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Closing discussion with brief arguments

Thomas A. Trainor*
CENPA 354290, University of Washington, Seattle, WA 98195 USA
E-mail: trainor@hausdorf.npl.washington.edu

This is a transcript of the closing discussion and several brief presentations responding to talks and discussions that occurred during the workshop. In some cases notes were added during participant review of the transcript (sources are specified). Participants were: Rene Bellwied, Yuri Dokshitzer, Ahmed Hamed, Rudy Hwa, Boris Kopeliovich, Guy Moore, Lanny Ray, Tom Trainor.

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*Secretary.

1. Comments on the Glauber model – Boris

If the bridge stands up and no one knows why one should nevertheless be cautious in walking on it. Same for the Glauber model. Glauber is just a model for elastic scattering, period. It provides no prediction for multiplicity. But now everything is called Glauber. Let's look at frequently-used formulas. Glauber model in p-A is derived and OK. Glauber model in A-A or A-B is ad hoc, has not been derived and is apparently wrong. Numerically, it may work just because of geometry. "Number of collisions" in p-A is fine. It depends what you are going to do with this number. In A-A it is wrong. [Yuri: even the p-A case may be wrong, due to fluctuations. Boris: question is covered in third transparency, is correct within the Glauber approximation]. Nucleons cannot collide N times. N-N collisions are inelastic (elastic cross section is miserably small), and nucleon breaks up in first collision. From then on only debris [partons] is involved. Glauber Monte Carlo is then nonsense because the model assumes multiple elastic N-N collisions, like classical billiard balls. "Number of participants" is especially ridiculous (no physical meaning), depends on what you do with it. Usually, it is used to measure centrality. What is legitimate is how to relate impact parameter to detected multiplicity (the theory is very difficult and not well developed). Models like HIJING are not serious. In terms of soft collisions there are at present no trustworthy tools. Number of participants is not a characteristic of any physical process.

Even the Glauber model itself is not correct. The Gribov correction to inelastic cross section is important: protons are quantum fluctuating objects. The physical cross section is an average over fluctuating Fock states. Basic problem is that the function of a mean value is not the same as the mean value of the function. The difference may be relatively large and has major consequences for the Glauber model. For instance, survival of nucleons [no dissociation] in p-A collisions. [description of algebra on the slide] Quantum fluctuations (Gribov correction) make the nuclear medium much more transparent than the Glauber model would predict. One should at least be aware of problems with the Glauber approach.

Ahmed: Does slide 3 mean that the d-Au experiments at RHIC do not really give us the correct handle on initial-state effects? I see from your first slide that the thickness function, the reference from d-Au, [may be compromised by fluctuation effects]. Then d-Au experiments are not enough for us in heavy ion collisions to obtain an estimate of initial-state effects. Is that correct?

Boris: I cannot say yes or no. I just want to say that if you are not interested in impact-parameter dependence or centrality of d-Au I think the Glauber model is fine. Although Glauber model is a success for total cross section, with 10% correction sometimes [the correction] it's important, [e.g.] for d-Au Cronin effect. The whole [Cronin] effect for pions is 10%. You normalize by total number of [N-N] collisions, which is subject to 10% Gribov correction. Thus, you can eliminate the whole Cronin effect by this correction. So, sometimes [Gribov, fluctuations] are 100% important. Otherwise, Glauber works fine for you in d-Au.

Ahmed: I need the total cross section for p-p (p-pbar) collisions. [With increasing CM energy it [the cross section] decreases and then increases.] Do you have a physical explanation for that?

Boris: Of course people have been working on this for years. It depends which approach you use. In Regge [theory] this falling part [on the slide] comes from q-qbar exchanges [Reggeons, hadron resonances]. Then gluonic exchanges give this rise [soft Pomerons, glueballs]. Even in Born approximation two-gluon exchange you already have the constant. And any higher-order correction makes it rising.

2. Comments on pQCD problems – Yuri

Regarding Boris's comments on fluctuations... It is really amazing to see how broad the distribution of cross sections is. The proton-proton cross section is some 40 mb. But that is an average. If you look at the real distribution over cross sections it's extremely broad and can actually be quantified. For instance from diffraction you could extract dispersion, etc.

