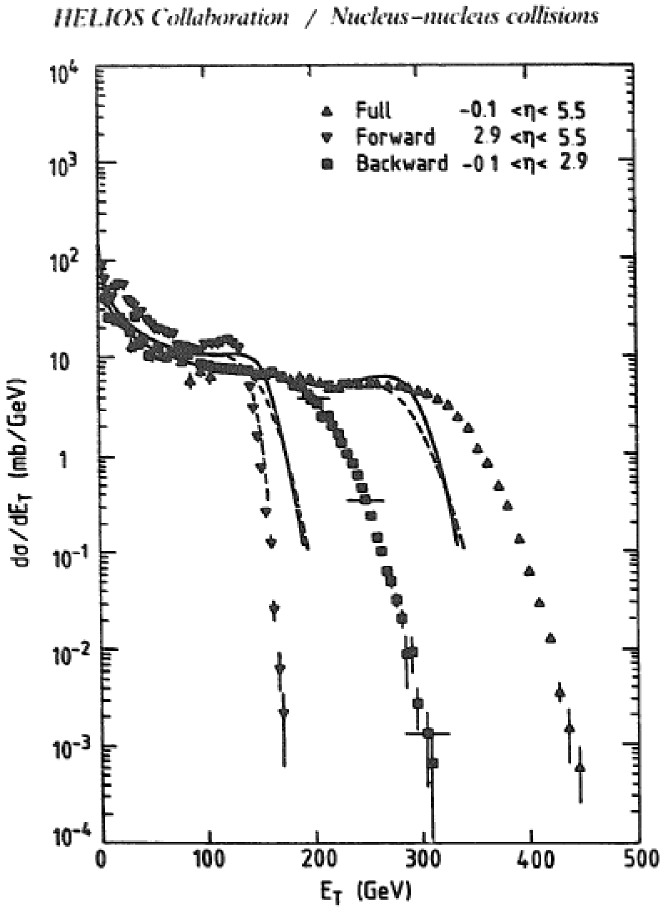
Eur. Phys. J. C (2015) 75:237





First issues with the "naïve" Glauber model

HELIOS S-W, Nucl. Phys. B 353 (1991) 1



Introducing color fluctuations:
PRL 67 (1991) 2946 based on
SPS/AGS ω values estimated

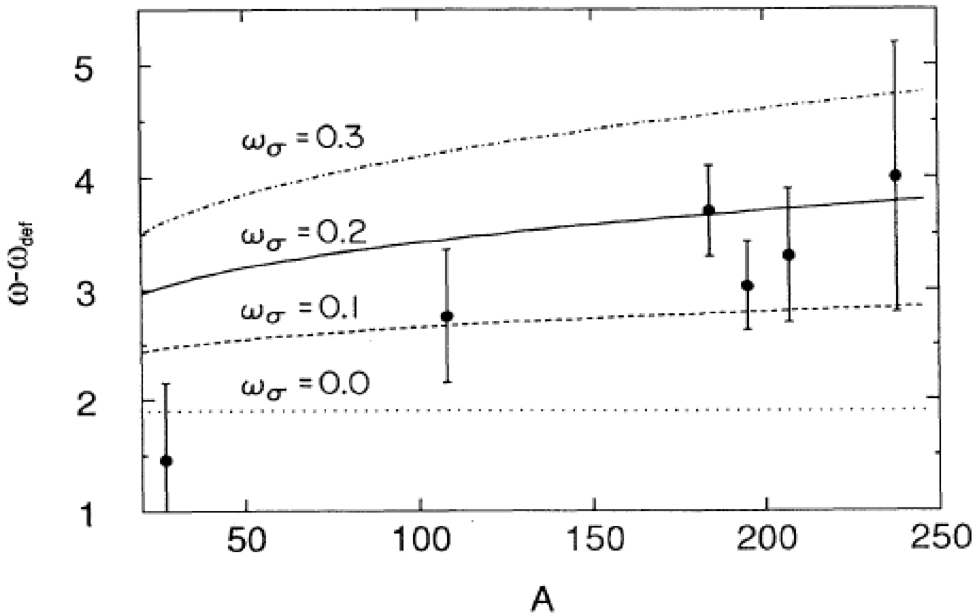
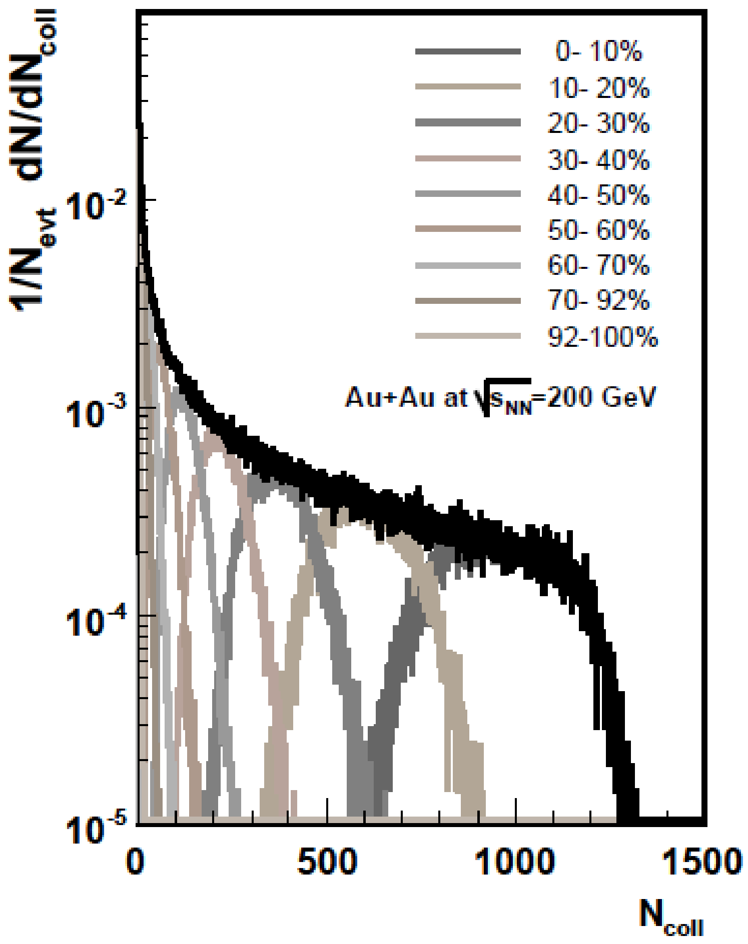


FIG. 1. Fluctuations $\omega - \omega_{\mathrm{def}}$ for central collisions of ^{32}S on different targets calculated with Eq. (13) for various values of ω_σ. The experimental values (dots) are taken from Ref. [12].

Bulk observable, dominated by average soft processes
The tails of the distribution (rare fluctuations) are not described

“Naïve” Glauber model for experimental determination of centrality (RHIC, LHC)



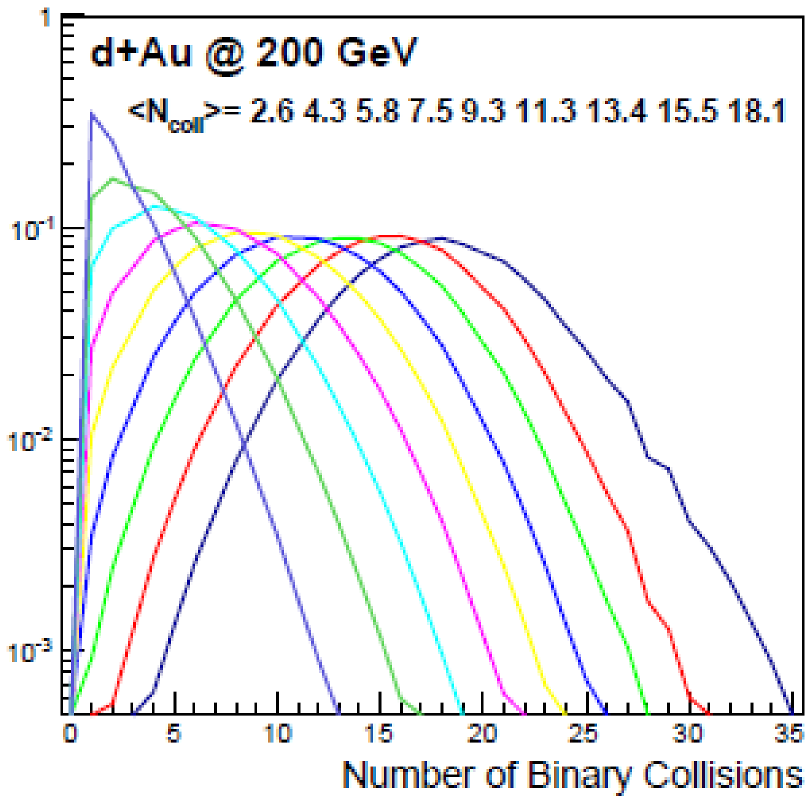


FIG. 4: Extracted distribution of the number of binary collisions in each of the nine centrality quantiles: 0%–5%, 5-10%, 10%–20%, 20%–30%, 30%–40%, 40%–50%, 50%–60%, 60%–70%, and 70%–88%.

