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Wednesday discussion: Coalescence vs fragmentation: hadronization mechanisms and their effects on the observed final state

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This is a transcript of Wednesday evening's discussion session. The day's talks had focussed on experimental results on particle production in p-p and Au-Au collisions, and the theoretical attempts to explain these results via fragmentation and coalescence. The participants in the discussion were: Rene Bellwied, Helen Caines, Charles Chiu, Yuri Dokshitzer, Rudy Hwa, Jiangyong Jia, Boris Kopeliovich, Denes Molnar, Guy Moore, Lanny Ray, Thorsten Renk, Lijuan Ruan, Edward Shuryak, Anne Sickles, Mike Tannenbaum, Tom Trainor.

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1. Beginning Comments

Lanny: We've had about thirty topics deferred to this discussion session. So, we would be here until midnight getting through those. [laughter] In order to remain productive throughout the rest of the workshop I suggest we don't do that, but discuss a few of the most relevant topics over the next 45 minutes. One of the main issues was the role of fragmentation in nuclear collisions.

2. Accuracy of identified-particle fragmentation functions

Tom: What a number of people complained about is deficiencies in theoretical [fragmentation function (FF) descriptions] like AKK fragmentation functions in terms of identified particles. Now, is that inherently a problem for fragmentation descriptions, or is it a problem for the theoretical *fitting* procedures, these global fits which may be based on the wrong premise of [FF] universality. Of course the situation is better for unidentified hadrons because there are more data available. The identified-hadron FFs are quite delicate in term of systematic errors. So it's a question: should we abandon fragmentation descriptions because of the identified-particle problems with spectra, or is it really that the fragmentation description is not properly implemented yet because of systematic errors.

Edward: It was Boris said... it was said that all particles except pions are formed inside the matter. And then Boris has said that even pions form inside the matter. So after that we don't have any case for fragmentation.

Mike: But that can't be true because at the AGS we obviously saw that there were no cascades inside nuclear matter. When you made a pion you didn't cascade inside. People said that there would be huge showers. Like, if you shot an electron into a lead brick you'd get a huge shower. This does not happen. That means the pions are born outside the medium.

Boris: If we are going to talk about data, this was tested in DIS, semi-inclusive. And it turns out that the energy loss scenario fails. There's a recent paper out by Xin-Nian Wang where he says that apparently one should add inside the medium production. There are many other pieces of evidence: measurements by HERMES on broadening and so forth. In DIS it is apparent that a significant fraction of the pion production is inside the nucleus. And if you take a more dense medium then the production time cannot become longer. It can only get shorter because energy loss increases.

Thorsten: Why do you argue this, that a high-p_t, say 10 GeV, pion is born inside the medium. Based on what equations/physics would you say that.

Boris: It is just energy conservation. You calculate the energy loss and you see that the main fraction of the energy is lost very quickly, in 1-2 fm.

Thorsten: Why?

Boris: Just do the calculations.

Thorsten: I did, and it ramps up because you [have induced???] radiation.

Boris: I don't mean induced radiation. I mean vacuum radiation. You produce a quark with extremely high virtuality which is the same as its energy. So, it radiates like crazy and cannot last a long time.

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Thorsten: Yes, but a 10 GeV pion is likely to be formed in a process in which there was little radiation, just because you want to pick the hard part of the fragmentation....

Boris: Little amounts of radiation is suppressed by Sudakov suppression. If you don't allow the quark to radiate something you immediately get Sudakov suppression. This is another source of suppression. Instead of energy loss you have Sudakov.

Thorsten: So, you try to balance wanting to have high-Z fragmentation against the Sudakov [suppression]. Empirically I dug this number out, I think from PYTHIA, in an event in which you trigger on say a 10 GeV pion that carries about 70% of the original parton energy. So that did not lose much energy.

Boris: I don't know anything about how PYTHIA is programmed. But we did calculations, and we found that as a function of jet energy the production time does not rise. It is more or less constant, maybe even going down a little. So the Lorentz factor that should be proportional to energy doesn't work, because the virtuality increases.

