"Discussion on jet and spectrum structure vs. pQCD from high p_t down to the soft sector"

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The following is a transscript of the discussion session on jet reconstruction and fragmentation that took place on April 16, 2010, during the workshop on *Critical Examination of RHIC Paradigms*. Participating in the discussion were Rene Bellwied, Helen Caines, Yuri Dokshitzer, Ahmed Hamed, Rudy Hwa, Jiangyong Jia, David Kettler, Boris Kopeliovich, Guy Moore, Jan Rak, Lanny Ray, Thorsten Renk, Lijuan Ruan, Anne Sickles, Raymond Snellings, Mike Tannenbaum, Derek Teaney, Tom Trainor, and one unidentified contributor. Their discussion contributions are identified below by their first names.

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1. Discussion topics

Tom: Here are a few things that came up today. Helen gave a very nice study of the hazards of triggering and jet reconstruction. The question is: Can we reconstruct jets which will give us physical information? Considering triggered vs. untriggered jet correlations – what is actually being studied? Is the near-side ridge a jet phenomenon, or is it some other thing that we should maybe ignore? Are fragmentation functions accessible in *AA* collisions? How do we best plot them, and how do we interpret them?

2. Jet reconstruction and inferred fragmentation functions

Mike: In principle, if we want to do jet reconstruction we ought to do it with γ-hadron correlations in *AA* collisions.

Tom: To get clean fragmentation functions?

Mike: I don't know how clean, because it's not a clean environment. In *pp* collisions I just showed today I think it's clean. In *AA* collisions there's so much going on with the away-side jet that I don't think there's one unique fragmententation function. I mean, things are happening with this outgoing parton.

Tom: I think we touched on this issue of fragmentation functions being "clean" in a previous discussion. CDF fragmentation functions are very different from e^+-e^- fragmentation functions for the same parton energy. For instance, in a CDF publication that has been shown here twice the fragmentation function for dijet energy 625 GeV has about the same amplitude as the fragmentation function for about 300 GeV. There's a saturation.

Mike: I can't guarantee anything. Today I just showed the plot for γ-hadron at 8 or 10 GeV, and our fragmentation functions lie on this TASSO curve.

Tom: But that's only a small part of the total fragmentation function.

Mike: It depends what you're interested in.

Tom: I'd like to know the whole fragmentation function.

Mike: Well, I don't think you're ever going to find the whole fragmentation function. Well, you might when you start going with 100 MeV/c particles. That's very difficult. Certainly you'll never see them in an *AA* collision.

Tom: So you say.

Mike: You can't get down that low and know it's from the jet. I bet you can't even do 1 GeV/c in *AA*, because there are so many 1 GeV/c particles that have nothing to do with the jet you're interested in.

Tom: We've been studying this for eight years now.

Mike: And you haven't gotten anywhere.

Jiangyong: It seems you can only do two-particle correlations. But if you do full jet reconstruction event-by-event, it will be very hard to go with the associated particle to very low momentum without understanding the background.

Tom: For jet reconstruction I agree absolutely. I think that's a very hard task. But I think with combinatoric studies of two-particle correlations you can get down to low momenta.

Mike: Except you don't measure the fragmentation function.

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Anne: Unless you do γ-hadron correlations.

Helen: Why does that help?

Jan: You can also do jet-hadron.

Helen: I don't see why you say that the γ-hadron helps. Yes, you know the energy, but you're still stuck with the combinatoric problem on the away side.

Anne: I think that's a solvable problem. It's going to allow you to get down the lowest in momentum. Maybe you are not going to get down to 100 MeV/c. But it's going to be an easier problem than the hadron-hadron correlations or the jet-hadron correlations.

Rene: Do we all agree with Jan's point that dihadron correlations have no sensitivity to fragmentation funcdtions? Did I understand that correctly from your talk?

Jan: Yes.

