# 1 Answer to referee 1

We thank Referee 1 for their careful reading and their insightful comments. The referee raised very relevant and important points. We answer their comments below.

1. *The quantitative results claimed require error/uncertainty, and the results need to be justified and interpreted. In particular, the paper claims three temperatures acquired. The fitted values 560nK and 1550nK are very high compared to the 100nK of the prepared gas*

   Thank you for pointing this out. Let us emphasize the three following points.

   First, it was a mistake to announce an expectation of $\sim$ 100nK for the temperature of our clouds in Sec. 2. We have corrected this, and we no longer mention this temperature of $\sim$ 100nK, see below.

   Second, we stress that the data of Sec. 5 are not the same as the data of Sec. 6. In Sec. 6, we deliberately choose a data set at a higher temperature, in order to enhance the expected asymmetry of the local rapidity distributions. This was not clear in the previous version, in particular in the discussion of the experimental setup in Sec. 2. We modified the text in Sections 2 and 6 to clarify this.

   Third, in Sec. 6, it was a mistake to overstate the significance of the fit of the density profile of the slice after expansion. There are effects that we do not understand here, so that we do not believe that the fitted temperature of 1550 nK is meaningful anyway. Therefore we have removed that fit from Sec. 6, and the temperature of 1550 nK no longer appears in the paper.

   (a) *The method applied for acquiring the temperature measurement/estimate of 100nK and its uncertainty need to be explicitly stated.*

   The referee is correct. It is our mistake. The temperature of $\sim$ 100 nK mentioned in the previous version corresponds to values routinely obtained in our setup using Yang-Yang thermometry for clouds confined in a harmonic potential. However, here Yang-Yang thermometry cannot be applied to the data of this paper because we do not know the longitudinal potential with sufficient precision.

   In the new version of the discussion in Sec. 2, we no longer mention the expectation of $\sim$100 nK, but instead mention a temperature range and emphasize that it is an approximate value.

   (b) *The fitted temperatures and chemical potential need to have their fitting sensitivity/robustness analysed, given the fluctuations in the measured profiles.*

   We now provide the standard deviations of the fit parameters. Those are the deviations estimated assuming that the discrepancy between data and fit is solely due to noise in the data. As explained in the text, however, in the case of Fig. 5, we believe the main reason for deviations from the fit to the data is actually the fact that the model does not faithfully capture the physics.

   We now emphasize this in the text at the top of page 10.

   (c) *The meanings of the fitted temperatures need to be discussed and the discrepancies from available thermometry methods need to be addressed.*

The referee is correct that it would be very interesting to compare the temperature obtained by fitting the boundary density profile to the temperature obtained by different thermometry. Unfortunately, we did not do so when we took the data. Now the experiment is no longer working, and it will not work for some time, so we cannot take new data.

Let us elaborate on each thermometry method and the reasons why we did not use them when we took the data:

- **Yang-Yang thermometry.** This thermometry based on the analysis of the density profile of the trapped cloud cannot be used because of our lack of knowledge of the longitudinal potential.
- **Insitu density fluctuations or density-ripple thermometry.** We did not apply such thermometries on the data sets presented in this paper because we believed that, most probably, the latter would have given a temperature substantially lower than that obtained fitting the boundary profile in the bi-partite protocol. The reason for this beleive is based on our numerous studies made on the quasi-bec gases produces in our experiment which indicate that phonons, probed by both the insitu density fluctuation and density ripple thermometryies, are at a "temperature" lower than higher energy excitation (see for instance chapter 5 of Bess Fang thesis (https://theses.hal.science/tel-01127046v1) , and the paper "Long-lived non-thermal states realized by atom losses in one-dimensional quasicondensates" [PRA, **96**,013623]).
- **Measurement of the local rapidity distribution measurement.** The temperature of the initial could have been probed measuring the rapidity distribution of a small slice of the gas, as done in the paper "Probing the Local Rapidity Distribution of a One-Dimensional Bose Gas" [PRL, **133**,113402]. Unfortunately, we did not performed such a thermometry for the data of this paper. This is our mistake. Unfortunately now the experiment is not working anymore and we cannot remedy to this in the near future.