Thorsten: But I still don't understand the argument. Of course you radiate 30% of your energy very early on because you usually start with a high virtuality and cascade down. And you do your vacuum radiation, and that goes somewhere into the medium. Then you have a leading quark which carries about 70% of the energy of your original highly-virtual quark. That comes out, and then you form a pion. So why is the argument changed by this 30% energy.

Boris: If you calculate vacuum energy loss taking into account that no gluons with more than 30% of the energy should be radiated then you have to multiply by Sudakov suppression, and I can show the result.

Thorsten: I can show you mine, and it's apparently different.

Rene: I think for the pions, the HERMES data for pions, if you go to the heavier particles I agree with you. But for the pions I don't agree.

Boris: What do you mean?

Rene: For example, the p_t broadening [HBT systematics???] doesn't show a surface that is smaller than the nuclear surface. So I think they could very well fragment outside the medium. But I don't see why that pertains to the discussion because this is all consistent with fragmentation. This is the fragmentation picture. We talk about whether it [a hadron] forms inside or outside the medium. But if you just want to have a quantitative description of the FF then that is still valid for all of these particles, whether they're formed inside or outside.

Edward: But the whole argument of fragmentation was that it would not be touched by the medium or anything.

Thorsten: If you are inside the medium then you have completely different dynamics

Rene: Well that's medium modification of the FF. Then the question is how you...

Edward: That invalidates the predictive power, because you have a new unknown function.

Boris: This is what Xin-Nian did in his last paper. He introduced a modification of the FF, modifying the splitting functions. Then he did a calculation with no free parameters except a transverse coefficient he was playing with. And he completely failed to describe the data at large z. Above $z = 0.5$ his calculations go much below the data. The energy-loss scenario with fragmentation outside doesn't work.

Thorsten: It doesnŠt work for what process?

Boris: For pion production from nuclei.

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Denes: I thought the question was even worse. I thought the first two talks of today showed we didn't understand the production even in p-p.

Lijuan: We compared to STAR's p-p results [spectra] to AKK and KKP FF up to 15 GeV/c, and we see a discrepancy.

Tom: Above 2 GeV/c though.

Denes: The ratios aren't right.

Lijuan: At 14 GeV/c you see a discrepancy.

Tom: I would argue you are not testing anything unless you go below 2 GeV/c.

Mike: But there the calculations go wrong anyway.

Thorsten: I would argue that you should start at high p_t

Tom: Why is it the opposite?

Edward: Because the argument was that at large p_t fragmentation is supposed to work. And now they say no, even at large p_t, even for pions it is wrong.

Mike: It's not fragmentation that fails. It's the guys who did the calculation that fail. So, if somebody asks me whether I believe in QCD (it was Kari Eskola) I say yes I believe in QCD, but I don't believe in many of the people who do calculations and claim it's QCD.

Anne: We have to remember that the FFs are not well constrained. They are not shown with error bars.

Mike: But I actually looked at this LEP data and they had a lot of anti-protons coming out of what they claimed to be the gluon jet.

Anne: Fine, but FFs are not defined with no uncertainties.

Rene: You have a real problem making baryons in fragmentation. For the pions it works, but as soon as you go to the protons there is a problem. And you have a problem apparently even at NLO. I think that's a fundamental problem that has nothing to do with the p_t range. It doesn't work at any p_t. Bourrely and Soffer did a lot of work on baryons, but that is a completely different approach.

Boris: For baryons the mechanism of production is probably quite different. It's string junction-anti-junction which you have to believe you can make from gluons. There are data that show you make them from gluons, not from di-quarks and quarks, at high energy and mid-rapidity.

Edward: This is related to baryons specifically. Before we discussed just pions.

Tom: If we look at the momentum distribution of unidentified-hadron FFs we have something in e^+-e^-. Then we see this is modified in A-A. So, by a simple prescription it seems that we can describe the modified FF momentum distribution. By the same token we should look at the flavor dependence of FFs. Yes, we can't bring e^+-e^- FFs into A-A and get the right flavor dependence. Is there a simple modification of the flavor dependence that could accommodate the A-A observations? I think if you look at the hard components in Au-Au spectra for more-central collisions the actual modification of the proton part is quite simple and is quite analogous to the modification of the pion part, except that the low end is cut off higher for protons compared to pions and it stacks up higher [in p_t]. That's what is giving the baryon-to-meson excess [at intermediate p_t]. So I would say look at the FFs and a simple modification a la Borghini-Wiedemann.

