

Friday question and answer transcripts

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This is a transcript of the question-answer intervals following the Friday talk presentations. In some cases notes were added during participant review of the transcript (sources are specified). Participants were: Rene Belwied, Helen Caines, Yuri Dokshitzer, Ahmed Hamed, Ulrich Heinz, Jiangyong Jia, Boris Kopeliovich, Guy Moore, Jan Rak, Thorsten Renk, Lijuan Ruan, Anne Sickles, Mike Tannenbaum, Derek Teaney, Tom Trainor.

*Workshop on Critical Examination of RHIC Paradigms - CERP2010
April 14-17, 2010
Austin Texas USA*

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1. Mike Tannenbaum

Jan: Could you show this x_E slope from the direct photons compare to π^0 ? There was this question of associated x_E being correlated with the fragmentation or not. I'm also worried about this direct photon because if the x_E is in a sense the associated mean p_t over the triggered mean p_t and if you change the trigger p_t on the associated side you still see kind of an underlying event.

Mike: It scales. In p-p we show that it completely x_E scales from photons. That's this part here. This is x_E -scaled data for all the different...[this is $\xi_E = \log(1/x_E)$] this completely x_E scales.

Jan: But the slope is still rising also for the direct photons...

Mike: No, I don't like to do it that way, if we compare it to the literal fragmentation function. Now, the next thing to do is a fit of these points from the fragmentation function to see if it's really constant. We don't get the whole fragmentation function because our detector is small. We haven't done that yet. This is the away-side x_E distribution for trigger on direct photons. That's the fragmentation function, assuming it's exponential. Away from the lower z it's exponential. You put it [FF] in and calculate for both sides and you find the x_E distribution does not match with this B at all....That to a power n . n is the same as for the p_t spectrum. This is not the fragmentation function. The only sensitivity to the fragmentation function is the *integral*, the mean [jet] multiplicity.

Jan: That's right but the right-hand side, the difference between photons and π^0 , is not that big right?

Mike: That's true, but in the new data it's dramatically different. You have access to that.

Rene: What defines your cutoff in the ξ plot, in the Borghini-Wiedemann plot? Why do you cut off at that point? [Mike: we can't mention that]. What is it, 2 GeV or so?

Mike: No, this is such a terrible... it's not quite as bad as Tom's variable but...

Tom: Why is this a "Borghini-Wiedemann" plot? This is a conventional plot of the fragmentation function.

Mike: The Borghini-Wiedemann plot shows a modified... has another thing on there which shows that its modified.

Tom: But in this plot they're irrelevant. This is a standard fragmentation function.

Mike: Yes. So I call this plot... The reason they did that was because they knew we couldn't measure jets and so they measured this. So, I said Sam Ting measured this with π^0 inclusive, and you know the energy of the jet.

Thorsten: This is a fragmentation function plotted in a variable that magnifies the low...

Mike: Absolutely. I thought this was a waste of time, not for what Yuri made the plot for. For that it's great. We just won't get there.

Rene: What is that cutoff?

Mike: Cutoff is half a GeV. There's the direct gamma. x_E is 0.2.

Tom: That's still not answering the question. If you plot these on real momentum or real rapidity you'd be able to answer these questions. With this fractional stuff you don't know where you are in real momentum. In this other plot you're down around 1 GeV or less, and that's where all the physics is.

Mike: Maybe all the physics but you can't deal with it.

Tom: *You* can't deal with it.

Mike: You can't do it in Au-Au, let me put it that way.

Rene: I'm just surprised that the 15 GeV jet cuts off at exactly the same point as the 9 GeV and 7 GeV jets? Why doesn't it reach further?

Mike: It does [reach further]. I have the p_t spectra. I just like the scaled spectra.

2. Jianguong Jia

Thorsten: In your brief overview you grouped medium response to initial fluctuations and a different group of medium response to energy deposition by jets. I don't think that needs to be done. Because you have fluctuations in initial conditions. Of course you have probably energy deposition by jets, and the way the medium responds is a property of the medium. So, it would respond to any perturbation, regardless of where it comes from, in a characteristic way for the medium.

Jianguong: I agree with you. But, if you have initial fluctuation plus radial flow you don't need a jet.