So, I wanted to make a couple of remarks to what was discussed this morning in Guy's talk about infrared problems. The first general comment: It's not the first time we see that perturbation theory is very bad. But there should exist a remedy, because in principle, as a general philosophical statement, if a perturbative expansion is bad it only means you didn't do your work properly. Something has been overlooked or not properly understood, namely some physical phenomenon which is responsible for this misbehavior of your perturbative expansion. This expansion has to be reorganized, or some new variables have to be introduced. I'm just talking simple things. That's why my question to Guy should have been whether you thought about some improvement to perturbative expansions. [Guy: it's already been resummed.] You were blaming soft gluons [Guy: right] and specifically non-Abelian effects. You were telling us these badly-behaving corrections were just proportional to g [Guy: right]. So this is really very specific class of phenomena. If you cannot describe them order by order in perturbation theory [Guy: we can]. You may try, but it's a bad expansion. You have to reorganize it in some sort of new object and try to... some new resummed input, so to say.

Guy: For thermodynamic quantities how to reorganize is well understood, because you have Euclidean-space techniques. What you need to do is... in periodic Euclidean timing you need to integrate out higher Matsubara frequencies and get a 3-dimensional theory, and you know the 3D theory has a coupling with explicit engineering dimensions, and so there's an infrared scale where everything goes to pieces. But the non-perturbative physics on that scale can be dealt with by lattice techniques. But that strictly deals with the thermodynamics. Now you're asking about very long-time correlations [yes] on this theory, which depends on an analytic continuation which, since you're interested in long time scales, is very delicate. So, at this time it's not understood whether there is a good way to resum.

Yuri: So, there are no tools but one has to look in a new direction. Again, to Guy's talk I want to make one illustration to his point of infrared sensitivity of the perturbative formula. In the arXiv I found a beautiful paper on jet quenching written some ten years ago [laughter]. People spoke about the possibility of looking at the jet quenching as a shift in momentum. The guy is losing momentum and so therefore you lose the cross section. There is a way to parameterize the answer from QCD approach in terms of a shift. But this shift turns out to be very specific. It has nothing to do with mean energy losses. It's a very peculiar object which can be related to the number, to the integral of the multiplicity of gluons that are kinematically forbidden in this configuration, which can drive the cross section too far. But, jumping to the end it turns out to be some calculable function which depends on transverse momentum as a square root and has a predicted dependence on...[Guy: so what scale should α be? Yuri: this will be discussed in the next transparency.] This is an "infraredish" α scale, so for our estimate I just took a number –

0.5. This is certainly small scale, has nothing to do with parton energy. This is a naïve model of a static medium, which could be done more reliably numerically, but...what I wanted to stress is that instead of using this approximation let's look at this exact expression of this shift, given by this integral over the multiplicity of gluons with energy larger than given omega. What you see here...this integral is cut at large gluon energies. What enters is the integral from zero to some value which is p_t of the particle divided by the steepness [exponent of the cross section falloff]. The integral of multiplicity of gluons which are harder than this one. What's important is...this integral goes from zero. So formally it is calculable, infrared/collinear safe, etc. But nevertheless it is sensitive to the region of gluon energies in the range of arbitrarily small omega where we barely know anything. And so the question is: how the answer for your quenching factor... how it would depend on our ignorance of this region. What these pictures illustrate is what happens when you decide to cut away some soft part of your gluon phase space. This is the pure perturbative calculation (naïve). And then you decide to get rid of gluons with energies larger than 100, 300 and 500 MeV, and this is how your answer changes, which changes the effective rate of drop of the cross section. You see that it depends on what's happening in the infrared, whether the medium allows you to have gluons with such energies. You can have uncertainties of a factor five, in spite of the fact this is a formally calculable...[Guy: So you agree with me?] Yes. I just wanted to restate how much numerically this sensitivity is. [Guy: when you go to the largest p_t you plotted it's not so bad.] Yes, factor 2 instead of 5, sure. Nevertheless, for realistic p_t despite calculability it is still infrared very sensitive. That's the comment I wanted to make.

3. Responses to three comments – Tom

3.1 First slide (middle panel)

[Response to comments on the right panel of p. 28 of David's talk [$v_2(p_t)/p_t$ plotted on y_t], and the inferred particle source boost distribution.]