Helen: But I don't think the FF for protons is even right. So before I modify the FF I need to get it to explain my p-p protons.

Tom: You're talking about the theoretical or the measured FF?

Helen. Under the assumption that the STAR measurements of the proton spectra are correct [spectra calculated theoretically from] the fragmentation functions don't pass through the data.

Tom: No, the measurement of the FF in e^+-e^-.

Helen: People are saying that's well measured and constrained, but I don't think that's true. They might be well measured, but I'm not sure they're included in the [theoretical FF] fitting. The gluon FFs are not well constrained.

Mike: Right. It's in gluon jets, that's the problem.

Helen: And it is knowing the glue vs the quark that is important

Boris: I can add about the data: There are data at low energies, Serpukhov measurements at 70 GeV fixed target p-p collisions. There are more protons with high p_t than pions, just in p-p collisions.

Mike: That's because they're not in the hard-scattering regime. It's like at the AGS all kinds of crazy things happened. Tell me about antiprotons. Then I'll believe you.

Rene: Well, now you are talking about stopping effects.

Mike: No, I'm not talking about that. If you're looking at RHIC and you go to 4 GeV/c what's the pbar/p ratio? It's one.

Rene: That's the beginning of the hard scattering regime.

Tom: But is this event-wise identified jets?

Mike: They don't have jets at 70 GeV fixed target

Boris: This is single-particle inclusive p-p to proton or pion at high p_t. You expect more pions than protons. The data show the opposite—more protons than pions.

Tom: That doesn't tell me what the FF is.

Mike: If you do $\bar{p}/\pi$ then I'll listen to you.

Rene: The p/π ratio from CDF at 1.8 TeV is not described by PYTHIA at all.

Mike: But PYTHIA isn't QCD

Rene: Well, I'm just taking a LO mechanism. Even if I turn on NLO corrections you don't get close to the measurements. So, there is a real problem with baryon production, even at 1.8 TeV [FNAL] where you are definitely jet dominated.

Denes: Wasn't even the K/π ratio [in FFs] a factor 2 off from measurement?

Rene: Yes, that too. Essentially none of the FFs except pions work.

Anne: But if the FFs aren't well known you shouldn't expect to get the right answer.

3. Is FF universality valid across different collision systems?

Helen: The [theoretical] FFs are essentially just parameterizations of the data.

Note added by Tom: The question is, what data? If theoretical FF parametrizations (based on DGLAP evolution) are [based on universality arguments] derived from global fits to FF data from several different physical systems (e-e, p-p, DIS) which are in fact incompatible, the resulting parametrizations may not apply to any single system.

Mike: So what do the particle ratios do as a function of p_t and $\sqrt{s}$? We're going to have nice data soon from the LHC. What's the p/π ratio in p-p collisions at p_t of 5 GeV/c say? What's the K/π ratio, at LHC, compared to RHIC, to ISR? We want to look at identified particles in p-p, not p-pbar, because in p-pbar most of the interesting physics gets wiped out [and is not comparable with A-A collisions]. You want to know how does experiment describe them [ratios] and how does theory describe them. They're two different questions.

Rene: Well, I thought the question was whether FFs, at least in p-p, can be defined in such a way that they can be used as quantitative descriptions.

Mike: FFs don't come from p-p.

Rene: So, you say there's no universality?

Mike: No, I'm not saying that. I'm saying they're universal. You just don't get them. What FF did anybody get from p-p? You tell me.

Rene: You say I cannot apply e^+-e^- [FFs] to p-p?

Mike: Well, I don't know. In principle CDF could trigger on a proton and measure the fragmentation function.

Tom: CDF measured FFs in general which, already for unidentified hadrons, are very different from what you get in e^+-e^-.