Mike: I can explain it to you in one sentence. You trigger on a particle that's at a certain p_t, right? Then you look at the other particles. Now you have to input that when you trigger on that particle that's fixed – say a 10 GeV/c π^0 – that it could have come from a 10 GeV jet, or a 12 GeV jet or an 18 GeV jet. When you integrate, fixing that p_t, you integrate over all the jet parton spectrum. Guess what happens? The jet spectrum is the same on the other side, so you also integrate over the fragmentation functions on the other side. And that's why you're not sensitive to it. Now, that doesn't happen if you're not doing a fragment trigger, but trigger on a γ that's a direct photon.

Jan: I think another ingredient is the flatness of the spectrum. As you go for example to higher $\sqrt{s}$, and the parton distribution becomes flatter, then the effect is smaller and you are kind of gaining in sensitivity.

Mike: I don't think so, but it's possible. I think you always integrate over the jet parton spectrum. I don't care whether it's flatter or not.

Jan: No. Part of it is the trigger bias, right?

Mike: It doesn't matter. I still think you fix the π^0 trigger momentum and integrate over the jet spectrum. It gives you that. But you are right, the away-side distribution is sensitive to that power of the parton spectrum. It's flatter as you said. But, it's still not the fragmentation function. It's flatter, so with regards to smearing and stuff like that, you're right.

Tom: Mike's integral is what I call the fragment distribution. What you've discovered is the property of a folding integral. The fragment distribution is the folding of all these fragmentation functions. It's insensitive to the shape of the individual fragmentation functions.

Mike: Oh, maybe so. But I thought the definition of a fragmentation function was there for thirty years before you got in and changed the definitions....

Tom: It's not a new definition. I respect the old definitions. A fragmentation function is a conditional distribution. What you're measuring is an integral over these things, over the parton spectrum.

Mike: Oh, OK.

Tom: So, you lose any knowledge about the shape of the conditional distribution (fragmentation function) when you do that.

Mike: OK, we agree, except when you trigger on a photon.

Tom: I would never trigger on a photon.

Note added (Tom): The photon trigger in principle selects a particular recoil-partner parton energy, and the conditional fragmentation function is then revealed, modulo secondary radiation processes as Guy noted.

3. Comments on intrinsic k_t

Yuri: A couple of times I promised to make this comment. We heard from Al, and Jan was showing these numbers, that the so-called intrinsic transverse momentum k_t is a huge thing, which depends on the hardness of the process. We know it from Drell-Yan, we know it from deep-inelastic forward accompanying particles, etc. But the formulae which Jan was showing us, from the QCD point of view, was simply wrong. You made a wonderful kinematical analysis. But the answer isn't a convolution of parton distributions with fragmentation functions. There are some extra factors which are very important, which appear because the process you are studying with your two-particle correlations is not a standard hard process. This is a two-scale hard process, so-called semi-inclusive. You have p_t as a scale, and you have $\Delta\phi$ as an additional scale – p_{out}, and this belongs between these two guys. And the ratio of these two scales enters like mad in perturbative calculations at higher order, it is not leading order as Thorsten wanted it to be. And it is an extremely strong effect, even in Drell-Yan. Instead of 500 MeV/c transverse momentum all of a sudden you get 2-3 GeV/c. And this increases with the mass of the pair. All this happens because of multiple soft radiation. If you move to hadron-hadron correlations instead of Drell-Yan it becomes twice stronger, where this "twice" enters the exponent.

Jan: But this is what we show in the plot as a function of $\sqrt{s}$.

Yuri: Yes. So, k_t increases like hell. What I want to say is, if you want indeed to extract fragmentation functions, this is possible especially if you do the photon trigger. However, one has to employ a better formula that accounts for soft radiation effects.

Mike: Where do we find this?