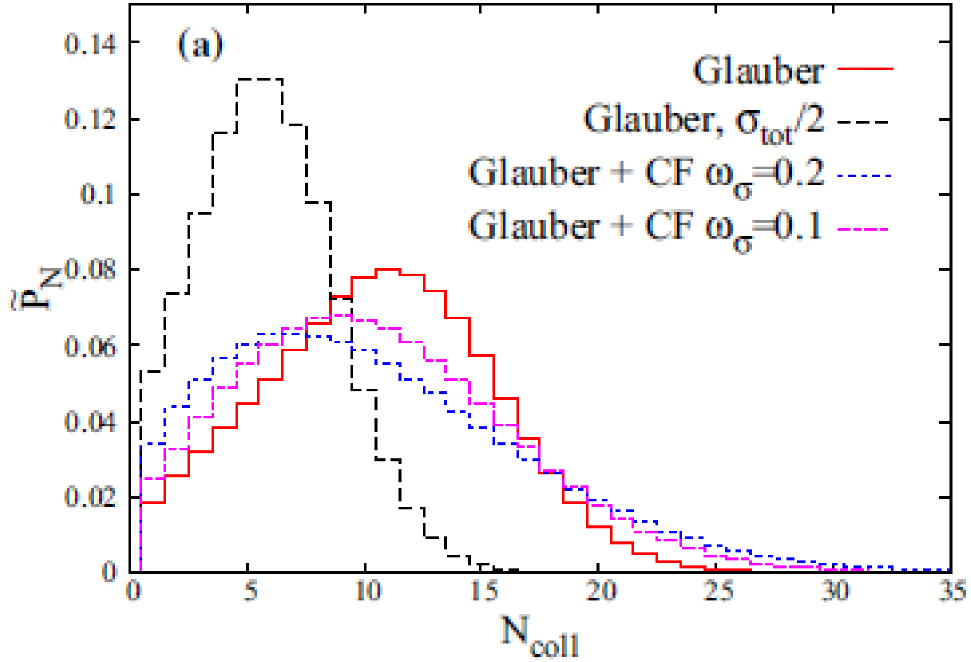
Both still based on “soft production” only

Cross-section (p-size) fluctuations?

PRC 94, 024915 (2016)

PRC 90, 034914 (2014)

M. ALVIOLI, L. FRANKFURT, V. GUZEY, AND M. STRIKMAN



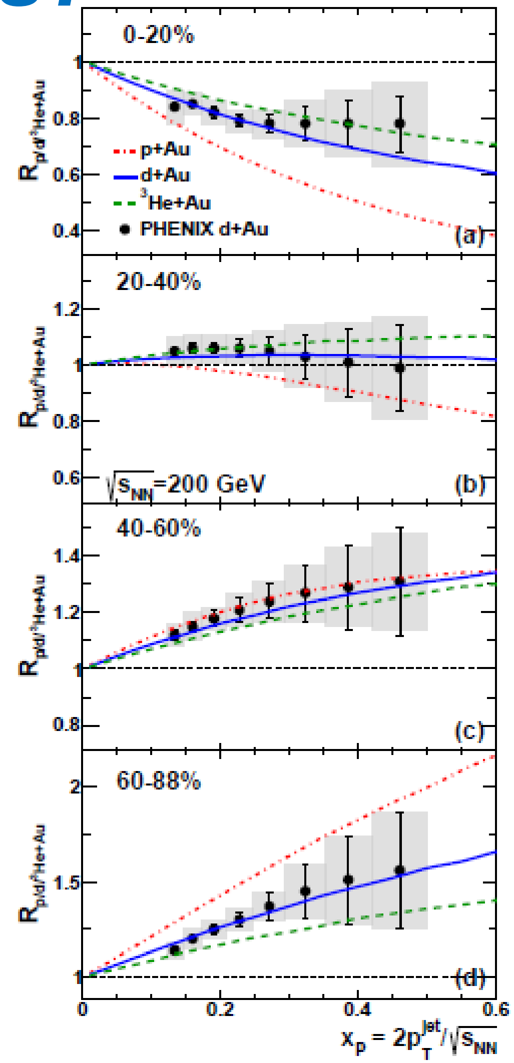
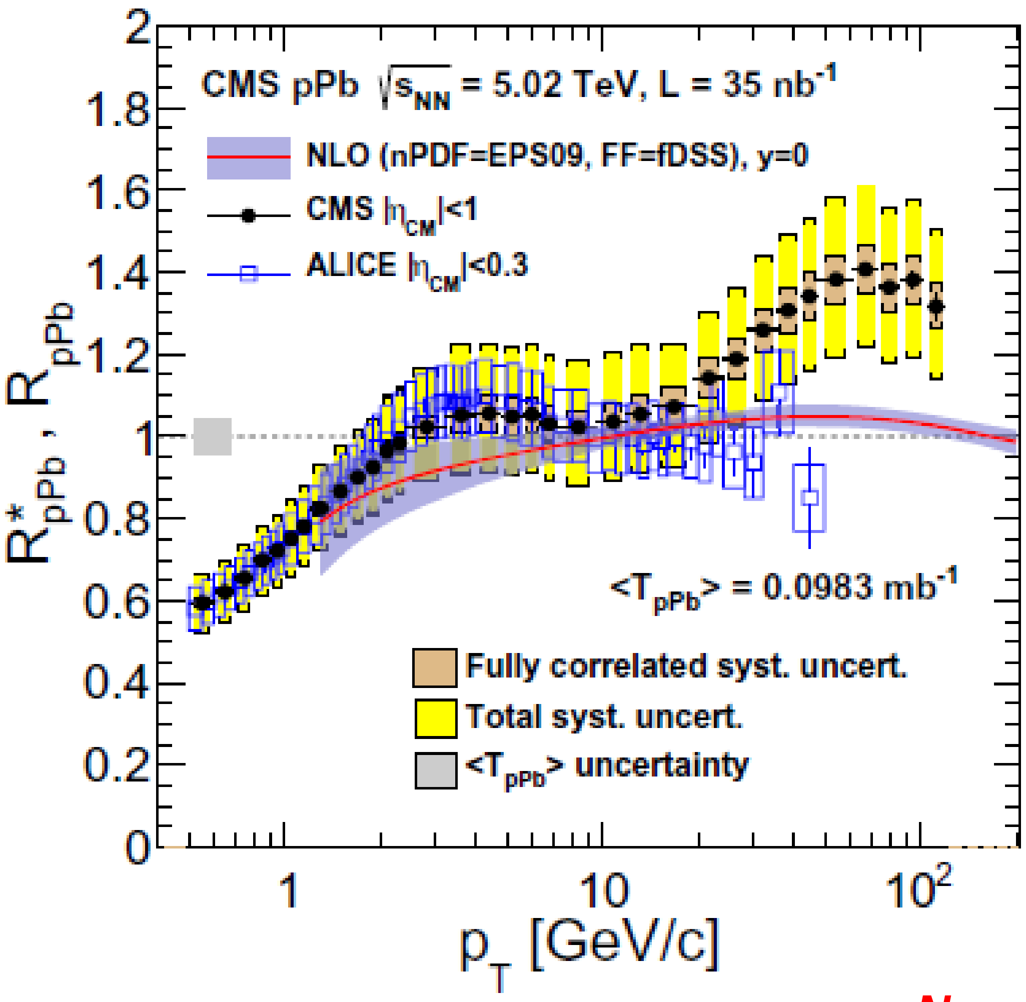