What's plotted in the middle panel [of slide 1] is a ratio of physical observables v_2 and p_t. v_2 is itself experimentally a ratio. In the numerator is an average of the single-particle distribution on azimuth weighted by $\cos(2\phi)$ relative to the reaction plane. In the denominator is the azimuth-averaged single-particle distribution. I've rewritten the v_2 numerator ($\equiv V_2$) as an integral over a hypothetical boost distribution $B(\Delta y_{t0})$. p_t' is p_t in the boost frame. p_t (unprimed) is in the lab frame. V_2 is the integral weighted by $\cos(2\phi)$. By hypothesis the $V_2(y_t)$ integral on boost includes a conditional soft spectrum (modeled by a Levy distribution) for a particular source boost value. If you insert the relativistically correct Cooper-Frye expression for the boosted soft spectrum into the V_2 integral and use a Taylor expansion you obtain factors including a "quadrupole spectrum" times p_t' (in the boost frame) which cancels with the factor $1/p_t'$ in the integral over boost, leaving the boosted quadrupole spectrum. The expression in curly brackets represents a universal function independent of mass, dependent only on source boost and plotted in the panel at lower left. For zero boost the function is just 1. That kinematic factor makes the v_2/p_t data turn over and go to zero at a specific y_t value. If that factor is removed from the data they follow a soft spectrum (Levy) shape on y_t, with a lower cutoff at some y_t which estimates the mean source boost ($\Delta y_{t0} \sim 0.6$ in this case). Given those results the data (with the kinematic factor removed) reveal a Levy distribution with a sharp edge (narrow boost distribution about a mean value). In general, I can calculate a boost distribution by inverting the integral equation to obtain $B(\Delta y_{t0})$. The better the data in the region of the turnover the more accurate the inferred boost distribution. We claim nothing about hydro details inside the nuclear system. We just want to infer an *effective* hadron source boost distribution from $v_2(p_t)$ data.

Note added (Tom): The inferred source boost in this case applies only to particles "carrying" the quadrupole component, which may be a small fraction of the final state.

3.2 Second slide

[Response to comments in Jiangyong's talk about possible jet contributions to $v_2(p_t)$.]

Jiangyong stated that hydro expansion contributes to $v_2(p_t)$ at lower p_t (say below 2 GeV/c), and jets contribute at higher p_t. The question arises, does that picture make sense? The points in the second slide are from David Kettler's v_2 analysis, extracted from model fits to 2D angular correlations. The model elements are a 2D Gaussian (interpreted as the same-side jet cone), an azimuth dipole (away-side jet peak, back-to-back partons) and an azimuth quadrupole (v_2, "elliptic flow"). $v_2\{2D\}$ is determined by the quadrupole amplitude $2v_2^2\{2D\}$ obtained from such fits. Do those data include a contribution from jets at higher p_t? We can go through the exercise described in slide 1 to extract the left panel on slide 2. $V_2(p_t)$ is just the measured $v_2(p_t)$ times single-particle spectrum $\rho_0(p_t, b)$. We can divide by p_t' (p_t in the boost frame, if we know the mean boost) to obtain the dashed curve in the left panel. Otherwise we can divide the v_2 data by measured p_t in the lab to obtain the dash-dotted curve in the left panel, which includes the kinematic factor p_t'/p_t. From that result we can infer a mean boost (~ 0.6) and use that value to remove the kinematic factor and obtain the dashed curve (Levy distribution), which describes the quadrupole data well. We find that $v_2\{2D\}$ data for all Au-Au centralities fall on the same universal curve, except for data below 0.5 GeV/c. The Levy distribution is very "cold"—$T = 0.09$ GeV. The $v_2(p_t)$ data follow the Levy distribution out to $p_t \sim 6$ GeV/c ($y_t = 4.5$). If there were a jet contribution to $v_2(p_t)$ we should expect significant deviation from the soft Levy at higher p_t (power-law tail?). But we don't see that out to 6 GeV/c. What we see at larger p_t is a competition between the tail of the soft quadrupole spectrum in the numerator of $v_2(p_t)$ and the tail of the single-particle distribution in the denominator. [Recall that the single-particle spectrum is strongly suppressed at larger p_t in central collisions, thereby increasing the v_2 ratio by the same suppression factor.] The ratio could continue smoothly to very large p_t (modulo statistics) and have nothing to do with jets, especially jets interacting with a medium of varying thickness. One further point: In the right panels the bold dashed curves [PRC 78, 034915 (2008)] represent viscous hydro predictions for $\eta/s = 0$ (upper curves) and $\eta/s = 0.16$ (lower curves) for the four most-central bins (0-5% $\rightarrow$ 20-30%). The $v2\{2D\}$ data fall increasingly below both viscous hydro predictions until for 0-5% the data are consistent with zero. If you take viscous-hydro theory as applied to nuclear collisions seriously those 2D v_2 data imply an infinitely viscous medium. You can debate the details of hydro theory, but you should also be aware that there are v_2 data which contradict any finite value of η/s in more-central Au-Au collisions.