Yuri: In Dokshitzer, Diakonov, and Troian, Physics Reports 58 (1980) 269. This is a qualitative statement, because if you were looking at back-to-back in the parton model at zeroth order, it would have been just a delta function at $\phi = \pi$, which means that when you are near the singular point you have to forbid radiation from two initial and two final particles. Of course that corresponds to the fourth power of Sudakhov suppression – this is really a huge effect. It can be mimicked by introducing intrinsic transverse momentum which increases in a predictable way with hardness, etc. But then, you have to remember that the away peak becomes extremely broad. It's not a back-to-back correlation anymore. And all these things have to be kept in mind. Because if you want to do it inside the cone you will lose things, etc. So, one should employ better means.

Just one more message in one sentence: Don't think that Borghini-Wiedemann is a trustworthy representation of QCD.

Tom: It was a toy model.

Yuri: Absolutely! That has to be kept in mind.

4. Is the ridge a jet phenomenon?

Rene: Over the last two days one of the things that transpired in the talks was that many people

think that the[near-side 2D peak is simply too wide in η to be a jet phenomenon or a medium response to a jet.

Tom: I think a homework for us who do the untriggered stuff is to just assume everything is pions and do a rapidity calculation, and see where that goes. In other words, if the ridge stuff is all very low-p_t pions then that's not going to go very far in y_z, if you reconstruct that variable. The structure that looks very broad on η would then come back into close proximity to the center of the jet on y_z. So, we can test that. An argument in favor of the near-side 2D peak being a jet phenomenon is that what we see in untriggered correlations: the systematics are tight for that whole near-side monolith (i.e.the jet peak and near-side ridge) – we have a 2D Gaussian. It's quasi-symmetric on angle up to the sharp transition, and then the whole thing turns on with the sharp transition. There's the η broadening, the amplitudes of the near-side and away-side peaks rising sharply. So, it's not like the "ridge" is off on its own, with its own systematics, and the central "jet" peak is doing something else. They're really marching to the same tune. If you want to claim that the "ridge" is some separate physics you have to deal with those correlated trends.

Lijuan: At the centrality where you see the correlation density as a function of centrality getting broad, the majority of ridge particles are in the p_t range above 0.8 GeV/c where, at the same time, the p/π ratio starts to be different in Au-Au collisions compared to pp collisions. So, can we do a p_t scan to see what's the correlation?

Tom: I'm not sure whether this is going to be a response to your question, but David has done studies with p_t (y_t) cuts, the marginal distributions. This is different from trigger/associated cuts. It can be described as taking particles in a particular p_t bin and forming pairs with all other particles with no p_t cut. That's a marginal distribution (on y_t) of the correlation structure. What he sees there is that the near-side peak elongation extends up to 4 GeV/c. So, it's not like the elongation is restricted to very small p_t. It's a phenomenon which extends up to 4 GeV/c. And then the trend returns to pp above 4 GeV/c.

Note added (Tom): The 4 GeV/c for instance applies to one particle of a correlated pair in the near-side peak. The partner can have any p_t down to 0.15 GeV/c. The "ridge" translates to some of those pairs having large differences in η. It is expected from first principles that lower-momentum particles in a jet appear at larger angles relative to the jet axis. The "ridge" anomaly includes both broadening on η *and* narrowing on ϕ.

Rene: The "ridge" is dominated by the low-p_t particles; it's not restricted to them but it's dominated by them. Because there's no difference between the ridge p_t distribution and that in the medium. That's what we see. There is a question whether the $y_t \times y_t$ plots cover this or not. I don't see that the p_t distribution in the "soft ridge" is any different than in the medium.

Note added (Tom): The "soft ridge" terminology has been used to describe the entire untriggered or minimum-bias near-side jet peak. In fact, the p_t distribution of the near-side jet peak is consistent with the peak on $y_t \times y_t$ shown by Lanny and Duncan. The mode is at 1 GeV/c for pp collisions and is reduced to 0.5 GeV/c for central Au-Au collisions. The p_t distribution of the untriggered near-side peak in general is not the same as that for the system as a whole, and especially not the single-particle spectrum soft component, which has been described as "the medium."

