Questions from the review by Márton Nagy

1. When defining the directions with respect to a given jet (such as "toward", "transverse", "away"), besides the $\Delta \phi$ azimuthal angle difference, wouldn't it be necessary to cut also on the difference in y or η (rapidity or pseudorapidity)? Or is it the case that the global cut in η that is pertinent to the experimental setup (and also included in the simulational analysis) makes this irrelevant? And if this is indeed the case, then is it not too inclusive to define the "toward" region with $|\Delta \phi| \leq \pi/3$ (knowing that the constraint in $\Delta \eta$ is much stricter)? Does this absence of "cylindrical symmetry" in the η - ϕ variables cause some systematic distortion in the results?

In this analysis, the azimuthal and polar coordinates are treated independently of each other, and in the polar angle we restrict ourselves to the most central rapidity. The figure below shows that the global cut in η is already enough to disconnect the leading process from the underlying event. Regarding the size of the toward region, the main goal is to clearly separate the region with the leading jet from the region dominated by the underlying event. If the toward region is defined too narrowly, then some jet constituents may fall into the transverse region (e.g. due to the wide gluon splitting). Therefore, it is more important to fully contain the leading process within the toward region. Recently, a new observable, called flattenicity, was suggested to describe the events [Phys. Rev. D 107, 076012]. It is also correlated with MPI, however, it does not require a trigger particle condition and therefore provides more data in a measurement. Also, it takes into account the whole studied η - ϕ domain.



2. How can it be that the topological cuts on $|d_0|$ are nearly independent of p_T , while the cuts on the d_0d_0 product very much depend on p_T ? Also, I see a number of cuts requiring various DCA values to be sufficiently small; where does a requirement on the *minimum* value of the DCA to the vertex enter? (As far as I know, such cuts are the ones that out of the many produced π , K particles, keep only the (compared to pixel detector resolution, off-vertex) ones that might come from D⁰ decay, thus reduce the combinatorial background efficiently.)

The cut on $|d_0|$ is a loose preselection which is needed to discard those kaons and pions which fall too far from the primary vertex, and is defined one order of magnitude higher compared to the values in d_0d_0 product (as such it is treated independently of p_T). The d_0d_0 product, on the other hand, is a stricter constraint on potential D⁰ candidates (pairs of kaon-pion), where both particles are required to be *simultaneously* close to the primary vertex. This cut is dependent on the p_T due to the different decay kinematics of D⁰ at different transverse momenta.

The DCA is the distance of closest approach between D^0 decay candidates (kaon and pion, which are potentially daughters of the same D^0). Therefore, the DCA value has only a maximum value, while it is not constrained from below (as the closer the two particles are, the higher the probability they originate from the same secondary vertex).

3. Why is PYTHIA 8 needed to be used for the determination of the experimental reconstruction efficiency of D⁰ mesons? Wouldn't it be enough to simulate the decay of D⁰ mesons with a given p_T "by hand" (i.e. without knowledge of a full event)? On the other hand, if then PYTHIA 8 is made use of, what is the reason for the average transverse activity, $\langle N_{trans} \rangle$ being (according to Section 4.6.1.) different in simulation and in data? What is the connection between the three cited values of $\langle N_{trans} \rangle$ (4.802 and 7.426 in simulation, 6.225 in data)?

The knowledge of the full event is required by GEANT 3 to adequately propagate the tracks through the simulated detector environment. The ALICE experiment has dedicated MC datasets which were generated specifically to reflect the Run 2 data. These datasets (and not a standalone PYTHIA) were used to estimate the efficiency and acceptance corrections.

The three different values for the average transverse multiplicity are due to the three different event sets: 6.225 for measured data, 4.802 for MC datasets in ALICE, 7.426 for pure PYTHIA 8 simulation. The generated events do not reproduce the average transverse multiplicity observed in data due to the different track selections in data and in ALICE-simulated events.