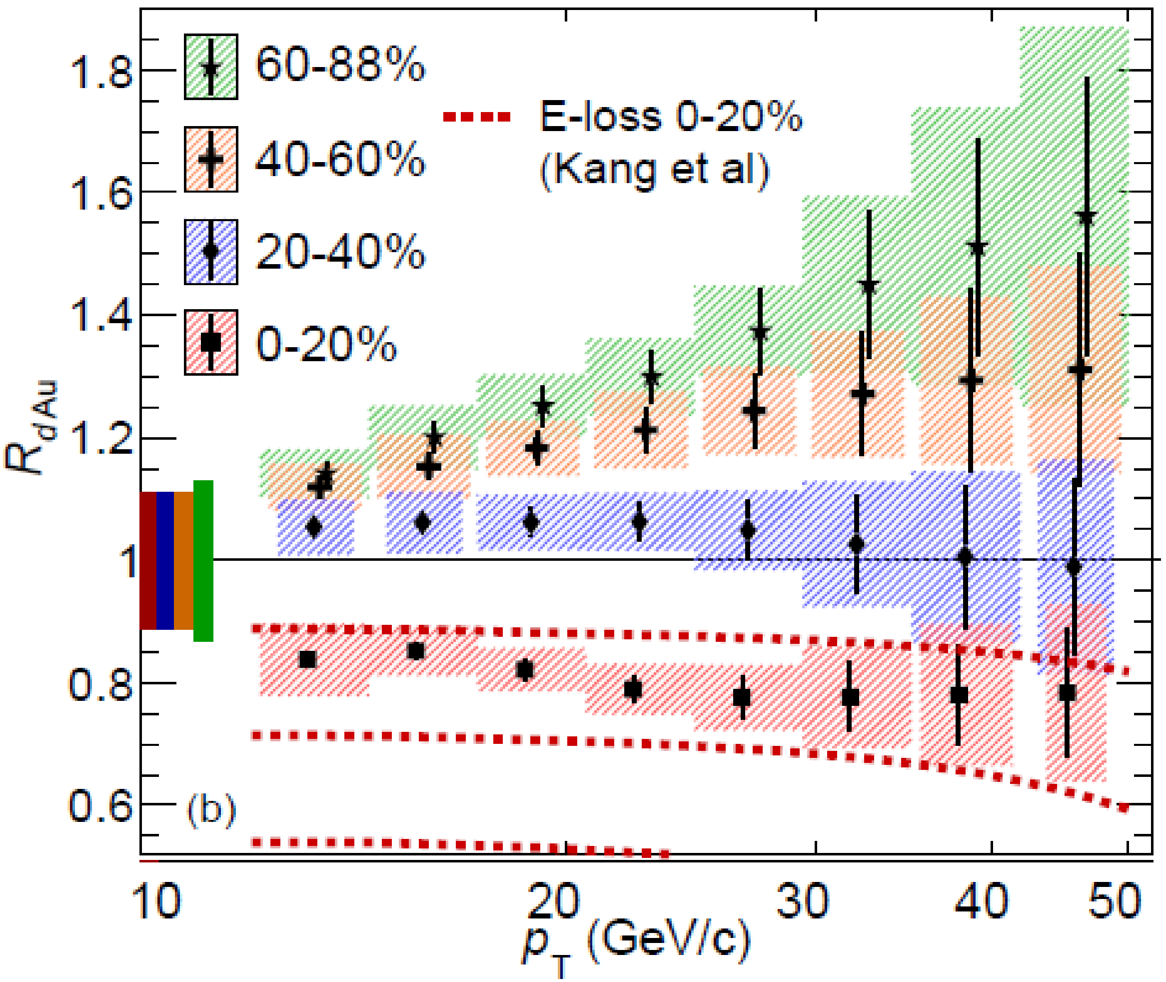
FIG. 1. (Color Online) The calculated R_{p+A} as a function of x_p in each centrality bin for $p/d/^3$He+Au compared to the measured R_{d+Au} of jets in d+Au collisions at $\sqrt{s_{NN}} = 200$ GeV [1].

Recall: some surprising results in p/d+A

CMS (w/ ALICE) EPJC 75 (2015) 237
(charged hadrons, MB)



PHENIX, PRL 116, 122301
(jets, centrality dependence)



New physics???

Are things different with (rare!) hard scattering present? Multiplicity vs highest p_T observed

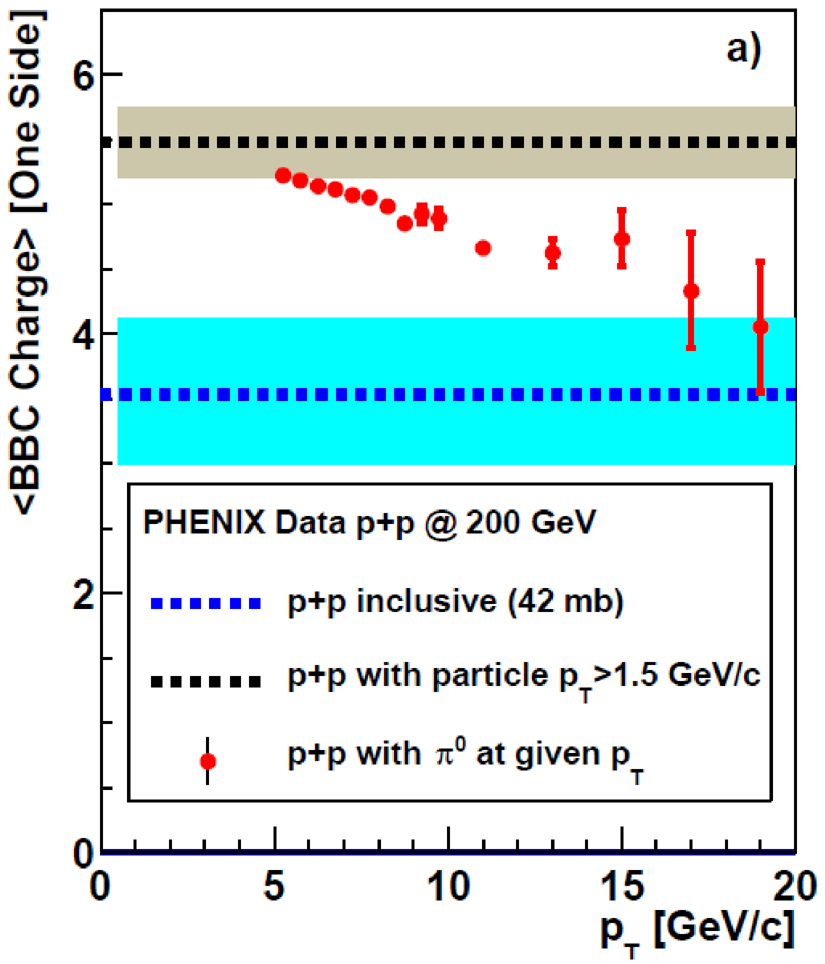
So far color fluctuations:
explained the global distribution of all events, including (but not treating differently) those, that have one or more rare, special interactions

Can centrality still be determined the usual way, or does the picture change?

Experimentally, the only thing you can safely claim is what you observed in single hadron-hadron collisions. Everything elase is hypothesis, even if very reasonably founded.

Fact: the observed multiplicity does change if a hard scattering is present. This change happens way before (and is larger) than kinematic constraints would dictate.