Thorsten: But you have a jet. And you see that the jets are suppressed. And the energy which you take out of the jet, the leading parton, goes somewhere. So, if it goes to the medium the medium will respond.

Jianguong: I agree with you. But the Brazil people don't have jets in their calculation.

Thorsten: Yes, they have a medium response. If they have a jet and dump jet energy in they have a response to that as well. If they don't transport their energy away they have a problem.

Rene: Go back to slide 30. We have indications that there is different physics going on in the ridge and in the cone than you would expect probably from modified fragmentation. Because you can look at the baryon/meson ratios, the spectra. The problem I see with that though is that everything then starts to look like the medium itself, like the bulk. The ridge chemistry is the same. [Jianguong: The spectrum is harder. The temperature is different.] In the soft ridge for example...harder. So, I don't think you can distinguish between a medium response and fluctuating initial conditions.

Thorsten: You wouldn't want to, because the medium responds to fluctuating conditions, that tells you about the medium. The medium response to jets that tells you about the same mechanism in the medium. So, why would you want to distinguish between one and the other?

Jianguong: You have two sources of fluctuations: Global initial-state fluctuations and also jets. [Rene: which is hard.]

Thorsten: The energy deposition from the jet is not hard. The jet is hard. The radiated spectrum of gluons from the jet is not hard.

Guy: But you're assuming it's fully absorbed by the medium.

Thorsten: If you assume that, yes.

Jianguong: But their correlation pattern may be different from a global fluctuations.

Ulrich: I was confused by what you said about the medium. You said you need to worry about the difference in the distribution of the primary production points of the jets vs the distribution of the medium. And you showed these color plots which show different eccentricities and more compact formation profile than the matter profile. And then you said if you change that matter profile from a Glauber distribution to a Color Glass [CGC] you have to rescale something?

Jiangyong: I had some slides to show you. This is a known fact, that these people Drescher and [??] when they did the calculation saw that the CGC leads to a narrowing gluon distribution relative to...because this starts with a Glauber model N_{part} distribution. Then I do some mathematics. They take an average of...they basically modify this Glauber distribution and the size in x and y direction shrinks. But in x direction it shrinks more. That leads to a larger eccentricity, but also smaller size. Is this physical? The eccentricity is, but is the size?

Ulrich: Yes, because in that model the gluon production is controlled by the saturation scale, and its distribution in the transverse plane.

Jiangyong: Of course in that model...

Ulrich: What you have to insure is that both matter profiles have the same total number of gluons. That's what controls how you should scale. You should *normalize* the Color Glass distribution (don't change its shape by changing its radii), you should readjust its normalization...

Jiangyong: That will break the eccentricity scaling.

Ulrich: You will always have breaking of the eccentricity scaling for R_{AA} if the eccentricity of your jet distribution is different from the eccentricity of your matter distribution.

Jiangyong: You compare two different matter profiles. The difference in the v_2 is not proportional to the difference in the eccentricity of the matter profile. But the jet is fixed. I'm looking at high momentum.

Ulrich: But if you have a jet binary-collision distribution and the matter Glauber distribution the latter is less eccentric than the former. If you take the CGC matter distribution it's more eccentric than the binary-collision distribution. So, the breaking of eccentricity scaling will go in the opposite direction.

Tom: A comment and a question:

The comment is this: We say the jet is modified by the medium. I would always put medium in quotes there, because the sharp transition (in jet correlation properties) is at a centrality where you just start to have more than one jet per event. You have two or three [for instance within the STAR TPC acceptance]. The actual particle density in those events is quite low. It's the multiplicity of jets [jet number], rather than the multiplicity of ultimate fragments, which seems to be important.

The question is this: You have the away-side jet which is deformed. That is, you have the away-side ridge deformed—the suppression, double peaks etc.—and you posit that this is “away-side” jets, a construct. You have a trigger, and you claim you are then looking at single jets that are opposite from your trigger. I believe this is a mythology. There is no away-side jet.

Jiangyong: I'm not saying away-side jet. I'm saying away-side medium response. It's not directly jet fragmentation.

Tom: But this [away-side double peak] has been proposed as [a single] jet structure. Why don't we see this [structure] on the same side. We should be seeing an *intrajet* structure on the “same side” which is modified by the medium. But we never see that.