3.3 Third slide

[Response to inference of radial flow from blast-wave fits to single-particle spectra.]

The upper panels show spectrum "hard components" for identified pions and protons obtained by defining a spectrum "soft component" as the limiting case of spectrum centrality dependence as the number of binary collisions goes to zero ($\nu \to 0$, the limit of no hard scattering, described by a Levy distribution) [IJMPE 17, 1499 (2008), 0710.4504]. For each hadron species the same fixed soft component is subtracted from all spectra to obtain the plotted hard-component centrality variation. r_{AA} is the ratio of the hard component for a given centrality to the p-p hard component. r_{AA} serves as an alternative to ratio R_{AA} which strongly suppresses jet contributions at smaller p_t. As centrality increases r_{AA} duplicates R_{AA} results for the legitimate physical suppression above $y_t = 4.5$ ($p_t \sim 6$ GeV/c). But at smaller p_t or y_t a large enhancement is apparent in r_{AA} which is artificially suppressed by R_{AA}. The enhancement at 0.5 GeV/c in r_{AA} follows exactly the physical suppression at 10 GeV/c. It's impossible for me to disregard the behavior at 0.5 GeV/c as independent of the experience of a fast parton in the A-A environment.

Yuri referred to an ansatz for parton energy loss in which the entire fragment distribution is shifted down [negative boost on y_t]. The dash-dotted lines in the upper panels represent just that process. The hard component (fragment distribution) for a given centrality is shifted down on y_t by an amount Δy_t representing a constant *fractional* energy loss. Shifts for several centralities are determined by the ratio data above 6 GeV/c (corresponding to R_{AA} "suppression"). For pions the data are quite close to the shift model, except the common intercept at unity is displaced slightly upward on y_t. For protons the situation is quantitatively different. There is a similar suppression at larger y_t, but things are different at smaller y_t. Nevertheless, at 2.5 GeV/c (pion $y_t \sim 3.5$) the centrality dependence of the proton enhancement closely follows (is anticorrelated with) suppression at 10 GeV/c. That structure is the origin of the baryon/meson anomaly. So, if you want to explain this [B/M anomaly] physically you have to keep in mind these relationships of the systematics for inferred fragment distributions.

Finally, radial flow inferred from blast-wave (BW) fits (lower panels). In the left panels the bold dash-dotted curves describe actual measured spectra for peripheral and p-p ($\nu = 1$) and central ($\nu = 6$) Au-Au collisions. The peripheral (and p-p) data are dominated by the soft component. The hard component (H_0) contributes a small fraction ($\sim 1\%$) of the yield. The bold solid curve is a BW fit to those data. To accommodate the data requires $\langle \beta_t \rangle \sim 0.25$ and $T \sim 0.145$ GeV as published for p-p collisions. In central Au-Au collisions the hard-component yield has increased by factor 30 or more and now includes about 1/3 of final-state particles. In the BW fit $\langle \beta_t \rangle$ increases to 0.6 and T decreases to 0.1 GeV, again consistent with published BW analysis. $\langle \beta_t \rangle$ and T are parameters which govern the shape of the bold solid curves. Does it make sense to interpret them physically according to the BW model? In this exercise the BW model function is mainly accommodating the fragment distribution (hard component) evolution with centrality. In the right panel are published $\langle \beta_t \rangle$ values from a STAR paper (solid and open points) plotted on centrality measure ν. The data are proportional to ν below $\nu = 2.4$ (for 200 GeV data). Then there is a transition to much smaller slope above that point. The transition points for 200 and 62 GeV Au-Au data correspond exactly to the sharp transitions in jet characteristics reported at this workshop by Duncan Prindle. A parameter conventionally associated with "radial flow" exactly matches the systematics of jet angular correlations and seems to result from the jet contribution to spectra (hard component).

