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**发送时间:** 2025年7月14日 3:56  
**收件人:** Yin, Shi  
**主题:** Your\_manuscript LD19694 Chen

Re: LD19694  
    High-order fluctuations of temperature in hot QCD matter  
    by Jinhui Chen, Wei-jie Fu, Shi Yin, et al.  
  
Dear Dr. Yin,  
  
We sent your manuscript to our referees for review. We received the  
appended reports.  
  
Based on the information at hand, we judge that your manuscript  
probably does not meet our criteria of impact, innovation, and  
interest and is thus not suited for publication in Physical Review  
Letters.  
  
However, if you feel you can overcome the criticism and make a  
convincing case for publication in PRL, you may resubmit to us. If  
not, your manuscript may well be suited for Physical Review D. Please  
use <https://authors.aps.org/Submissions/> to transfer.  
  
The editors of that journal would make the decision on publication,  
and might seek further review. However, our complete file would be  
available to them.  
  
With either a transfer to Phys. Rev. D or a resubmission to PRL,  
please include a response to all referee comments and a summary of  
revisions made, with a marked PDF manuscript if possible.  
  
Yours sincerely,  
  
Nikhil Karthik  
Associate Editor  
Physical Review Letters  
Email: [prl@aps.org](mailto:prl@aps.org)  
<https://journals.aps.org/prl/>  
  
NEWS FROM THE PHYSICAL REVIEW JOURNALS  
  
PRL now publishes End Matter  
<https://go.aps.org/endmatter>  
  
----------------------------------------------------------------------  
Report of Referee A -- LD19694/Chen  
----------------------------------------------------------------------  
  
The authors introduce a new thermodynamic potential which is suitable  
to discuss higher order moments of temperature fluctuations. Their  
motivation for doing so is that they aim at a systematic analysis of  
transverse momentum fluctuations measured in heavy ion collision  
experiments. This could have been done also in the conventional  
micro-canonical ensemble. The ensemble introduced by the authors  
differs from the micro-canonical ensemble only by exchanging the  
particle number in favor of the chemical potential as a parameter the  
describes a thermodynamic equilibrium state.  
  
A goal of the paper is to contribute to a systematic discussion of  
transverse momentum fluctuations. This, however, is not the case.  
Transverse momentum fluctuations are generally discussed in terms of  
'initial state fluctuations' of energy and particle number and the  
temperature introduced in this context is an effective temperature  
related to the evolution of the medium, generated in heavy ion  
collisions, towards an equilibrium state. The paper merely discusses  
thermodynamic relations valid in a certain thermodynamic ensemble that  
describes equilibrium thermodynamics keeping certain state variables,  
characterizing a thermodynamic state, constant. The argument that the  
newly introduced ensemble described by ``W'' is in this respect  
preferable to the usually considered micro-canonical ensemble is not  
convincing.  
  
I thus think that this paper does not contribute to a more systematic  
analysis or better understanding of transverse momentum fluctuations  
in heavy ion collisions. A more detailed analysis and direct  
comparison with experimental data may be needed, which could be given  
in a regular article.  
  
----------------------------------------------------------------------  
Report of Referee B -- LD19694/Chen  
----------------------------------------------------------------------  
  
New experimental data are available by the ALICE and ATLAS groups for  
the fluctuations of the transverse momentum of charged hadrons in  
heavy-ion collisions, STAR has presented such fluctuations in the  
context of the collision energy dependence at the Quark Matter  
conference in 2025. While a non-monotonic behavior of mean p\_T  
fluctuations as a function of centrality was suggested as one of the  
possible signals of the QGP, such fluctuations are also affected by  
non-thermodynamic variations in the initial geometry of the collision.  
  
This paper attempts to calculate a theory prediction on these momentum  
fluctuations based on Equilibrium QCD. The procedure is based on the  
calculation of temperature fluctuations in as system with fixed  
entropy. <p\_T> is understood to be proportional to temperature, that  
admits equilibrium temperature as a proxy. The proportionality factor  
can be canceled by building various ratios of fluctuations. All this  
is discussed in the context of a low energy effective model  
parameterized using FRG equations in the past. The model is summarized  
in the supplemental material.  
  
There are three principal problems or questions that I formulate:  
  
1) To what extent are the fluctuations in thermal equilibrium relevant  
in the interpretation of experimental results. How much of the  
fluctuations actually describe the initial state of a hydrodynamical  
evolution?  
  
2) The state function of (S,V,mu) that the authors define through a  
Legendre transformation allows the calculation of the fluctuation of  
temperature. This point (and the actual computation of the  
fluctuations themselves) is the novelty in the paper. The connection  
between dw/ds and <p\_T> should be worked out better. Presently it is  
just a single motivating sentence in the manuscript, what I would like  
to see is a proof. One could move a plot to the supplemental material  
if more space is required for this.  
  
3) The computation uses high order temperature derivatives of the  
pressure function in an intermediate step Eqs (9-10). The PQM model  
used by the authors may be a crude approximation for this task. While  
a comparison to lattice QCD was performed for this model in earlier  
works, the high order temperature derivatives are not among the  
observables that have ever been tested. Note, that even lattice would  
have severe difficulties going beyond chi\_2, because of the numerical  
nature of obtaining these.  
  
Some more further points:  
  
a) The rich structure visible in the result plots might just be an  
instability of making higher order derivatives. Do the authors insist  
to use that high chemical potentials?  
  
b) Would the computation of the fluctuation ration make sense on the  
chemical freeze-out line, instead of fixed mu, T-scans?  
  
Then the question: is this a PRL?  
  
As far as a can tell, the result is a significant step forward and  
represents a welcome theoretical contribution that complements the  
progress in experiment. This can clear the way for further research  
with better models.  
  
Yet, I am unsure: the paper in its present form does not provide  
enough details to convince that the presented idea is actually  
correct.