Mike: You're absolutely right Tom.

Tom: In fact we see this same-side narrowing [on azimuth] which Lanny has emphasized, which Duncan has emphasized. We don't see any local two-particle correlations corresponding to this [claimed jet] modification [double peaks].

Jiangyong: You mean the narrow peak?

Tom: Yes, we should see this [away-side jet deformation] on the same side, small-angle correlations.

Jiangyong: On the same side you're looking at two-particle correlations. If you do three-particle correlations, look at two-particle on the away-side, Fuqiang will tell you that you see that structure.

Tom: I'm well aware of what Fuqiang would say. [laughter]

Yuri: One comment. The punch line is don't get fooled by theorists. There's one specific aspect, the question about AdS/CFT stuff. This is very special field theory, a wonderful mathematical tool which may yet help understand a lot of things about QCD where gluons are dominant. But, for example η/s may be a very nice thing to study. However, when it comes to energy loss the situation is totally different. Because what they're doing is taking an external force which is a triplet and putting it into the medium, into the field theory, which has only octet objects. So, [??] theory has nothing to do with [??] because there's no confinement, etc. And then what happens is the energy loss is proportional to the energy of the particle, which can *never* happen in a quantum field theory. What is actually happening is that when you are dragging your charge through this medium it's not a particle [??] couple your universe. That's why you have to supply work over time. And this I suppose...just translated into LPM... Here you have to be very cautious.

Jiangyong: That's why I'm not using...in my calculation. Just need some motivation. But if you take Edward Shuryak's... Looks to me like [??] this large anisotropy ... model... you need some nonlinear type of path-length dependence. It could be path length still be linear but it's not directly proportional to density. So, Edward said you have this onion region where around T_c you have the largest energy loss. But that could effectively translate to a higher-order length [dependence?].

Derek: AdS/CFT is in this formal limit where [??] goes to infinity. So for instance, in this calculation you referred to the calculation itself will tell you that it breaks down when the gamma factor reaches $1/\sqrt{\Lambda}$. So, if you put any realistic parameter there it breaks down when gamma is 3, 5, 10. So it will tell you itself that all is not well in the real limit that you're talking about.

3. Thorsten Renk

Tom: Referring to your comment that ridges are not causal, and so forth. I repeat what I said to another speaker: You have a plot on η from PHOBOS. That's polar angle, not true rapidity. So, this is not longitudinal momentum that's being indicated. There's no problem producing a high-angle particle that's very soft. PHOBOS has a hit detector. There could be a 0.2 GeV/c particle coming out at 70 degrees. There's no prohibition against that. You also have the fact that the elongation on η in the untriggered studies has a sharp transition [in jet properties on centrality] that is exactly correlated with the redistribution of the spectrum hard component [suppression at high p_t , enhancement at low p_t], which would argue against the statement you're making. I would not say energy loss. I would say medium modification of fragmentation.

Thorsten: I can actually make the statement just based on the ridge seen in the STAR acceptance. That is not enough. You cannot get the STAR ridge from energy loss just based on that energy conservation [??].

Tom: But that's still pseudorapidity. We're doing the same experiment.

Thorsten: You don't have rapidity?

Tom: Not unless somebody has done an identified-particle correlation study.

Thorsten: My argument would be that the Jacobian translating η into y_z is mainly different around zero. The more you go to forward rapidity the better the correspondence.

Jiangyong: I agree with you \hat{q} is hard to define but for comparison across different theoretical models since you need to define some quantity you can calculate it's the same setup. In that sense \hat{q} might still be worthwhile.

Thorsten: You can do two things. If you want a cross comparison across models you take the underlying medium evolution and the jet quenching formalism and average across your model. That gives you \hat{q} . The problem is that \hat{q} has nothing to do with the data. Or you can ask for a \hat{q} which is related to the data. That knows about trigger bias, but that erases differences between models because you probe a model in a very specific way, so you might not be sensitive to the fact that your models are different. So there is no one single averaging procedure which gives you *the* correct answer.

Rene: A short comment to the γ -jet [results], I don't think there's a discrepancy between PHENIX and STAR. I think the STAR data are a little bit higher in fractional momentum than PHENIX data. I would assume that you will see that in the STAR data too. You would be able to go to lower fractional momentum. I think we stop at 0.3. And we get data below 0.3.