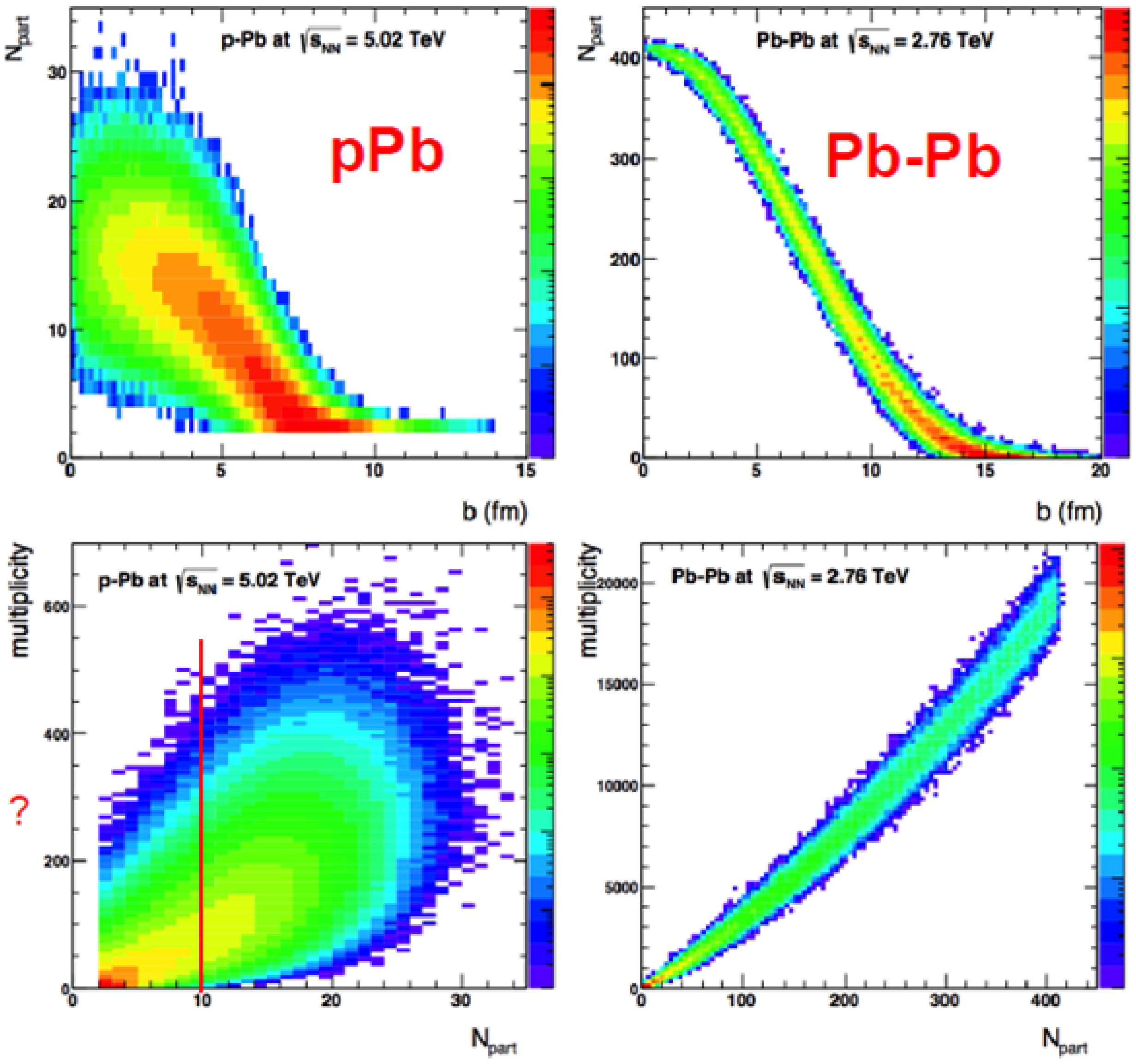
PHENIX PRC 90 034902



A+A vs p+A collisions

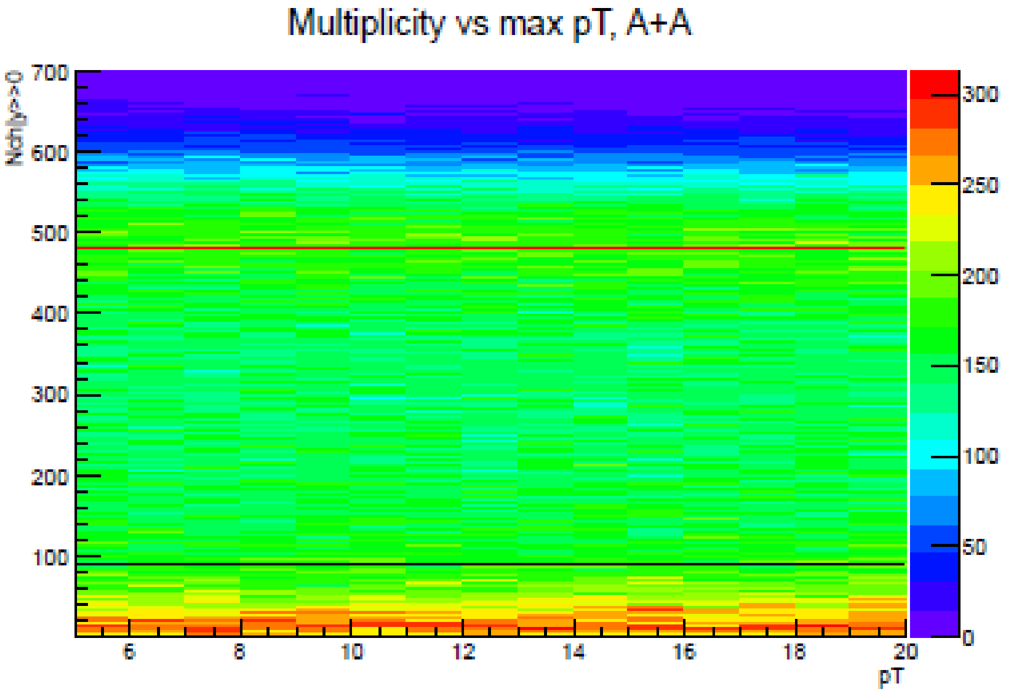
This is not an artifact!
Due to fluctuations and increasing ring size the highest N_{part} values are reached at b>0!

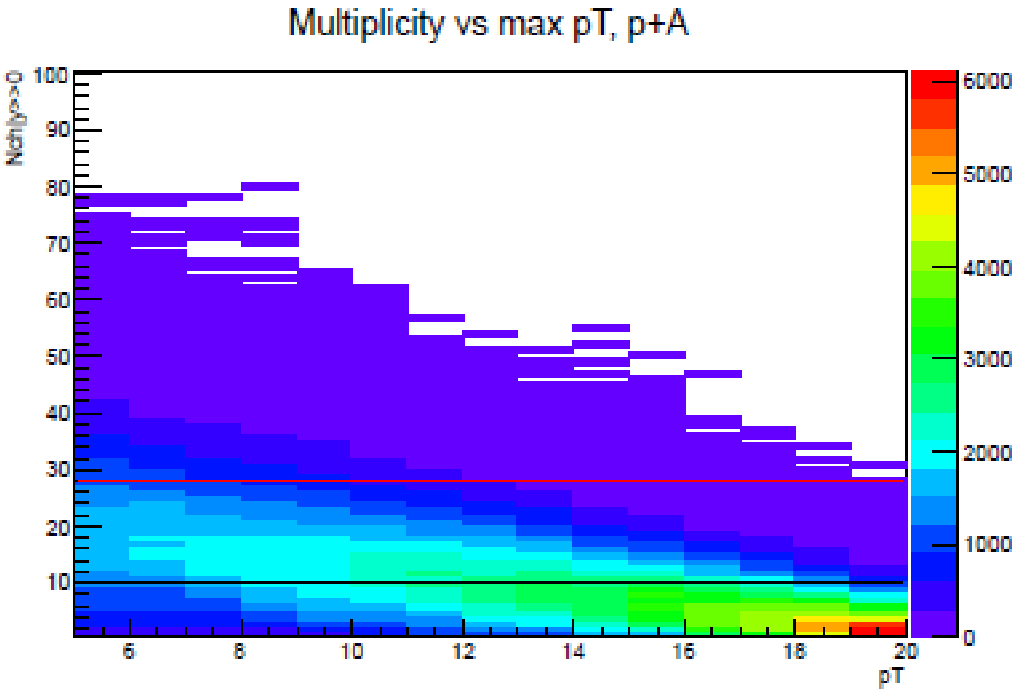
N_{part} (N_{coll}, *multiplicity*) are tightly correlated to geometry (b) in A+A collisions.
The correlation is weak, or even breaks down in p+A (small-on-large) collisions