Lanny: Let me comment. There are three different analyses. There is Chanaka's analysis with his way of cutting on y_t. There's what I showed with what our UTA students have done, and then there's what David has done. They are all different ways to look at the same thing. Right now, I

don't know that I have a clear picture of the p_t composition of the large-η extent of the ridge. The suggestion we have is that it continues to be pairs of fairly hard ($\sim$GeV/c) particles. But, I don't think we have a closed answer to that question.

David: If you really want to talk about the p_t of particles at large η you should do it directly by making an angular cut and looking at the $y_t \times y_t$ distribution, instead of slicing on $y_t \times y_t$ and looking at angular correlations.

Rene: The problem is what Lijuan just said, that you have proton and pion distributions, and if you look at y_t you mix them all together, although they're sitting at very different momentum. You have to look at the p_t distribution, not the y_t distribution.

Tom: To be clear, y_t *is* essentially $\log(p_t)$. In the transformation, the pion mass is used for all hadrons. The only point of using y_t is to employ a logarithmic p_t scale. And you've seen other people using logarithmic p_t here.

Rene: But not every particle has the pion mass, because the proton-to-pion ratio is one. So, you have as many protons as pions in your central peak on $y_t \times y_t$.

Tom: y_t is simply a device for logarithmic p_t.

Note added (Tom): As used in Estruct correlation studies $y_t \sim \log(p_t)$, and all hadrons are treated equivalently. There is no "mixing together." That variable choice has nothing to do with the fractional abundances of hadron species. There are apparently two issues confused in this exchange: a) how correlated proton pairs are distributed relative to pion pairs on $y_t \times y_t$ and b) how best to represent unidentified hadron correlations on transverse momentum. The choice of y_t with pion mass relates to issue b). Issue a) is currently not addressed except indirectly via implicit projection of $y_t \times y_t$ to spectrum hard components on y_t for identified hadrons.

Thorsten: Apparently there are many people working on analysing the ridge. Does anyone have a number which is, if you subtract everything that you believe is not part of the ridge and just focus on the ridge region, what is the energy content of that remainder? All I want is just a number – energy in GeV.

Rene: That's the stated question – you take the p_t spectrum and you integrate it up.

Thorsten: Why is that so difficult to get?

Lanny: Because what we look at are correlated pairs. We see a statistical sample of pairs. Most of the pairs in any bin are random. All we observe are excess and deficiency. We don't know which particles are in the ridge.

Mike: Can't you just do it as a function of p_t?

Thorsten: Can you give a lower limit?

Lanny: And that's what Rene's student has done.

Rene: I showed that plot in my talk.

Thorsten: And that number is?

Rene: I can show you what p_t the particles are at.

Unknown: Can you just get the average p_t value in the bulk, and subtract the bulk contribution?

Lanny: The problem is going from measuring statistical differences in pairs and relating that to a single-particle distribution. That's tricky.

Derek: When you're making your correlation function, couldn't you simply compare the p_t-weighted correlation function with just the number correlation function? Wouldn't that have the same kind of information?

Rene: We have to be very clear about this. In the hard ridge, the triggered ridge, all of this was done. What Anne showed, what STAR showed, is that the hard ridge looks like the medium, almost exactly.

Rudy: I thought the temperature was higher in the ridge.

Rene: A little bit, but far away from the distribution in the jet (i.e. in the central peak observed in trigger studies). It's much closer to the medium distribution than to the jet distribution, in terms of baryon/meson ratio, mean p_t, slope parameters. So, it would be very surprising if that were different for the softer particles when it is already the case for the hard particles.

Ahmed: I think the thermal ridge can tell us about that, if there is any problem with that. We can do γ-ridge stidies, and they can directly tell us if the ridge is coming from the jet or not.

Anne: You're going to wait a long time to get that out.

Helen: Why? If you want to do γ-jet he can do γ-ridge on the same time scale.

Anne: It's going to be difficult because it's so small.

Ahmed: This cannot be done at PHENIX?