Thorsten: OK, but this is a drastic effect. You should be able to see this.

Anne: We are in agreement with other models that go up, just not yours.

Thorsten: That is a very characteristic [property] of this energy redistribution. It happens in Borghini-Wiedemann just the same way as [other models].

Rene: Let me ask my question. Can you tell me what the difference is between dependence on the hydrodynamic profile and the path length dependence? When you say it's not the path length but it's the hydrodynamic profile, aren't these things highly correlated?

Thorsten: No. The hydrodynamic profile is a distribution of density. The pathlength dependence is the filter through which I observe this.

Rene: Well, if the density goes up then the pathlength dependence is affected, right?

Thorsten: No, my pathlength dependence is what it is, quoted for a constant medium. So, if I put my probe into a constant medium, that shows a given pathlength. And now I probe a medium which is not constant and this is the hydro profile. If I change the hydro profile obviously my result changes also if I leave the filter as it is. Or I can leave the density distribution as it is but change the filter. So, it's not the same thing.

Yuri: Very short comment on your conclusions and the gamma problem. I don't think [??] a very short length, because in purely perturbative radiation there is also a relation between energy and angle, a very specifically organized situation. Also, If you really want to study this modification one has to be careful, one has to be able to make subtraction, not just take a ratio, subtract medium and vacuum, and then to look at this medium modification.

4. Helen Caines

Jiangyong: Please go to slide 27. The out-of-cone energy depends...it's only different by 500 MeV between the three cases. Out-of-cone energy is 2.5 for 10-15 GeV and 3 GeV for 20-50 GeV.

So, you're saying that can explain the difference of the away-side suppression?

Helen: Yes. If you shuffle the jets by of order "ish" 3 GeV down and then take the ratio...if I just take the p-p and move it 3 GeV...yes.

Jiangyong: What I mean, the half-GeV difference makes the R_{AA} factor 2 difference on the away-side. Is that your message? Because the away-side suppression strongly depends on trigger energy.

Helen: No. I said it *isn't* strongly dependent.

Jiangyong: The suppression of 20-50 GeV jet is much less than the 10-15 GeV jet. [Helen: No, it doesn't.] I'm looking at the away-side, recoil. You see the R_{AA} is 0.2 for the 10-15 GeV jet...

Helen: This is the number of particles, not the total energy.

Jiangyong. Right, but you're saying that out-of-cone energy can explain this difference, or not?

Helen: I'm saying that if you take the folding of these two and add it up you get what we see here. You just shift it. You don't have to shift it by huge amounts to get these dramatic [changes in R_{AA} ??]

Tom: This is perfectly consistent with what I showed on Wednesday, the Borghini-Wiedemann change in the fragmentation functions.

Jiangyong: What's the threshold of the particles you're selecting on the away side?

Helen: Probably 200 MeV.

Rene: Go back to slide 27. Can you actually show that you conserve the jet energy? Maybe that's exactly the same question. When you look at what's below 1 [GeV] and then you compare to what's above 1 on the right-hand plot can you actually show that the energy's conserved?

Helen: I haven't checked, and I'm not sure it has to be fully conserved yet. This guy [???] could go to an even wider radius.

Tom: This is very much like what I showed, except it's [R_{AA} is plotted] on a linear p_t plot. And what I showed is inherently energy conserving [in that application of the Borghini-Wiedemann prescription]. So, this is certainly consistent (to the extent you can see) with energy conservation within the fragmentation process.

Helen: Because if it moves out (lots more particles) let's say between [cone radius] 0.4 and 0.7. Then these guys could go outside, and I wouldn't have that these two integrals add up to the energy.

Rene: I thought what Tom showed was not inherently energy conserving *on the plot*. [Tom: It is by construction.] Maybe there's something outside the cone, and that makes it energy conserving. But it would be interesting to see what your energy balance is between what you're pushing down and what...

Mike: Why don't you integrate the energy in these plots?

Helen: We're getting there. I agree.