Multiplicity vs highest p_T observed

Multiplicity forward (high η gap) vs highest p_T particle observed at central $\eta = 0$





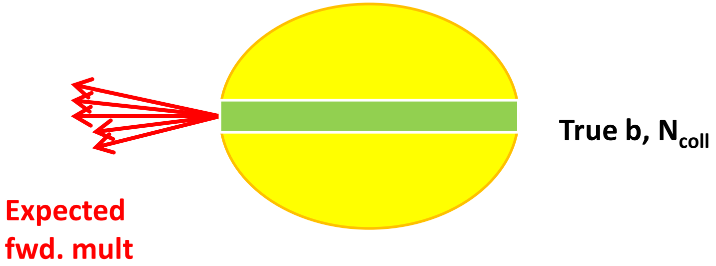
Lines correspond to the lowest/highest centrality class, selected based on overwhelmingly low p_T (bulk) particles in (mostly) average events (no hard scattering). *These are purely experimental observables.*

Centrality classification remains unbiased in A+A, has a *pT-dependent bias in p+A. (Alternately, it can indicate very strong suppression in "central" collisions, but dijet correlations contradict this!)*

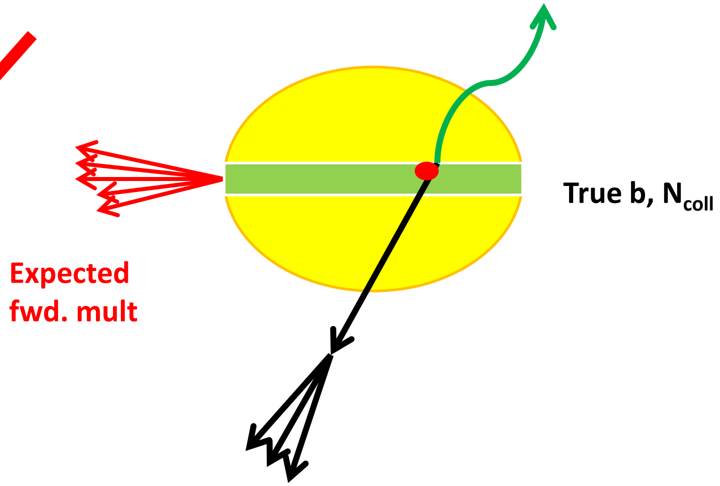
I would strongly advocate to always make these types of plots, separately for hadrons and photons!

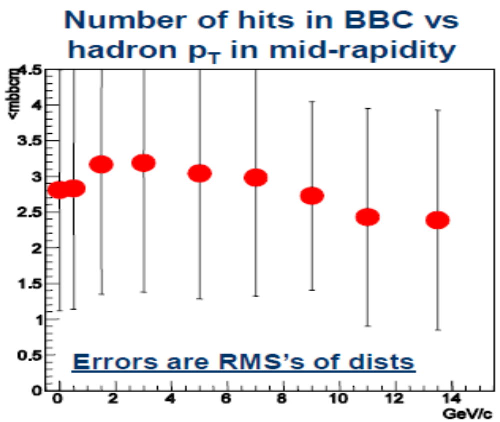
Illustration: shift between multiplicity classes

Here is your average,
higher centrality p+A event

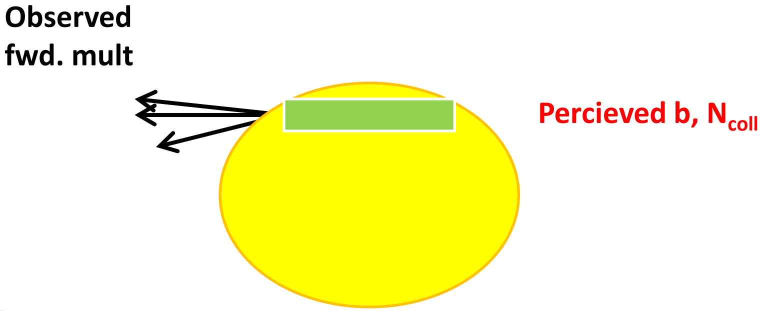


But now a very hard scattering happened (very rare!)
The forward response is reduced, for some reason
(color fluctuation? kinematics? jet bias? other?)
At this point doesn't really matter, because…





…you only observe
multiplicity and that's
how you classify the event…

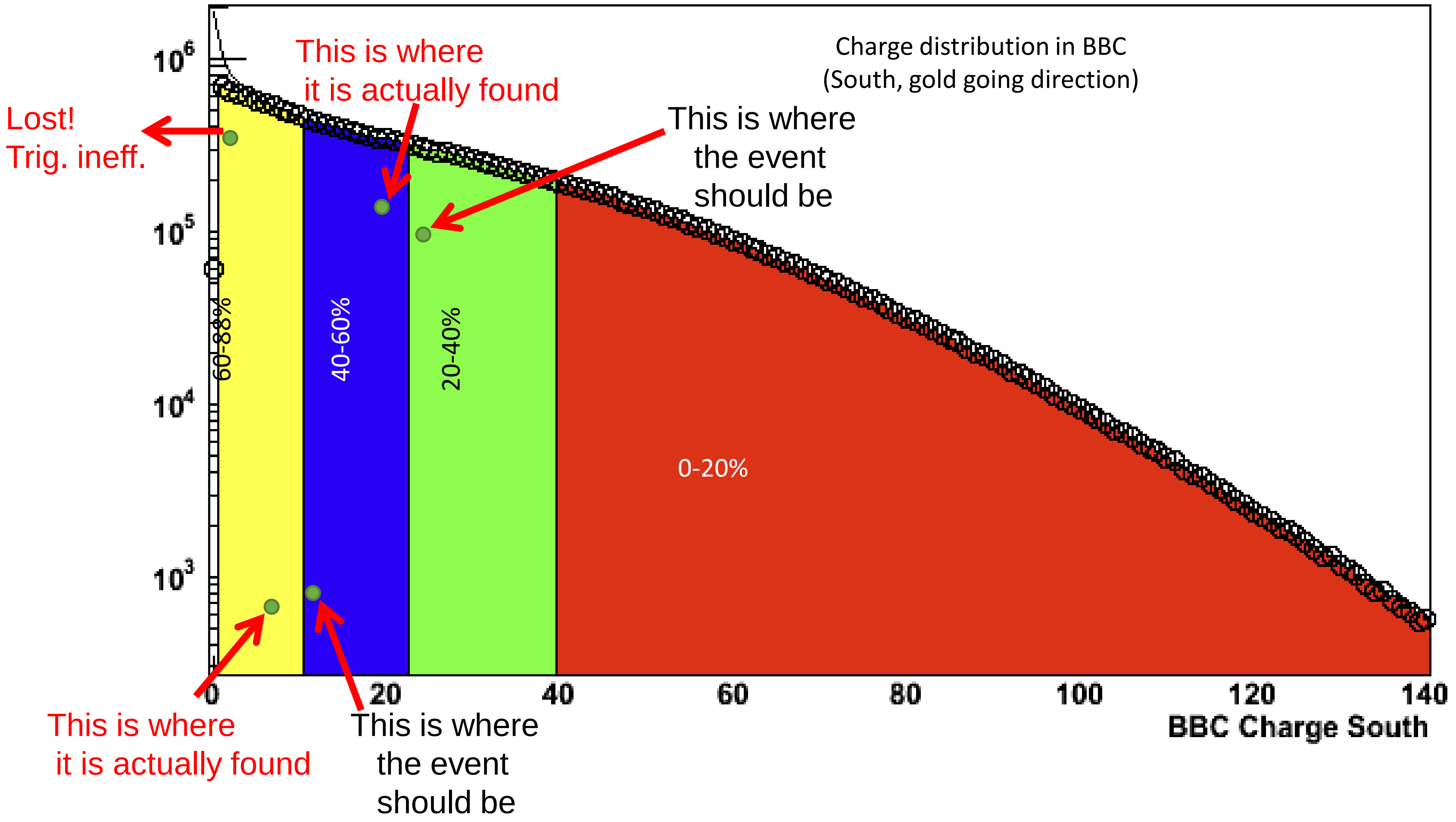


…and when you then calculate R_{AA},
the denumerator (N_{coll} * s_{pp})
will be smaller than it should be
→ R_{AA} increases

