Dear referees,

We appreciate the revision of our manuscript, we have implemented the changes that were suggested. Your comments significantly improved the quality of the text. Please find below a detailed replies to your comments/questions.

Regards

The authors

Referee: 2

COMMENTS TO THE AUTHOR(S)
The authors of the manuscript „Revealing the source of the radial flow
patterns in proton-proton collisions using hard probes“ suggest to
perform a more differential experimental analysis of proton-proton
collisions according to the multiplicity and, in addition, the presence
of a jet. The idea is to disentangle different possible mechanisms that
lead to radial flow patterns. The authors support this claim by the
presented studies within the Pythia and Epos event generators.

In general, the manuscript addresses a topic of high interest, namely
the investigation of collective effects in small systems, here
proton-proton collisions. The suggestion of defining event classes not
only by multiplicity, but also by requiring a jet seems to have
interesting prospects to get more insights about the mechanism behind
the observations. Even though the results are of interest, the
presentation lacks clarity at the moment. The reader needs more guidance
of which observables are investigated and why and how the main
conclusions are drawn (see more detailed questions below for examples).

In addition, the manuscript needs to be clarified in many places by
checking the language carefully for grammar mistakes. For example,
already in the abstract, there is an incomplete sentence: „Namely, the
strength of the coupling …“ and the meaning is obscured.
Namely->Specifically

More detailed questions and comments:

1) The notation of ‚pp‘ and ‚ppb‘ looks a bit strange, I recommend to
use ‚p-Pb‘ (and ‚p-p‘)
We now use in a consistent way the following notation: Pb-Pb, p-Pb and pp.

2) Introduction, line 48ff: The mechanism of multi-parton interactions
and especially the main point that the soft and hard scattering are
coupled by these has to be explained more clearly.
We have added a reference and the text was slightly modified.

3) Introduction, line 59-60: A priori, it is not clear that the
hydrodynamical evolution is not influenced by high pT particles. High pT
particles are required for high multiplicity collisions (as the authors
show later on themselves) and those events have a particularly strong
hydrodynamic evolution. Also, the high transverse momentum shows up as
hot spots in the initial state and can influence the evolution.
We wanted to say that jets could influence the hydrodynamical evolution, but the effect should be rather small. We added a reference and a sentence to make it clearer than before.

“On the contrary, in the scenario where the hydrodynamical evolution of the system is the prime mechanism, jets are not expected to strongly modify the radial flow patterns. Albeit hard partons cannot thermalize, momentum loss of jets could affect the fluid dynamic evolution of the medium. However, the effect has been studied for heavy-ion collisions and it was found to give only a minor correction~\cite{ https://arxiv.org/pdf/1407.1782.pdf }.”

4) Section 2, line 43-46: The authors explain what minimum bias means
experimentally, but do not explain, which events are selected in the
event generator calculations. Please add this information.
In Pythia we included diffractive and non-diffractive events (codes: 101, 103, 104, 105 and 106). We add this information.

5) Why does z as defined in equation (1) stop at 6 times the average
multiplicity? What about the really high multiplicity events that could
be included by plotting z > 6, if those exist? Just to introduce this
variable z, it would be nice to show a plot of number of events per z
range, such that one gets an idea how rare which event class is.
Of course, higher multiplicities can be generated, however, as shown by the ALICE Collaboration (arXiv:1606.07424), <dN/deta>~25 is enough (high) multiplicity to see novel effects like strangeness enhancement and radial flow-like patterns. Therefore, adding more multiplicity bins would not change the message that we want to transmit in our paper. We would keep the section as it is, extending a bit the text, in this way we would also avoid showing distributions with large statistical errors:

“It is important to say, that according with ALICE results~\cite{Adam:2016emw}, <dN/deta>~ 25 is large enough to see new phenomena in small systems like the strangeness enhancement. For the simulations presented in this paper, such high multiplicity densities are achieved for z>4.”

6) Figs. 1 and 2: If comparison to experimental data is not the point
here, I wonder why figure 2 is included? The two different models Epos
versus Pythia should be introduced on equal footing, therefore, I
suggest to show Figure 1 for Pythia as well or to only show Fig. 2,
where the main features are summarised (and remove the current Fig. 1).
We have updated figure 1, now Pythia results are also shown. The text was slightly changed.

Regarding questions 7-9: Now we see that the inclusion of this very technical discussion about the jet finder can confuse, specially, because in that section we used special simulations (fix pThat). We would reduced it and just mention the settings that were used for the jet finder along with a reference. Actually, FastJet is a very well know and tested algorithm. It has been even used in challenge environments like central Pb-Pb collisions (e.g. The ALICE Collaboration, JHEP 30 (2014) 013).

7) The whole section 2.3 needs a better introduction, why it is
important to explain the jet reconstruction details. The conclusion that
results suggest that the jet finder is suitable needs to be backed up
with arguments - when would it not be suited? Does it matter that the
found jet is not necessarily the leading one?

8) The ‚maximum invariant pT‘ needs to be defined properly. Also, it is
confusing, that the text (Section 2.3, line 45) states that the trigger
is on 25-26 GeV/c whereas the figure label suggests it being a fixed
value at 25 GeV/c.