Ahmed: I can't express enough the notion of hydrodynamics, that at RHIC there is a collectivity. Can you explain how you end up with your two-dimensional fit method...why this correlation, collectivity, going from peripheral to central [Tom: are you talking about the quadrupole?] Yes.

Tom: I stipulate that this [quadrupole] is a collective phenomenon extending over a large pseudorapidity interval.

Ahmed: Yes, but it has a hump like this at the middle centrality and smaller at peripheral and central [Tom: are you talking about jet or quadrupole correlations?] Both: add quadrupole and jets and get published STAR v_2 data, the centrality dependence.

Tom: The centrality dependence of the non-jet $v_2\{2D\}$ quadrupole follows binary-collision scaling and (optical) eccentricity [analysis by David Kettler]. The centrality dependence of jets has been presented here at the workshop (Duncan). David showed the implications for published STAR v_2 data, which do include a strong jet contribution (“nonflow”).

Note added (Tom): Centrality and energy systematics for p_t-integral $v_2\{2D\}$ are reported in D. Kettler (STAR collaboration), Eur. Phys. J. C **62**, 175 (2009).

4. Challenge to minijet model – Rene

The ratio of protons to pions or Λs to kaons is a factor three different in the bulk medium than in minimum-bias [in vacuum?] jets. One possible explanation is the recombination of thermal partons. Another is uniform jet quenching for partons. But then how do you get one hadron species to pile up over another? One possibility is to push the protons out to the point that they are a factor three larger than the pions. To do that you need thermal expansion. You can get this picture [baryon/meson anomaly] by having thermal expansion from the lower side and jet quenching from the upper side. Or you can get it from recombination. But from a simple superposition of unmodified minimum-bias jets you will not get it. So, this is a challenge. If you want to describe this [baryon/meson anomaly] with modified minimum-bias jets then the question is what kind of modification do you need to get that factor three. Furthermore, if you want to apply this to the [triggered] jet/ridge problem (you could say there’s a jet in the middle and ridge on the sides) you make a B/M measurement in ridge and jet parts. The jet part follows inclusive “minimum-bias” [in vacuum?] jets. The ridge part follows the “medium” behavior [B/M anomaly]. The away-side B/M “ridge” value also matches the "jet" [in vacuum?] trend. Measurements of the p_t dependence of B/M for “jet” and “ridge” parts compared to inclusive spectra follow the same trends (with limited statistics and below 2 GeV/c).

Note added (Tom): We have at present no particle-identified correlation data for minimum-bias jets. Such data should soon be available given the recent commissioning of the STAR time-of-flight barrel.

With the “soft ridge” [minimum-bias jet structure with η elongation] for Cu-Cu with varying lower-p_t cutoff, the width [variation with p_t] becomes at some point flat. If you compare this with an initial-condition-plus-thermal-expansion model it doesn’t describe the p_t dependence so well. It’s too low. So maybe that model is not enough and there is a contribution from jets or minijets in there, but the challenge is to compare this type of distribution with the minijet picture and see whether the minijet picture describes that.

Lanny: We need some particle-identified correlation data right where the minijets are sitting – pions, kaons, protons.

Rene: One point is hadrochemistry, the other is kinematics. Kinematics of the minijets are different from the kinematics of the medium. There's a different p_t distribution.

Tom: I think you may be making a distinction without a difference. You are talking about a [jet] peak and [ridge] background and decomposing the same-side peak structure that way. I believe that is not a legitimate separation.

Rene: I am talking about a p_t spectrum, from 0 - 20 GeV/c if you want. And you have a different p_t spectrum for minijet pions, kaons and protons than for medium pions, kaons and protons.

Note added (Tom): There are no particle-identified angular correlation data at this time. We do not know for instance what is the p_t spectrum for protons in minimum-bias jets (minijets), or how protons from jets are distributed on angle difference.

Tom: I think I just showed otherwise [with spectrum hard components on slide 3].

