Reviewer's opinion on the PhD dissertation

*by **László Gyulai**, titled*

"The origin of collectivity in small systems via heavy-flavour measurements at the ALICE LHC experiment"

Opininon prepared by: Márton Nagy
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General remarks:
The dissertation written by László Gyulai deals with strong interaction phenomenology. The candidate presents experimental work (as a member of the ALICE collaboration; however, with well-defined individual contribution), as well as theoretical/phenomenological work. The thesis is based on three few-author papers (one in J. Phys. G, one in the MDPI "Particles" journal, one in Int. J. Mod. Phys., at this time in production), an ALICE Public Note (that stands in for the publication of the experimental results for the time being, as such a publication might take year(s) according to the customs of such large collaborations), as well as numerous conference proceedings, proving that the candidate is accustomed to presenting his results to the scientific community. Based on this, I conclude that the candidate has fulfilled the requirements on publication activity needed for a PhD degree.

In general, the topic of the dissertation corresponds well to its title: production mechanisms of heavy quarks (i.e. hadrons containint them) in proton-proton collisions at LHC and RHIC energies, and what do these tell about collectivity in the final state of such p+p collision (called "small systems" when compared to nucleus-nucleus collisions at the same energy). Investigation of collectivity in small systems (the discovery of what put many of the prevailing decades-old paradigms on how collectivity can be interpreted as a signature of the Quark Gluon Plasma into question) is a front line of today's high energy physics. The research topic as well as the methods and results contained in the dissertation are thus definitely interesting and timely.

Structure of the dissertation; level of documentation:
The English language dissertation provided a delightful and immersive reading experience for me; the candidate uses the professional language on a high level. I needed a magnifying glass to find some (by any standard, very few) typos and stylistical errors.

The structure of the dissertation is interesting. After Chapter 1, which is an introductory one, starting with wide scope and somewhat narrowing it down to the current topics, introducing high-energy collisions, fundamental results in heavy-ion physics, collectivity in small systems, and the significance of heavy flavor measurements, Chapter 2 is also an introduction, however, now with a distinct focus on the definitions, methods to be applied later on in the candidate's original work. The main results are presented in Chapters 3, 4 and 5, with a summary at the end of each one, and the closing Chapter 6 again summarizes, first as "raw text", then grouped in thesis points. In doing so, there are many repetitions (sometimes almost word-by-word) in the various summaries; I would have recommended to shorten and compress these. Also, the four thesis points, while clearly indicative of the topic and the work done, are unnecessarily lengthily written, even to the point of somewhat obfuscating the results. Thesis points should concentrate on the results, not on the recapitulation of the measurement methods, techniques etc. Also, an outlook-like section at the end is sorely missing; many things (for example, the remarks about what can be done in the future with more available data) would rather belong to such an outlook instead of the thesis points. The thesis booklet and the Hungarian summary fit well to the dissertation as accompanying material; the remark that more compressed and straightforward statements on the results could have been made also applies here.

Thesis points, scientific results:
Chapter 3 deals with proton-proton collisions simulated with the PYTHIA 8 event generator. Contemporary event generators are, however well developed, complicated enough that a clear picture on the causes of various effects get lost sometimes; it is thus interesting to investigate various production mechanisms, both from a viewpoint on what to expect from data, and also from the viewpoint of how to better tune the simulation, using observables that open up new sectors. The main result here is that in simulation of jetty events, there is a clear distinction in heavy flavor production mechanisms: in sectors governed by hard scattering of heavy flavor, D and B mesons are produced in the leading hard process, while in other sectors, in the underlying event, that is responsible for the observed collective behavior in such collisions. Chapter 4 turns to real data: it details an analysis (measurement) of the R_T-dependent heavy flavor (experimentally, only D^0 meson) production in p+p collisions with the ALICE detector. Bringing through such a measurement (first through the analysis chain, then through collaboration scrutiny) is a daunting task, and the candidate performed well here. The main result is that real data and simulation (in the veins of the one detailed in Chapter 2) are consistent. I fully accept these as new scientific results by the candidate.

Chapter 5 is somewhat separate from the two preceding ones; here the concept of Tsallis-Pareto distributions (capable of simultaneously describing soft and hard parts of spectra) is introduced and shown to be applicable (and indeed, applied successfully) to charm hadrons. Scalings (and the presence of a low-multiplicity limit) observed for light mesons seem to be obeyed by charmed mesons as well, albeit with different temperature, suggesting earlier formation.[1] This line of thought is elaborated further by determining the formation time ("freeze-out time") of various hadron spectra using the Bjorken flow picture (almost as simple as a scaling law). The formation times of mesons and baryons turn out to follow different scaling with hadron mass. The utilization of the Tsallis distribution is advantageous here not just because the temperature as an experimental input is more well-defined in terms of quality of the fit result, but also because the q parameter can be given significance as an input to the determination of the specific heat of the system. While I have some clarificational questions here (No.4 and 5 in the list below) on some points, I do accept the work expounded here as new scientific result.

In summary:
Based on the merits of the scientific results and the quality of the presentation, I consider the dissertation to be fit for public debate. I have a couple of questions (grouped into six items below) that I would like to be addressed. Depending on a successful defense, I recommend the conferral of the degree of Doctor of Philosophy (PhD) on the candidate; I also seize the opportunity now to congratulate him for the hard work, and wish him a successful future career.

[1] I must note that I particularly disliked the main Figure 5.3 here: too much information, too intricate labeling, while not labeling a crucial ingredient for the determination of the common grouping points, event multiplicity; I would urge one to choose some other plotting scheme for such an investigation.

Questions:

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?

2.) How can it be that the topological cuts on $|d_0|$ are nearly independent of p_T, while the cuts on the $d_0 d_0$ 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 D^0 decay, thus reduce the combinatorial background efficiently.)

3.) Why is PYTHIA 8 needed to be used for the determination of the experimental reconstruction efficiency of D^0 mesons? Wouldn't it be enough to simulate the decay of D^0 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, $\langle N_{\text{trans}} \rangle$ being (according to Section 4.6.1.) different in simulation and in data? What is the connection between the three cited values of $\langle N_{\text{trans}} \rangle$ (4.802 and 7.426 in simulation, 6.225 in data)?

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 m_T, 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 $\varepsilon + p - Ts - \mu n$ (calculated with fitted parameter values) can be non-zero precisely because the $E \approx m_T$ 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?

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 $T \leftrightarrow \tau$ 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 $\varepsilon = \frac{4\sigma}{c} T^4$ 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?

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?

Budapest, May 7, 2025

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[2] There 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.