9) page 6, line 51: The authors mention a peak at 5 GeV that appears in
low multiplicity events, but since the axis is cut at that value it is
not clear how the curve continues in Fig. 3 (b).
The minimum pT used for the jet reconstruction is 5 GeV/c.

10) Figs. 4: The interesting conclusion (p. 8, line 34-35) that the peak
in the proton to pion ration might be a feature of fragmentation in low
multiplicity events is based on observing a shift in the peak to higher
pT for higher trigger jet pT. First of all, it would be helpful, if the
figures next to each other had the same scale on the y axis, otherwise
the shapes of the curves are hardly comparable. Secondly, the above
quoted observation is not that obvious since the statistics for higher
jet pT’s is not as good and there is no real peak visible anymore.

We have updated the picture following your suggestions. In addition, the x axis has been extended up to 9 GeV/c to facilitate the evolution of p/pi as a function of the jet pt and multiplicity.

I guess the main point is that Pythia and Epos show qualitatively similar
results in low multiplicity events, but very different results for the
high multiplicity events, when different jet pT’s are selected?

Yes, that is the point. In addition, by analyzing the data as a function of the event multiplicity (see Fig. 1) the ratios change, albeit, the differences are significantly smaller than those observed when on top of the multiplicity selection, we also cut on the jet pT. In the text we refer two times to figure 1 in order to emphasize this difference.

We have added the following sentence: “In summary, an analysis of data as a function of multiplicity and hardness provides a more powerful tool for testing the aforementioned models than that which only uses multiplicity.”

11) page 9, line 20-22: The given ranges are pT ranges not pT^jet ranges
I assume, so the labelling needs to be corrected.
Yes, now it has been corrected.

12) page 9, line 30: The „three specific leading jet pT intervals“ have
to be introduced more clearly. Before, it is always a selection of
events with jets that is splitted in three different categories and in
this section a different definition is used, where the left hand side
figures do not contain any jet and two classes from the previous three
are shown as well. Please motivate why the definition is now changed and
mention this in the text not only in the figure caption.
We have changed the sentence as follows: “For pp collisions at $\sqrt{s}=$ 7\,TeV simulated with \textsc{Pythia}~8, Fig.~\ref{fig:bwPythia8} shows the $p\_{\rm T}$ spectra of charged pions, kaons and (anti)protons for two multiplicity classes, $0<z<1$ and $5<z<6$, being each one split in three specific subclasses. The first subclass, treated as a baseline since no jets at mid-pseudorapidity are found, is compared with samples where low (5-10\,GeV/$c$) and high (20-25\,GeV/$c$) $p\_{\rm T}$ jets are produced. There are some interesting observations one might read off from this analysis:”

12) The blast-wave analysis in Section 3.3 is certainly a good way to
condense the information from the transverse momentum spectra. It would
be good to explain in more detail in the discussion of Figs. 5 and 6
which fits are claimed to be ‚good‘ and what is the quantitative measure
that was studied. By eye, I do not see any significant difference in the
fit quality in the considered pT region for all different cases,
high/low multiplicity, with or without high pT jets with or without
color reconnection or hydrodynamics.

Gyula: Please provide the chi2/ndf for each BW fit.

The results in Fig. 6 from Epos are
only mentioned in 1-2 sentences in the text. Especially Fig. 7 that
contains a lot of interesting results needs to be explained and
discussed in more detail also connecting the results to the underlying
physics mechanisms.
Antonio: TODO

13) The summary of the main conclusions on page 13 is nicely written,
but hard to connect to the previously shown results. The authors need to
make an effort to guide the reader to those conclusions by emphasising
the crucial results and leaving out unnecessary details.
Antonio: TODO

Once the authors have addressed the above comments and make a more
convincing case this manuscript can be considered for publication at
Journal of Physics G.

Referee: 3

COMMENTS TO THE AUTHOR(S)
     The authors analyze the transverse-momentum distributions of charged
pions, kaons, and protons as a function of event multiplicity and the
transverse momentum of leading jet for the pp collisions at
\sqrt{s}=7TeV simulated with the Pythia8 and Epos3 models. By comparing
the results for the models with and without the mechanisms of color
reconnection and hydrodynamics, they discussed the relationship between
the mechanisms and the radial flow patterns in the lower and higher
multiplicity events. The work is helpful for the data analyses in the
experiments. I will recommend it to JPG publication after finishing the
following changes:
     1) P. 2, 17 line, “… pp and ppb collisions …” --> “… pp and pPb
collisions …”.

Done it
     2) P. 6, figure 3, the labels of y-axes are not clear enough.

We decided to remove this very technical discussion since only confuse people.
     3) Also, in the caption of figure 3 and in the context, the meaning
of the symbol “\hat{p}\_T” is not clear enough.

The figure was removed.

     4) As there are various forms for blast-wave momentum spectra,
please present the formula for the blast-wave fits in the paper.
We think that the reference to the paper (where BW is defined) is enough.

Letter reference: DSMa01