5. Anne Sickles

Thorsten: I have a comment which I made earlier about your [data] ridge and shoulders. Of course when we talk about Mach cones we illustrate this by this nice picture of a bullet or a boat in water, a shock wave and so on. But maybe the way to think about it is really you have somewhere

a perturbation in your medium. And if you have a collective medium that's going to respond in a characteristic way. And that doesn't care where the perturbation comes from. If the perturbation comes from some initial-state fluctuation it's there. If it comes from jet energy loss they end the same way and the response of the medium should not care. It should be universal.

Anne: But we should care where it comes from, is my point. A Mach cone is very different from an initial-state fluctuation.

Thorsten: No it's not. That's what I'm telling you. It's a perturbation. The medium reacts to it in a characteristic way. So, you expect that there is some universality. And the other thing is, of course you can argue that we have a ridge from the initial state and we have an away-side structure which is the reaction of the medium to the ridge. But you still have your trigger that still has some momentum which needs to be balanced on the away side. So, part of the away-side correlation structure must be correlated with the trigger, just by momentum conservation. It's just a question of disentangling.

Anne: I think we agree on that. I don't know if I agree with your first point. Before we go off on exotic calculations or ideas about whether this is due to some AdS/CFT neck region, we just have to be aware that these things are pretty easy to come by in a lot of other models.

Rene: I think we're getting to the point that we are equating ridges with cones. I think there's a real big difference between a ridge and a cone. All of us agree that there is an away-side structure that is balancing a same-side structure. But, claiming that you have a Mach cone...let's go back to where this came from. You can relate it to the speed of sound and make a really nice measurement of a phase property. That should be viewed much more critically than just saying I have a structure.

Anne: I agree with you on that.

Mike: One trouble with that is that the so-called Mach cone observed by PHENIX doesn't depend on centrality at all. That says the medium doesn't depend on centrality.

Rene: There are many problems with cones in both STAR and PHENIX.

Anne: I think we completely agree. My point is that before we go interpreting these as semi-exotic scenarios we should be aware of the more mundane scenarios that can give rise to these sorts of phenomena. As for the connection between the ridge and the shoulder I don't equate them with each other. I think that they share a lot of similar properties, and I think that's worth looking into as an experimentalist. And they appear to be similar to inclusive particles. We need to understand that.

Rene: To the point that Helen brought up, I think what we really see in STAR in R_{CP} is that the strange quark quenches like the up and down quarks. When you look at R_{AA} it probably doesn't, because of phase-space factors in p-p collisions. So, it would be really nice to see R_{CP} for the ϕ meson.

Anne: I agree. I don't think that's a plot that PHENIX has made.

Rene: Because, I wouldn't say that charm, bottom and strange R_{AA} ... they may be the same, but for completely different reasons.

Anne: Helen makes a valid point.

Mike: In general I don't like R_{CP} because I feel the peripheral is different and measures how fast the Cronin effect turns on. However, I do tend to agree with you, for the strangeness you might need this.

Yuri: Has anyone looked into fluctuations in the ridge? From the point of view of purely perturbative QCD there should be a ridge structure because of string fluctuations. But on an event-by-event basis it should be either left or right.

Anne: STAR has looked into this. They don't see any evidence for just being on the left or right. But the error bars are rather large. It's a tough measurement.

Jiangyong: There's a point I forgot to make in my talk. It looks like for the soft ridge you don't necessarily need a CGC fluctuation. Glauber has fluctuations. Any fluctuations tend to couple with radial flow, can produce these kinds of long-range correlations on the away side. The question is, can we distinguish using this feature based on strings, based on these two different geometries, or is it sensitive to these two geometries?

Anne: I think that's a really good question. I did show these two different models, and I think the physics that gives rise to these is different.

Rene: It is different, and if you talk to the people that push CGC they will tell you there is a difference. That's why we're waiting for this quantitative explanation from the Brazilian group or Klaus Werner [NexSphRio]. Because there's a big difference in the number of color ropes, if you want to take the Glauber picture, compared to the number of flux tubes in the CGC picture. If you believe that's the order parameter that you are sensitive to then the strength of the ridge would be very different in the two pictures.

Jiangyong: It depends on how radial flow magnifies.

Rene: But the radial flow is the same in both pictures.