(There can be other, even more serious effects)

A simple way to mess up

… if you neglect the fact that the multiplicity distribution (absolute and/or η) changes with the highest Q^2 interaction in the event. At this point, as an experimentalist, I don't care what caused the change, because I don't know (yet) how to prove it. All I know is that I have to correct for it, if possible.



Theory is great, but is it verifiable?

Since QM'12, p/d+A results – specifically, strong suppression in "central" and large enhancement in "peripheral" R_{pA} at high p_T caused pretty vivid discussion.

Claims of "new physics" vs claims of bias in centrality determination (essentially a ***breakdown in factorization of hard and soft processes***)

Some examples (there are many more):

PRC 93, 034914 (2014) → "flickering" , ***x***-dependent color fluctuation; kinematics also plays a role at mid-rapidity

PRC 94, 044901 (2016) → hard scattering (large ***x***) reduces soft production; basically empirical approach with a touch of kinematics

PRC 94, 024915 (2016) → color fluctuations; large ***x*** connected to "shrinking" of the nucleon plus "impact parameter dependent shadowing and saturation effects"

Issue: all these resulted in re-interpretation of centrality based on some model except for ALICE who simply "gave up" (in a positive sense) and stopped showing "R_{pA}" referring to a purely experimental quantity instead (Q_{pA}).

ALICE – same quantity, different centrality definition

Important, bold step:

stop talking about "nuclear modification factor vs centrality"

→ "***event activity***"

then add another question:

→ "***where***"?

(i) CL1: the number of clusters in the outer layer of the silicon pixel detector, $|\eta| < 1.4$;

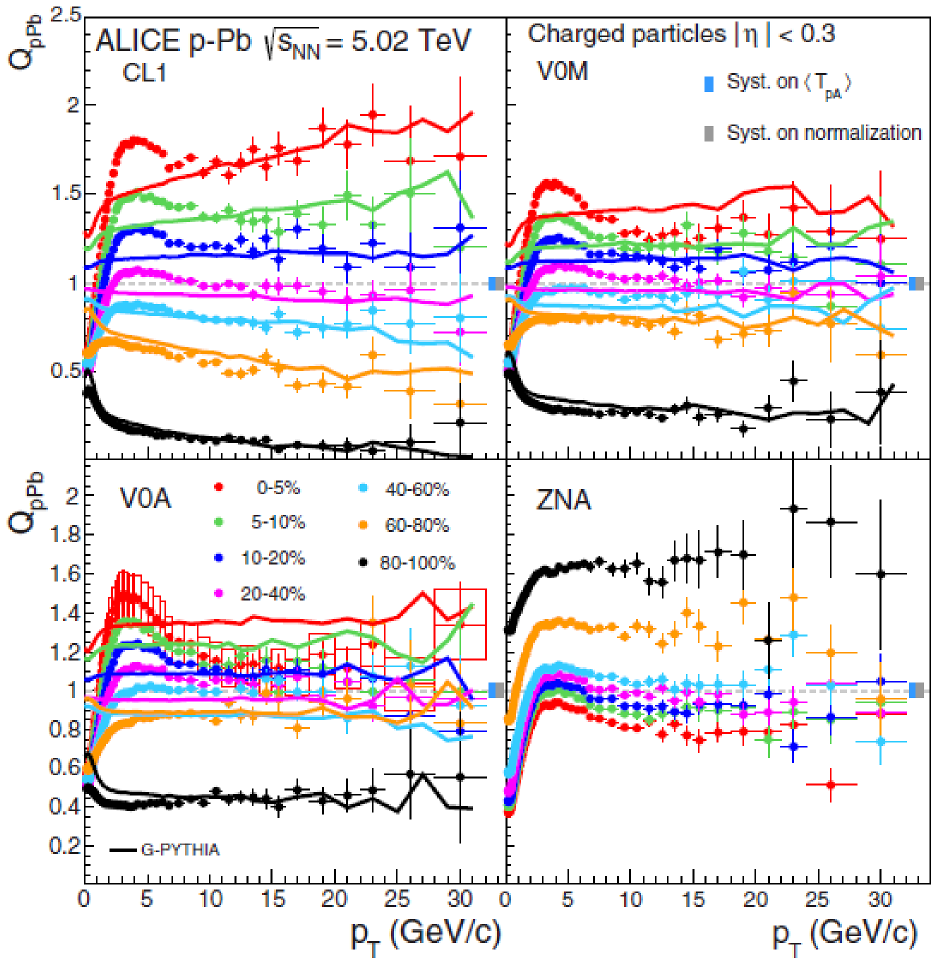
(ii) V0A: the amplitude measured by the VZERO hodoscopes on the A side (the Pb-going side in the p-Pb event sample), $2.8 < \eta < 5.1$;

(iii) V0C: the amplitude measured by the VZERO hodoscopes on the C side (the p-going side in the p-Pb event sample), $-3.7 < \eta < -1.7$;

(iv) V0M: the sum of the amplitudes in the VZERO hodoscopes on the A and C side (V0A + V0C);

(v) ZNA: the energy deposited in the neutron calorimeter on the A side (the Pb-going side in the p-Pb event sample).

J. ADAM *et al.* PHYSICAL REVIEW C **91**, 064905 (2015)



ALICE, Q_{pPb} – current final word

Hybrid method
Assumptions:
- ZN is unbiased by global mult. or high p_T production
- midrapidity N_{ch} scales with N_{part}

Deduce N_{coll}

(i) N_{coll}^{mult}: the charged-particle multiplicity at midrapidity is proportional to the number of participants (N_{part});

(ii) $N_{coll}^{high\text{-}p_T}$: the yield of charged high-p_T particles at midrapidity is proportional to the number of binary NN collisions (N_{coll});

(iii) $N_{coll}^{Pb\text{-}side}$: the target-going charged-particle multiplicity is proportional to the number of wounded target nucleons ($N_{part}^{target} = N_{part} - 1 = N_{coll}$).