Rene: No, what you showed is that if you move it into y_t space where you essentially ignore the radial expansion that you can map them on top of each other.

Tom: That analysis on y_t was intended to *find* radial expansion. [Rene: and you did] No! [Rene: the boost] No! The boost [referred to in previous discussion] applies only to the quadrupole component [which is not apparent in single-particle spectra]. For azimuth-averaged single-particle spectra there is no evidence for radial flow [IJMPE 17, 1499 (2008), 0710.4504].

Rene: I just showed on a linear scale a factor three difference

Tom: That agrees with the spectrum hard components that I presented.

Rene: You showed the baryon anomaly [in the spectrum hard component, slide three]. What pushes the protons out further in y_t than the pions?

Tom: Why do you think the protons are "pushed out."

Rene: Because they show up at a different y_t.

Tom: That [proton spectrum hard component in slide 3] looks to me like a change in the fragmentation process. It's a variation on the pion evolution where there is suppression at larger p_t and enhancement at smaller p_t.

Rene: Right, but the whole thing is at different y_t.

Tom: That's correct. Somehow in the fragmentation to protons the FF modification process "hangs up" at $p_t \sim 2.5$ GeV/c. Below that point there is *no* variation with centrality in proton H_{AA}, below pion $y_t \sim 2.7$ ($p_t \sim 1$ GeV/c). That in itself is extremely interesting: With a hydrodynamic "push out" you would expect the largest effect to be there [at smaller y_t], but the variation [change in the hard component] is zero! It's a very anomalous situation relative to blast-wave expectations.

Rene: What you call a "hang up" is thermal expansion. It pushes the thing out to higher p_t.

Tom: What you are missing from Gavin et al. is a detailed centrality dependence, and that's key. Because what I showed you is that the baryon anomaly [peak near $p_t = 2.5$ GeV/c] follows exactly the sharp transition that appears at 10 GeV/c [and dominates jet correlations]. The proton centrality evolution at 2.5 GeV/c and at 10 GeV/c are perfectly (anti)correlated.

Rene: Gavin published a centrality dependence on the basis of Estruct data, and the sharp transition was within his error bars.

Lanny: Sean [Gavin] was comparing to p_t [not number] correlations which do not show a sharp transition. As far as I know he has not looked at number angular correlations. We only see the sharp transition in the latter case.

Tom: I encourage you to look at the single-particle spectrum paper published in 2008 with the two-component analysis [IJMPE 17, 1499 (2008), 0710.4504, plots from Figs. 9, 10 shown as upper panels on slide three] with the detailed centrality dependence of the proton anomaly and fold that into any explanation you have for the B/M anomaly.

Rene: Then draw a line through the p/pi ratio data as a function of p_t, not y_t.

Tom: It's just the ratio of the two [proton and pion] fragment distributions [the spectrum hard components] on slide three.

Note added (Tom): The two-component spectrum descriptions in [IJMPE 17, 1499 (2008), 0710.4504] accurately describe published proton/pion ratios, as in Fig. 13 of that paper.

5. Comments on ridge structure in the parton model - Rudy

From PHOBOS data we see the ridge extending to $\Delta\eta \sim 4$, so we see apparently "long-range" correlations. The trigger, let's call it at $\eta = -1.5$ (I just turn it around), and the ridge goes out to 2.5, so the difference is 4. Let's consider $\eta \sim 3$ (even larger than the edge of ridge). This is pseudorapidity. The corresponding value of polar angle is around 0.1. It's not space-time rapidity, it's pseudorapidity. So, this tangent of polar angle is the ratio p_t/p_z. Let's say $p_t \sim 0.4$ GeV/c. That means $p_z \sim 4$ GeV/c. If a hadron is at 4 GeV/c the contributing partons will be even less than that, say 2 GeV/c. That means x... [Boris: why is that true] ...two quarks recombine.

Note added (Boris): A hadron from parton fragmentation may carry only a fraction of the parton momentum, so the parton momentum should be larger than 4 GeV/c. Rudy may reply that in the recombination model two partons, each with 2 GeV/c, combine to form a 4 GeV/c hadron.