Jiangyong: But it's a quantitative question compared to data. When I look at AMPT calculation it looks pretty big. The magnitude compared to the harmonic term you expect from flow seems almost comparable to data. It's not like they can't produce a large enhancement—large shoulder and large ridge.

6. Ahmed Hamed

Ahmed: If you look at this particular point, we had a hard time with p-p. You see that the uncertainty should come here, but actually it is behind the [??] here. But it is fixed. If you take just 1.3 times the uncertainty it is spanning the whole scale. But it is just one point. The other point, you can see is a factor 3 maximum uncertainty. Yes, it's still big. It can deliver a message to the community that there may be a difference, but it is not strong. It [the trend] [????]. It can be L , it can be \sqrt{L} , but it is not a strong dependence.

Thorsten: The expectation you formulated is a pen and paper thing. The expectation which comes out of the calculation when you take the ASW energy loss and Monte Carlo the π -hadron and the γ -hadron correlation is precisely what you measure. I have calculated it and it comes out they are on the same order of magnitude. And there is lots of complicated interplay which is not in your pen and paper estimate. I think in some paper I was even forced by a referee to investigate all this and comment on why this comes out almost the same, so you can read about it. So, the proper expectation is that if you put in the theory which has been known before into the hydro which you have fixed before they come out with the same order of magnitude. They are not precisely the same.

Ahmed: I am responding as an experimentalist. Why should I trust what is going on inside hydro? We don't understand everything inside hydro.

Thorsten: When you say that the community expected something that expectation is a pen and paper estimate. It is not a proper theory expectation in the sense that you would calculate something and run a simulation.

Rene: We also agreed that when you look at the PHENIX data in addition to the STAR data that you are not really looking at the correct fractional momentum range. You would really like to see this below 0.3. And there you can actually make a nice connection between PHENIX and STAR data. And then it looks pretty consistent.

Ahmed: I agree. But one of the points, this one with the expectation, you cannot [??] Xin-Nian Wang also because of the different energy-loss fluctuations, we should go up to 0.1 as many as from ... you are cutting the most important [parts of] the fragmentation functions.

Rene: I think we should really stop claiming that the proton/pion measurement has any sensitivity to the color factor.

Mike: I couldn't agree with you more.

7. Boris Kopeliovich

Thorsten: A comment. In your pion formation-length estimate as far as the parton shower was concerned which goes before you made the argument that it's possibly stronger than the string. I believe you could argue this if you ask for the formation length of say any pion formed in a jet. But this is not what R_{AA} measures. It measures the leading pion from a shower, so that's a very specific bias on the shower which you do not include. That's the problem with your estimate.

Boris: Of course I did. [Thorsten: No you did not.]

Yuri: His argument works only for direct pions.

Boris: What I used in my calculation is energy loss taking into account this bias. So, any gluon with a fractional momentum larger than $1 - z$ was forbidden.

Thorsten: I have not seen any equation showing me the bias.

Boris: It was implied.

8. Jan Rak

Mike: You showed this thing at LHC in 2005. I want to comment since Roy Schwitters is here, we had in 1983 a canceled ISABELLE with the promise of SSC in 1993. Now finally we have something that's almost as good. And that's great. Congratulations.

Boris: With this exponential fit for multiplicity it's probably as good as ϵ [??] fit...it's pretty standard. I wonder, ϵ equals what? [Jan: I don't know.] It's very interesting because Mueller-[??] theorem predicts 0.1.

Jan: If you look next Monday it will probably be on the [??]. It was not really the focus of my talk.

Lijuan: Mike, you said these data are not consistent with CDF.

Mike: No, it's not CDF. It's the one with Al Golshaw from Duke.

Lijuan: But the CDF is not as high as that one.

Mike: No, I'm talking about the multiplicity distribution.

Rene: But there was never a curve from CDF or D0 that had a bump. The only thing that had a bump was at that other experiment.

Mike: That's the one I'm talking about, which I was dubious about, because it was another [case] where a theorist with an interest was getting the data. Not that I don't collaborate with theorists, but don't collaborate with a theorist who has an interest in the outcome. [laughter]

Derek: We should warn experimentalists they should not have too much interest in their outcome either.

Boris: Why do we have points from ALICE, from CMS and nothing from ATLAS?

Rene: ATLAS came out with one a week ago, a paper.