Anne: I told you what we had for Run 7 data, and we see no excess compared to *pp*.

5. Reconstructed jets vs. triggered dihadron correlations vs. untriggered jets

Rene: Let's go back to Mike's talk and compare to Helen's talk. In Mike's talk we heard that it's much better to study dihadrons than to study dijets if you want to look at fragment distributions.

Lanny: Right, trigger particles vs. reconstructed jets.

Rene: What do I really learn from reconstructing the jet? That might be the question.

Mike: Yes, I can give what I like about jets. First, imagine if we had only done the methods described in Helen's beautiful talk, starting in the year 2000. Where would we be now? The answer is: "nowhere", compared to where we are. Jet reconstruction comes ten years later, with all the mistakes that have to be made. And we know that this is the time scale, because *pp* collisions took ten years of mistakes. Now, here's what I think reconstructed jets can buy you. First, let's agree that jets are not partons; I'm interested in partons, maybe they don't exist, I don't know, but jets are not partons. They're a theoretical legal contract with experimentalists. But, the big advantage they have is rate. For instance, my favorite jet experiment in *pp* collisions at RHIC is to measure parity violation in jets, through A_L. You should see a bump at 40 GeV/c from the *W* boson. So, why don't you do that with single particles? The answer is the rate is 1000 times higher with jets. Now, the jet-hadron correlations that Helen showed are really cool, I think. They are like γ-hadron correlations on steroids. You don't quite know what's the energy of that jet. You do know the energy of a photon, but not of a jet. Nevertheless, it's more close to the parton, I assume, than the energy of a leading π^0, and the rate is much higher. So, that's cool. Whether you'll be able to see jet quenching I don't know. I complained about this at QM09, where I heard Andreas Morsch talk about how, when you fully reconstruct the jet, R_{AA} will be 1 because all the energy lost by the leading parton, if it loses energy, will be inside the jet cone. I said "give me a break". You can never do that in Au-Au collisions because eventually you're going to get down to the background, and you're never going to recover the whole thing. In short, I think that in principle maybe we can learn something from reconstructed jets but it wouldn't be my choice of experiment. There's a lot of people who do this physics, but it will not be my choice.

Rene: I think full jet reconstruction will definitely give us a handle on broadening. Would you agree with that? If you do it differentially as a function of cone radius?

Mike: Why wouldn't we see that in triggered two-particle correlations?

Jan: Exactly. You can also do that there. I wanted to add to Mike's comments. You can do everything with two-particle correlations. The only difference which Mike didn't mention is if you go to the high-p_t leading particle you are also biasing yourself to higher z, so the jets might have different properties if all the energy is down to only a few particles. You are selecting jets with smaller multiplicities. This should be complementary.

Mike: I wouldn't say there's nothing to be learned with jets. I'd just say it's much harder than single-particle relative to a trigger. The latter was first done in STAR. First you showed that jets disappeared. That was PR. Then this beautiful paper, really one of the best STAR papers, showed that jets didn't disappear, they widened. You see that, we see that, everyone now sees that. Now, how are you going to reconstruct jets in that kind of a cone? You'll never see that effect with reconstructed jets. Comparing a 4 GeV/c particle with a few hundred MeV/c particles – how are you going to do that with jets?

Rene: I think you have to do that with a particular p_t cut. But it becomes very differential when you do it simply as a function of the recombination parameter.

Jan: But this is impossible. All these studies we did, this tomography with two-particle correlations with various tiny p_t bins and so forth, each time you have a different underlying jet structure. You are moving z of the trigger particle and the associated particle.If you want to do the same studies with jets, you have all these background issues. You want to start with low-p_t jets, but the entire mechanism is very different. So, you should really do it complementary. At LHC we are developing jet algorithms, people are studying them. We have already jet cross section measurements. But we should also do two-particle correlations, photon-hadron correlations, jet-hadron correlations. There is no way to say now that we will do jets and forget about two-particle correlations, or vice versa.