CENTRALITY DEPENDENCE OF PARTICLE PRODUCTION . . . PHYSICAL REVIEW C 91, 064905 (2015)

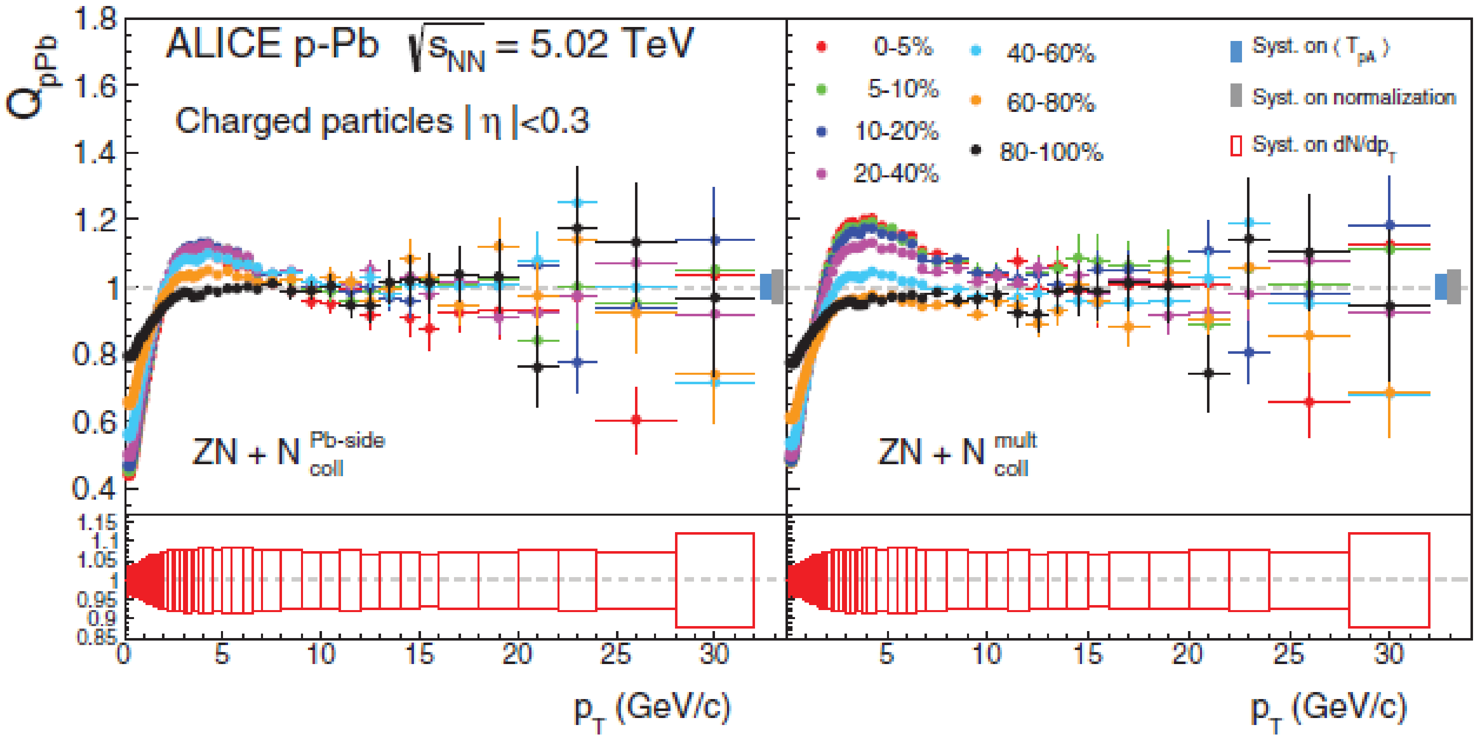
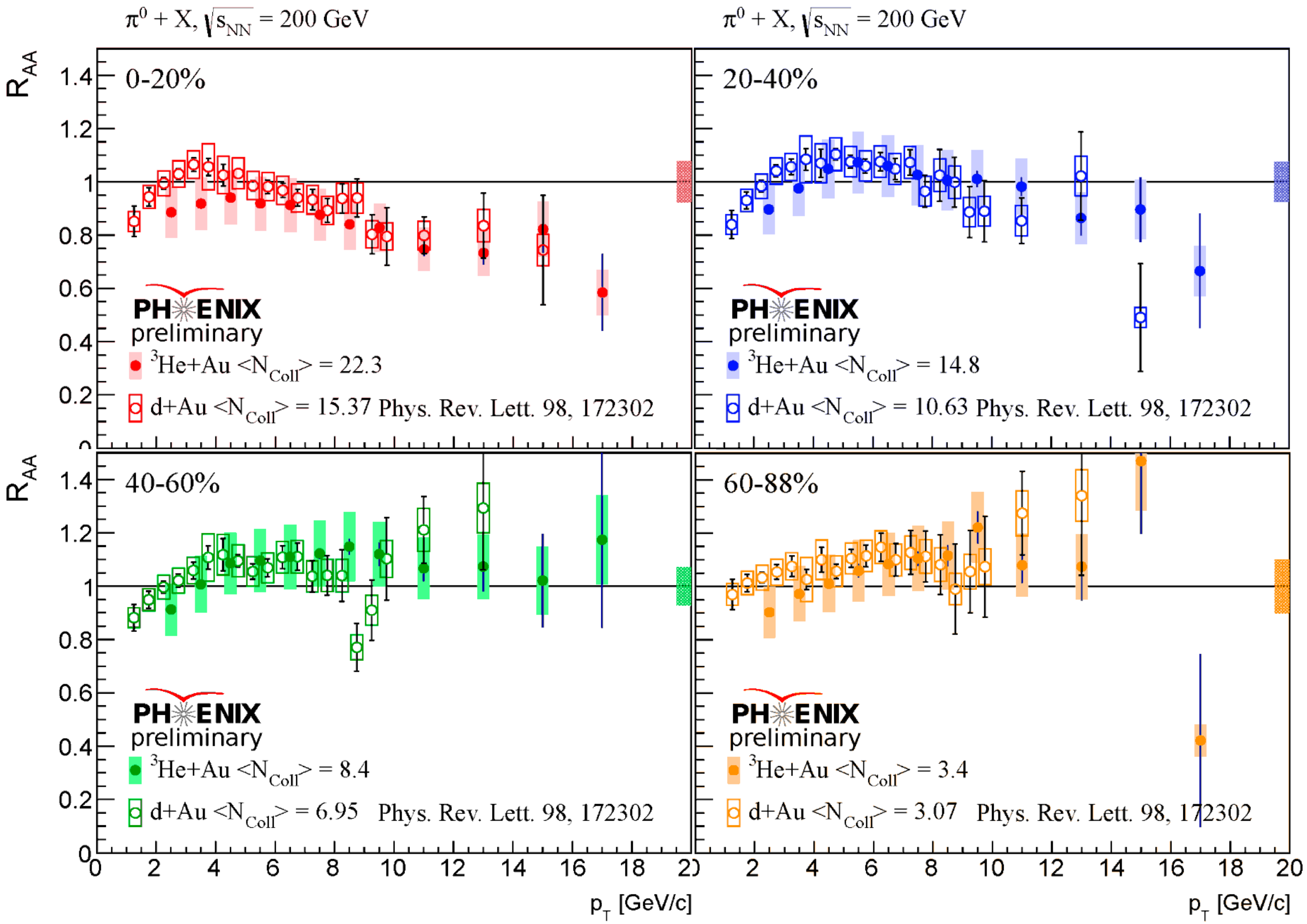


FIG. 20. (Color online) Q_{pPb} spectra with the hybrid method. Spectra are calculated in ZNA classes with ⟨N_{coll}⟩ as given in Table VII, and are obtained with assumptions on particle production described in Sec. VI.

PHENIX – 3He+Au

3He+Au,
Final d+Au and p+Au
coming soon!