Edward: What does it mean? It means that the whole thing is fake, or what?

Tom: No. It means there are environmental issues already in p-p.

Yuri: We can conclude this discussion by simply saying that fragmentation functions just don't exist – in a very simple sense. What is being measured in e^+-e^- is inclusive spectra for identified hadrons in e^+-e^- annihilation. Then a theorist tells you (for instance) this is 2 times quark into hadron fragmentation, this is a theoretical construction, not in nature.

Boris: We believed you [Yuri's extensive history with FFs]... [much laughter]

Denes: Perhaps this is [a measure of] uncertainty in the FFs.

Yuri: Then we have to start discussing what we mean by [FFs].

Denes: Perhaps one could try to adjust them to reproduce RHIC p-p data properly, then go back to the e^+-e^- data and see if we screwed them up. And if not maybe you could use this revised set as a baseline.

Yuri: In principle this is possible. Then we could quantify universality or not. But before doing that [and I insist and repeat] you have to remember that hadron-hadron collisions and e^+-e^- and DIS are completely different processes. The first being totally and violently biased. Before we start to apply the idea of a quark fragmenting into hadrons, by definition it cannot be true because somebody has to participate. A colored fractional object cannot give you just one hadron without the rest of the world participating to some extent. Before we attempt to quantify this discrepancy for good fits we first have to understand how and where we have to measure FFs in e^+-e^- to then transfer to p-p collisions.

Thorsten: So are you saying that fragmentation is not universal and that QCD factorization is not a good idea?

Yuri: No. First we have to determine what we are talking about. So when Tom says fragmentation is not universal, what does he really mean by fragmentation? If by fragmentation by definition I mean what happens to a quark in the vacuum producing a K^+ with 0.65 momentum fraction this is one thing. If we apply the word "fragmentation" to *any* environment this is another thing.

Edward: We mean [by universality] that a quark jet in e^+-e^- and p-p collisions with the same hardness, same p_t and Q etc., would be the same.

Yuri: If there is a medium, like in heavy-ions...

Many voices: No medium!

Yuri: Well in principle a proton is also a medium.

Thorsten: Can you write the process as the product of the initial state times the hard process times the fragmentation?

Yuri: The answer is yes, if formally you take very high energy and extremely high transverse momentum you can prove that up to power corrections things have to factorize, full stop. And then comes real life: p_ts are not so huge, the corrections may be very large, and then there can be some competing mechanisms, like production of hadrons from very small distances because you don't want to muddle through whatever medium is there.

Edward: Power corrections, in case some do not know, are like coalescence but at time zero. At the collision moment there is a scale Q already with a larger scale. That I think is very small.

Yuri: It should be very small. But since we're talking about very small processes anyway it's a question of what stands in the numerator.

Edward: This I think would be the leading twist...

Yuri: Certainly not. If you look at the Tevatron search for QCD, they really start seeing perturbative QCD working for jets only from 50-70 GeV [jets]. It's a very long process. The spectrum is not p_t^4. It [the exponent] is rather 10 or 12, because there are many effects which come together and drive you very far from....

Edward: But even including... if you take K/π, would it be the same [in e^+-e^- and p-p]?

Yuri: Yes, by definition. If you are talking about [vacuum] fragmentation of a ["bare"] quark or a gluon or whatever its fragmentation has to depend on nothing. It has to be universal by definition. If you say non-universality you have to think what are the mechanisms to disobey.

Edward: So, when Tom says non-universality...

Note added by Tom: The intended reference is not to ideal in-vacuum parton-to-hadron fragmentation [dijets in e^+-e^- collisions] but to extended collision systems which may then affect the parton fragmentation process (e.g. p-p, DIS).

Rudy: When you say the Field-Feynman [model of fragmentation] is not correct, did you mean that?

Yuri: No. I said that, but I meant only one thing. Field-Feynman fragmentation is also universal, but their fragmentation was purely non-perturbative, a plateau distribution of hadrons full stop. What I am saying now is that we know from experiment that there is additional physics which comes from small space-time distances which gives additional gluon radiation which is also universal. All these things taken together give you again a universal final-state distribution, provided you measure it in an un-biased way. If you insure that you have a hard process by measuring a kaon in the final state then you have to renormalize all the [relevant] contributions which came in.