Jiangyong: I think two-particle correlations are something like a calibration for your jets, right? You need to know the shape first. There is another limitation with regards to low-p_t jets, below 10 GeV. Do they carry the same physics as high-p_t jets? Below 10 GeV we cannot do event-wise jet reconstruction – it's hopeless. But you can still do two-particle correlations. Do they carry the same physics?

Jan, Helen: At the LHC the limit is 50 GeV; below that, jets cannot be reconstructed.

Thorsten: From the energy-loss perspective you want to go as high up as possible, because the calculations become cleaner and cleaner as you go up in energy. Maybe we are not so interested from that point of view in 10 GeV jets at the LHC.

Jan: Why not? All the physics is there.

Thorsten: If I want to study energy-loss calculations or medium tomography, give me 400 GeV jets and I'm completely happy.

Tom: With 400 GeV jets you will observe no effect.

Mike: Thorsten, you are the one who is breaking the paradigm. Suppose jets really lose energy in the sense of bremsstrahlung, and the partons radiate some gluons, not collinear in their cone, in which case you see them within the jet, but out there sideways (for example through elastic collisions). You have to know the physics better than we know it. If you believe that jets all do

collinear radiation, then you will reconstruct that energy when you get the whole jet. So, think about doing 100 GeV jets. How are you going to learn anything?

Jan: Exactly.

Thorsten: Give me a p_t cut and study the reconstructed jet as a function of that p_t cut. If the radiation is soft, then the p_t cut will filter it out and I'll see immediately what happens.

Mike: What's the p_t cut? You mean on the fragments?

Thorsten: Give me a p_t cut when reconstructing jets.

Helen: Then you'd have to lose a lot of energy, because our jet energy resolution is never going to be better than 20-30%. So, a 100 GeV jet has to lose 20-30 GeV for me to be able to tell you that it lost it. That's the trouble with the resolution. Now if I drop down I'm starting to get somewhere. On the other hand, if you say you think it's going to lose 50-60 GeV then we might be able to tell you something. For your theory to be relevant, the parton has to lose much more energy than the resolution of our jet reconstruction.

Derek: Wouldn't the vacuum radiation be dominant then?

Thorsten: No, because I know what vacuum radiation is by looking at jets in pp.

Tom: Your interests our not necessarily our interests. You want a certainty you can describe theoretically, and that drives you to a kinematic region that may be insensitive to the medium that we're studying. You can go to the other extreme, which was introduced a hundred years ago. That was the question: Is matter molecular? Einstein confronted that by inventing Brownian motion, or reinventing it. His reasoning was: I want a probe which is the smallest thing that I can possibly observe and therefore most likely to be sensitive to molecular motion if it exists. And the strategy paid off. Perrin made his observations from 1906 to 1909, and those observations killed any claims that there were no molecules or molecular motion. We're in the same boat. We want to understand what's going on in this medium or if there is a medium at all. So, for us as experimentalists the best probe is the softest probe we have that is still observable.

Thorsten: If you want to understand anything you have to be able to make a theory. And you have to be able to calculate it.

Tom: That's from your point of view. From my point of view I want to know what the internal structure of the medium is.

Jan: I had one question, maybe also for theorists, related to the fragmentation function. Some while ago we discussed leading-particle fragmentation functions, jet-hadron correlations, etc. Is there any advantage to focus on leading-particle fragmentation functions, or not?

Anne: What do you mean?

Thorsten: The conditional probability, given a parton energy, that the leading hadron takes the momentum fraction z. That's my understanding.

Jan: Leading-particle fragmentation is identical to fragmentation above 0.5. Thorsten, you wrote a paper about it, right? Is there any good reason to try to measure that?

Tom: Does that solve the problem we were just talking about?

Boris: At least it is easier, because the p_t spectrum is very steep, and it picks up large z.

Jan: So, what's the answer?

Thorsten: I answered yes.