Now an observation and an Ansatz:

1/ Observation: Glauber model (and the connection between geometry – multiplicity) works in A+A well (logical: only a few participants have "extreme" collisions, this is swamped by the regular particle production of the remaining “average” binary collisions)

2/ Assumption: whatever effect (IS, FS, modifying R_{AA}) does NOT exist is A+A, will not exist in p+A, p+p (doesn't mean it is necessarily measurable in A+A)

-> Corollary a/ ***if photons prove to be "standard candle" in A+A, they will be standard candle in p+p, p+A***

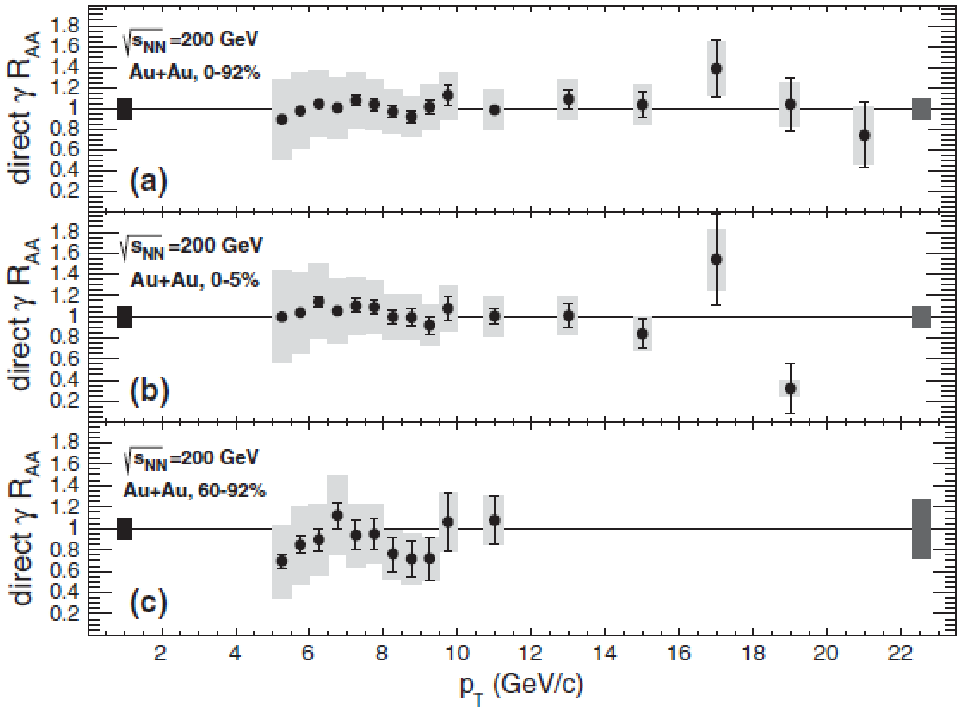
3/ for all we know, photon (W? Z?) IS a standard candle (SC) at high p_T (pQCD region) – modulo isospin (pp, pn, nn, calculable)

-> Corollary b/ ***since photons are not modified in A+A (where centrality is unambiguous), there’s little reason to assume they will be modified in p+A***

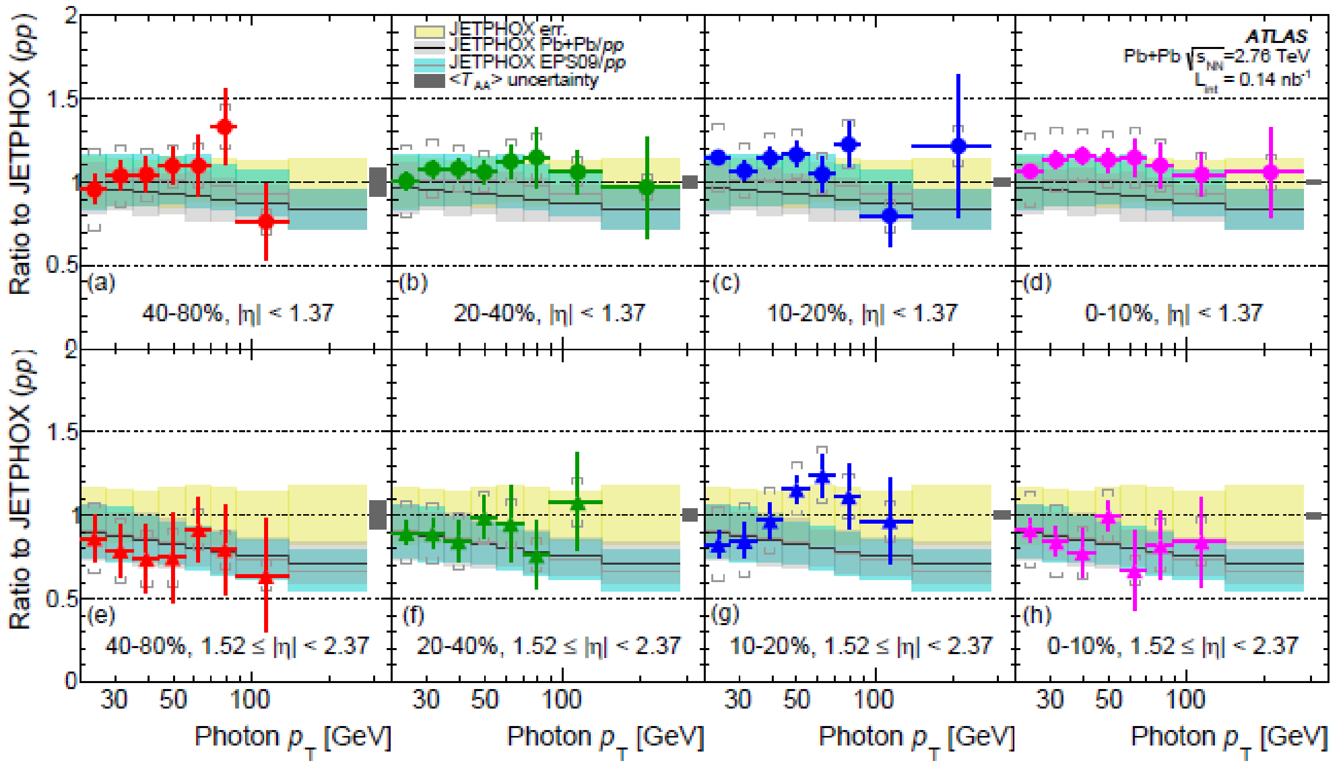
Disclosure: the only new mechanism able to spoil high p_T ISOLATED photon spectrum is jet-photon conversion, but this 1/ is small in current calculations 2/ could in principle be measured in the back-to-back isolated photons channel (Norbert Novitzky)

But photons are standard candle in A+A

PHENIX photons, Au+Au
PRL 109, 152302 (2012)



ATLAS photons, Pb+Pb, PRC 93, 034914 (2016)



(And recall: they worked perfectly in p+p, at all energies!)

A truly experimental way out

Assume that high p_T photons are indeed standard candle of N_{coll}

Feel free to play with any phenomenological model of hard/soft production, bias, specifics of frozen initial conditions, generalized PDFs, fluctuations of interaction strength, nucleon size, diquarks… etc., try anything you want, but…

…once you came up with a model to connect geometry to observables, test it against production of high p_T photons, and over the largest p_T range available

If you find that the photon "nuclear modification factor" (defined with your method) is not unity, your model is wrong.

(Small deviations from being a "standard candle" may exist, but they are testable.)

Summary

Some very ***counterintuitive*** new results seen in p+A collisions
→ strong ***temptation to declare new physics***, discovery

My personal preference: if you found something ***revolutionary***, go ***back a dozen times***, and try to disprove it, asking: "***what did I miss or mess up***"? What assumptions did you make automatically that worked well – but not under these conditions? (Everybody makes his own choices, but this rule of thumb saved me more than once from declaring victory where there was none.) We ***don't need pseudo-discoveries***.

My belief (***unproven, but in part testable***) that traditional methods of connecting geometry to multiplicity (or other bulk variable) ***in p+A*** introduces a ***strong bias*** that changes (increases) with the ***momentum of the hardest scattering*** in the event

Suggestion: you can define/model centrality in p+A any way you want. It's fine: hypotheses are our basic modus operandi. However, if it doesn't pass the test, that ***prompt photon production is insensitive to centrality (R_{pA} ~ 1, modulo isospin effects at any high p_T)*** then your model is wrong.

This admittedly doesn't give you a recipee how to find the right way to nail down collision geometry in p+A, but gives a ***decisive test to weed out unreasonable models***.

Sermon

Avoid ***ideomorphic*** science (looking at the data only in ways that favor your shining new idea)

Remember ***Occam***: if your result might be an Earth-shattering discovery, re-writing textbooks
– or just a mistake or unintended bias, usually it is the latter

Yes, a big discovery means rapid promotion. Unfortunately, Nature couldn't care less
about promotions, your ego, career, desires – she does what she does.
Learn to listen to what ***she*** says, not what ***you*** want to hear!

Healthy paranoia is your best friend.

Backup slides