Thorsten: So your statement is that the apparent discrepancy between FFs measured in e^+-e^- and those applied to p-p has to do with power corrections and other processes?

Yuri: No. I think it's just a misunderstanding. My first suspicion is not wrong data but theoretical knowledge. For example for many years we were told that pQCD does not work for heavy quark production. Why? Because people drew the FF for heavy quark into heavy meson—which had a very nice shape, momentum conservation was OK. But they forgot one thing: When you are measuring 50-100 GeV B mesons at the Tevatron you are sensitive to the region of z which is 0.95. So you don't have to fit the whole curve, only the small tip where the cross-section is very very small. This is an example of a bias effect. This is a pure theoretical misunderstanding of the bias in the data analysis.

Guy: I wonder if the same bias effect arises in the data we were talking about. Because it was triggered on a high-momentum hadron rather than on a high-momentum jet.

Yuri: Absolutely.

Edward: But it is not 0.99, it's like 0.7. [typical RHIC measurements]

Yuri: It depends on how fast the cross section falls.

Denes: Doesn't this just mean that you have larger errors in that [large-z] region because your e^+-e^- measurements could have an incomplete error estimate?

Edward: It [the problem] is not in the [FF] concept.

4. Is fragmentation measurable or even valid at smaller p_t?

Rene: If you go back to what Tom presented. You look at this and you say that the FF is already wrong in describing the high-momentum particles in fragmentation, it gets more and more difficult the further I go down in momentum, because I get more and more contributions from non-fragmentation into the spectrum. So this extrapolation of the fragmentation into the very low p_t region is impossible. Because what you do is essentially you have additional contributions like multi-parton scattering in the initial state which you are not even taking into account which only populates the low-p_t part and not the high-p_t part. So if I can't trust my FF in the high p_t part I certainly can't trust it in the low p_t part.

Note added by Tom: That reasoning is fallacious because of failure to maintain several distinctions: 1) theoretical vs measured FFs, 2) Fragment distributions (FDs) vs complete (soft+hard) spectra, 3) Theoretical vs measured FDs. 4) Whether a process is "taken into account" in a theoretical description as opposed to whether it manifests in a physical measurement. Whether theory can *describe* (or fit) the high-p_t part of an FF may have nothing to do with how well the low-p_t part of an FF can be *measured*. Non-fragmentation contributions to spectra (are there any? what is the proof?) may have nothing to do with accurate comparisons of theoretical and experimental FDs.

Yuri: There is another complication here about what Tom was telling us. When you say FFs in e^+-e^- you measure, as he said, all particles everywhere at fixed total [parton] energy, and you have 4π in angle. You want to talk about jets [as in p-p] which have some size [angular cone radius]. Because small-p_t parts [of individual jets] will actually belong not to a single jet, they belong to the whole ensemble, the inter-jet particles. You don't want to mess with them because they don't belong to any particular parton. They're collective.

Edward: So in principle you could go back to e^+-e^-, make a cone of the same angle, do exactly the same analysis and find exactly the same thing [incomplete or truncated FF].

Yuri: Yes, some people did that.

Jiangyong: Can I ask a theoretical question? Suppose you have e^+-e^- and you have a very hard gluon which produces hadrons. Can one use that to define a gluon FF? Would that FF in principle also work for A-A collisions if the gluon itself is hard?

Thorsten: That's just how they're [gluon FFs] measured.

Tom: That's what is actually done.

Jiangyong: That part seems to be correct. [Boris: modulo medium modifications, whatever...] Right, so therefore simply it is a matter of how one can figure out how much pQCD can calculate, it is not even a matter of whether you choose one or the other. Rather, both processes are there. You have a pQCD calculation which gives you a certain answer. But that's not sufficient. You have recombination etc. It seems both should be taken into account.