4. It seems to me (after a verificatory calculation) that the quantities defined through the Tsallis–Pareto distribution, Eqs. (5.2)-(5.5), are indeed thermodynamically consistent (in the sense explained), for any T, q parameter values; *if and only if* instead of mT, the full E particle energy is put in the integrands, and in Eq. (5.4), instead of the m particle mass, the real μ chemical potential is used. The quantity $\varepsilon + p - T s - \mu n$ (calculated with fitted parameter values) can be non-zero precisely because the E \approx mT and $\mu \equiv$ m hold only

approximately. Knowing this, what is the relevance of the check of thermodynamical consistency explained in Section 5.1.2?

The approximations of E to m_T and μ to m were needed in order to define such a function which would incorporate only measurable variables and could be fitted to the studied spectra, as detailed in [*J. Phys. G: Nucl. Part. Phys.* 47 105002]. The E≈m_T approximation was motivated by the fact that all measurements were carried out in the transverse plane. The mu≈m approximation is commonly used in the field [*Symmetry* 14 (2022) 8, 1530, *Symmetry* 15 (2023) 8, 1554], while a recent work [*MDPI Physics* 2 (2020) 4, 654] validated that μ is in the order of magnitude of m at the kinetic freezeout. In [*J. Phys. G: Nucl. Part. Phys.* 47 105002], it was found out that in case of heavy multistrange hadrons, the μ ≈m assumption becomes less valid and the quantity ϵ + p – Ts – µn starts to deviate significantly from 0. The consistency check was needed to explore this behaviour in case of D mesons, as otherwise the defined form of Tsallis-Pareto distribution could deviate largely from the thermodynamic picture.

5. What does it mean that the Bjorken model "imposes no specific thermodynamic assumptions" (Section 5.1.5)? To my knowledge, the Bjorken picture rests on an extremely simple flow velocity profile, and this, together with the T \leftrightarrow T connection in Eq. (5.7), is a solution to the hydrodynamical equations (as it should be the case) if a specific (class of) Equation of State is assumed; one that incorporates, among other things, the $\varepsilon = 4\sigma/c^*T^4$ Stefan-Boltzmann law.² How would it influence the results about the spectrum formation times if one took other (in some sense, more advanced) hydrodynamical solutions, or some other Equation of State?

²There is a typo in the dissertation concerning these, before Eq. (5.7): the energy density ε corresponding to the Stefan-Boltzmann law is the one I wrote up here; omitting c is tolerable, but the factor of 4 is important.

The Bjorken expansion is independent of the underlying thermodynamical picture. From the cited article [*Phys.Rev.D* 27 (1983) 140-151]: "Finally, we have not addressed questions of experimental observables and signatures, other than commenting that in this model the final pion multiplicity should not depend upon details of the equation of state or how the system evolves in time but only upon the entropy density imposed in the initial boundary conditions." This allows one to assume the compatibility between the Bjorken picture and the Tsallis–Pareto framework. The more advanced hydrodynamical solutions or other EoS could be incompatible with the assumption of the Tsallis–Pareto framework, which would not allow for the estimation of the spectra formation times.

The definition of the Stefan-Boltzmann law in the dissertation was indeed unfortunate. While it does not influence the result (as both constants cancel out during calculations), it would be more appropriate to use a proportion sign (~) instead of equality (=).

6. Can the observed scaling of the Tsallis temperature with hadron mass be interpreted as radial flow? If not, does this cast doubt on such interpretation in heavy-ion collisions?

The question of the radial flow was investigated in the earlier paper [J. Phys. G: Nucl. Part. Phys. 47 105002]. There, it was found that the effect of transverse flow is negligible for the Tsallis fits (c.f. a few %). In case of heavy flavour, where collective effects are always weaker than in light flavour due to earlier formation, there is also no reason to assume larger transverse flow parameter values. It is also worth noting that T_{eq} and q_{eq} , from which the conclusions are deducted, correspond to the sparse system limit where there's no radial flow, which probably mitigates the effect of any residual flow effect.