That means $x \sim 0.02$. The gamma factor is not 100. In the early days Bjorken was talking about contraction of leading partons with gamma factor large — until you get into the wee region where Δz gets fatter and fatter, and there's always an uncertainty (in the wee region) of about 1 fm in pp collisions. Now in nuclear collisions, we're talking about an even larger region, larger than 1 fm, which corresponds to a diamond at the tip of the light cone. So you look at (this is longitudinal, this is transverse) a region where there is confusion (what Feynman talked about), partons on the right going left and partons on the left going right. There could be ones going this way or that way. It is not Hubble expansion at early times. And you could have a hard parton going up — they talk. Thorsten was worried about how you can have so much energy deposited in the forward direction in the ridge due to energy loss of a jet. That has to do with the forward-going partons; they carry the forward momenta whether or not they are perturbed by the high k_T parton. Actually, it doesn't have to go very far, only about this much. This is already able to give you η of 2-3. So, as the high k_T partons that come through the medium and they interact with the forward-going partons, an enhancement of the transverse momenta of the $\eta \sim$ 2-3 partons can lead to the ridge measured in correlation. Basically, there are two issues. One is early-time interaction, which occurs in this diamond in this picture. The other is how these hadronize downstream. And those are due to forward-going partons. And the two pictures can both be satisfied in this simple parton picture. We can work out the details, but there's no big deal. And certainly there's not long-range correlations. And I don't agree with the CGC's naïve picture of two receding disks with flux tubes in between. These are not infinitely contracted disks going apart and giving you flux tubes. I don't see much of an infinitely-contracted disk, since the gamma factor of the valence quarks is not infinitely large. There is overlap in the beginning where partons can talk to each other.

Yuri: I think there is no contradiction, because the disk is just "leaders." We forget about them.

Rudy: They want to put the valence quarks in the disk, so they pull the gluon strings in the middle. Even in the best circumstances a valence quark has 1/3 of the [nucleon] momentum. So, the range of gamma factor is reduced accordingly. Actually, in the parton picture half the momentum is taken by the gluons. So, the range could actually be even less. So, the gamma factor is not 100, maybe 20.

Boris: You don't need much gamma factor to create tubes.

Yuri: ...because the tubes are the image of the wee partons (different language to say the same thing).

Rene: Do you have a problem with the flux tubes?

Rudy: I have a problem with the dual-parton model from the very beginning.

Rene: Then take strings...

Rudy: I complained to Alphonse Capella a long time ago. In p-p collisions you have a diquark and held-back quark. And you are in vacuum, therefore you pull a color string (according to confinement). But in the nucleus situation how many forward-going partons are there? Quite a lot. There's a whole lot of color charges. I don't see where you can have any time for color flux tubes to be pulled, for a string to be pulled. There are color charges in between to short out the color field. So, I don't see how in a nucleus-nucleus collision you can develop color flux tubes in the way the dual-parton model tries to describe them.

Rene: So you are saying you may have strings but the strings melt.

Rudy: The strings never get going.

Ahmed: There's a result from 2+1 [trigger+associated analysis] where [with two back-to-back high-p_t] triggers we don't see the ridge [in the associated particles].

Rudy: I'm not sure about what you are saying. There's the "hard trigger" ridge and the "without trigger" ridge [minimum-bias same-side jet correlations with η elongation].

Tom: Thank you for probing these early-time issues. I wouldn't go so far as point 4 [on the slide], because PHOBOS used a hit detector. What's detected is mainly very soft particles. So the large η values are even less remarkable given your argument.

Rene: Anne Sickles showed the η to y mapping of the PHOBOS detector. It peaks at 1 GeV.

Tom: They don't know what the p_t is. You can guess what the mean p_t is, but you don't measure what the hits actually are.

Rene: But you know exactly what the p_t distribution is from the other measurements. You know where the hit detector sits and what it's p_t coverage is.

Rudy: Coverage from 35 MeV/c up.

Rene: They integrate over that, but you know what the p_t distribution of the pions is, right?

Tom: No, I don't.

Rene: I do, measured by 5 or 6 different experiments.

Note added (Tom): What is at issue for Rudy's presentation is not the mean p_t of all final-state particles (which is indeed about 0.4 GeV/c at mid-rapidity in central Au-Au collisions) but only of those particles included in "jet/ridge" correlations at large η difference. The mean p_t of those jet-related hadrons may be significantly less because of “medium modifications” and is not measured.