Yuri: There is an empirical finding, that if you calculate naïvely production of gluons, or the gluon field in a gluon jet—What is gluon jet? – you make some graviton, you make it annihilate into gluon-gluon (just calculate, don't think about the experiment)—you get some hyper-geometric function, which Tom was showing today also [the beta distribution], and then you compare it with the spectrum of pions in a gluon jet. Experimentalists can extract gluon jets from 3-jet events. And for some reason which we don't understand well they—the measured [hadron] spectra and the naïve calculation—are mathematically similar [LPHD - local parton-hadron duality].

Charles: In our discussions I heard it said that once you have pbars then you are going to have trouble [Mike], but then in pQCD you can still calculate how much is the pbar contribution to the FF.

Yuri: I can't calculate them. I can only predict from first principles that at large enough momentum scales all the *ratios* should be universal. I cannot tell you [absolutely] how many antiprotons there will be. I can't predict what the $\bar{p}/\pi$ ratio should be. I can only tell you that it should be a constant [for a given Q^2???].

Tom: Returning to a comment from Mike: We should distinguish between measured FFs, which are in a sense all I'm interested in, and the perturbative attempt to describe FFs. Those are different problems. They're both interesting, but in trying to describe A-A collisions in terms of FFs I only want the [FF] measurements. If I take the measurements from e^+-e^-, as Yuri was saying, I have to include the caveat that what I'm measuring in e^+-e^- is dipole radiation from a color dipole in vacuum over 4π. In a p-p collision, if the [colliding] protons have exchanged a color singlet (a hard Pomeron) then the jets that are coming out at mid-rapidity are not color connected. What are they color connected to? They're color connected to "holes" in the nucleon projectiles. In effect, I have a 4-jet process. The jet I am looking at out one side is not expected to continue across, as in e^+-e^- collisions, to the [momentum] partner. The *momentum* partner is different from the *color* partner. So, that a priori breaks universality, right there. It's a totally different color-field geometry [from e^+-e^-].

Note added by Mike: The production of particles at large p_t >2-4 GeV/c in p-p (and presumably A+A) collisions which exhibits a power law p_t spectrum is due (in LO) to 2-2 partonic subprocesses involving the exchange of a gluon e.g. q+q → q+q. The two outgoing high p_t quarks are color-connected by the gluon. Exchange of a color singlet pomeron is only for soft processes, the exponential spectrum at low p_t , and is not relevant for the large p_t power law processes.

Note added by Tom: In his talk Yuri pointed out the strong inter-jet effects in three-jet (Mercedes) events at LEP.

Yuri: You could have compared those guys [p-p single jets] to a single-cone environment [from e^+-e^- collisions].

Tom: Yes, if we put a cone down we can get asymptotically [with decreasing radius] to an equivalence. But, if we take the base (low p_t) then they're very different. [Sure] This doesn't mean that the concept of fragmentation is broken. It is just that we have to understand these complexities.

Yuri: Yes, we shouldn't compare scrambled eggs with...

Tom: My feeling is that the most interesting physics lies at the low-p_t base of these jets. That's where all the modification is [and where the majority of the particles sit]. We don't abandon fragmentation, the concept of factorization, but we become wiser [about how to model it] and we have a more encompassing picture of what's going on.

Boris: Diffraction also makes corrections.

Tom: Yes. Stan Brodsky commented on this. He looked at this material and said DIS is ŞweirdŤ (I'm paraphrasing), p-p is weird. This doesn't make the [FF] problem unsolvable, it just means we have to be aware of these complexities.

Rene: But in p-p you have additional contributions that have nothing to do with fragmentation. Do you have an underlying event or not? If you have an underlying event then where you want to probe the FF, at the base, you're completely affected by the fact that the underlying event is also there.

Tom: There is a [quantitative] separation problem, and it will have a systematic limitation, and we have to estimate that.

Yuri: You have to learn how to subtract it.

Tom: Yes. You see that my stuff [spectrum hard components] ends [becomes undefined] at some point [at small p_t, below 0.5 GeV/c]. The uncertainty band goes to 100% at that point. But that's what an experimentalist is supposed to do.