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Mike: I had a comment about energy loss. In bremsstrahlung it goes forward and would stay in the jets. In elastic scattering the energy loss goes sideways. [*Thorsten:* No, it doesn't.] Identical-particle elastic scattering comes out at 90 degrees.

Guy: This is only true in the CM frame, determined by the jet particle, so it does go forward in the lab.

Thorsten: Pretty much everything goes forward.

Rene: There was one thing I thought was stunning today. Thorsten gave a very nice talk about all the different models, but if you listen very closely to what he said this morning – he essentially says that in the outer two fermi there is no energy loss. And then you listen to Boris, and he said that the only energy loss is in the outer two fermi, because everything on the inside doesn't make it out. That's exactly the opposite. I don't think we have a good idea of the geometrical evolution of the energy loss.

Tom: Right. I believe that what our group has done is to take inventory, by accounting for every low-energy parton that's generated in the initial state via jet-correlated hadrons in the final state. Based on these correlation measurements I would stronly contradict the idea that only a small fraction of the initial partons make it out.

Rene: Let mel pick this up. What you, Tom, are saying is you want to look at the low-momentum hadrons to study this rather than the high-momentum particles.

Tom: I want to look at low-momentum *partons*, via their manifestation mainly as low-momentum jet fragments.

Rene: But isn't it a fact that you will never be able to unambiguously determine those, because you have an underlying event that is right there? You have a bulk? Don't you have to get out of the bulk to make any useful measurements?

Tom: That's what the correlation analysis is about. That's giving you the pairs that are correlated within jets.

Thorsten: How do you know it's not a recoil of the bulk from any jet effects? Being correlated with a jet can have many reasons. It can be an accidental correlation, because they share the same bias. For example, jets tend to go out tangential to the surface just because it's the shortest path. Anything radial-flow driven—correlations tend to go the same way because of the direction of the pressure gradient. So, they're correlated, but not by any physics mechanism, but just because they accidentally share this property. Or you hit part of the medium with a jet and it tends to go into the direction of the jet without being part of the fragmentation cone. So, there are plenty of ways to generate correlations with a jet which are not actually representative of the jet structure.

Tom: Most of those statements are based on very strong model assumptions.

Thorsten: No, I'm saying all those things could happen. So, how do you tell them apart just by making a correlation analysis?

Tom: We begin by assuming the simplest scenario without unjustified complexity.

Thorsten: That's a model in itself.

Tom: That's Occam's razor. It has been recognized for centuries as a pretty good starting point. You assume the simplest scenario, which is transparency and elementary parton production and fragmentation and see how far that takes you. You don't start with an opaque-core assumption.

Guy: That's already dead.

Yuri: Thorsten has a point. If you want to understand what is the medium you have to come up with some sort of a *simple* probe. Simple means something you can trace and understand. This means that you need moderately-large-energy jets.

Raimond: That's clear. But if in such a simple model as suggested by Tom you can't describe important features of that medium, then you know that simple model is not enough. So, then you go to these other models which were just discussed before. And then a lot more comes into play.

Yuri: We don't know basic things: whether it's radiative energy loss, whether it's collisional energy loss, whether there is a third possibility. For that we need a simple probe.

Lijuan: I have a naïve question. Conside, for example, that we made a 100 GeV jet and want to look at the fragmentation function. For this, I think it is very important to understand thev identity of the particles inside the jet cone. However, even with the ALICE detector I think the PID capability is quite limited. So, have you thought about how to improve this? How do we understand all these fragmentation functions?

Thorsten: When you have a 100 GeV jet, most of the hadrons inside the jet cone are not hard. The sum over momentum of all hadrons is 100 GeV but most of them are in the soft regime where you can do PID.

Lijuan: Am I right that PID will only be possible below 3 GeV/c?

Tom: 90% of the particles in any jet will be below 2 GeV/c, for any jet energy.

Rene: With dE/dx in the relativistic rise region, PID will extend to 15 GeV.

Thorsten: So, you should be able to do most of the hadrochemistry of the average jet.