*(d) I would like the authors to comment a bit more on the fit results, whether they are realistic or unexpected, etc.*

The fitted temperature of the boundary density profile (Fig. 5(a), black dashed line) is higher than typical temperatures obtained in our setup using the Yang-Yang thermometry on clouds cooled in a harmonic potential. In the latter case, the ratio $k_B T/\mu$, where $\mu$ is the peak chemical potential, ranges from about 1 to about 2 depending on the cooling parameters. We added a sentence in the text to discuss this point in the paragraph after Eq.(12).

*(e) Justification required: Page 12, Figure 7 - Why is a thermal fit instead of a GGE (generalized Gibbs ensemble) fit applied, while the system is not globally equilibrated?*

We have removed the curve corresponding to the fit that was giving an unrealistic temperature of 1550 nK, see our reply to point 1 above.

For the other fitting temperature of 560 nK, let us stress that it is obtained by a fit of the density profile in Fig. 6. We stress that, in Sec. 6, our goal is to investigate whether we can observe the expected sharp border of the local rapidity distribution resulting from the expected abrupt jump to zero of the local occupation factor distribution for $\theta > \theta^*$. The exact shape of the initial occupation factor $\nu_0(\theta)$ distribution is not very important for this study. This is the reason why we were extracting $\nu_0(\theta)$ from a fit of the boundary profile assuming it corresponds to that of a thermal state. This fit is shown in Fig. (6).

Using for $\nu_0(\theta)$ an ansatz with more paramaters, that capture not only thermal states but also a family of non-thermal GGE, would not change drastically the expected local rapidity distribution: in any case the latter would show an abrupt border on the side of large $\theta$ and would vanish for $\theta > \theta^*$.

2. *Cross-checking results*

(a) *- Wherever possible, the temperatures presented should be checked against established thermometry methods, e.g. Yang-Yang thermometry (A. Vogler, Phys. Rev. A 88, 031603 (2013)), the thermometry from fluctuations (T. Jacqmin, Phys. Rev. Lett. 106, 230405 (2011)), or density ripples (S. Manz, Phys. Rev. A 81, 031610 (2010)).*

We do not check the results of this paper against established thermometry. Unfortunately we cannot remedy to this weakness in the short term, because the experiment is no longer working. See also our answer to point 1(c) above.

(b) *- Page 7, Figure 4(b): Since the experiment is conducted at 100nK, it would make sense to include the finite temperature calculation for comparison.*

The mention of 100 nK was a mistake (see answer to point 1(a) above), we no longer mention that temperature in the paper.

Otherwise, we agree that the way Figs. 4(a) and 4(b) were presented in the previous version was confusing. We have now reorganized this part of the manuscript. We have split the former section 4 about experimental data and integrated it into the theory discussion.

Now Section 3 is dedicated to the discussion of the ballistic scaling with the ratio $x/t$, and displays the collapse of the density profiles (the old Fig. 4(a)) as the new Fig. 2.

Section 4 is dedicated to discussion of the zero-temparature profiles. Here we show the experimental data for our coldest clouds (the old Fig. 4(b)) in the new Fig. 4(b).

(c) "Page 13: "the temperatures obtained in this paper by the thermal fits are large so that the effects of populated transverse states might have an impact". Can the authors compare quantitatively the transverse excitation energy to the tail of the rapidity distribution?"

In order to compare the typical longitudinal energy per atom to the energy gap between the transverse ground state and the first transverse excited state, we shaded the regions where $m(x/t)^2/2 > \hbar\omega_\perp$ in the bonudary profile plots shown in Fig. 5(a) and in Fig. 6(a). We also added a sentence at the end of Section 6, when discussing the difference between the profile of the expanded slice and the GHD prediction.

3. *Concerning completeness*

(a) *- Page 9, top: The authors have provided a description of the first method attempted, however this part is closed without showing explicit results. It would be helpful if the authors provide the results to demonstrate the difficulties stated.*

Thank you for suggesting this. We added an appendix where we explain this.

(b) *- Page 10, Section 6: As I understand, it has not been established that local rapidity distribution can be directly probed in this work. The title "Probing the local rapidity distribution" may overstate the scope of the results. I would like to see the authors either update the results, or rephrasing the section title to avoid misleading implications. Can the authors give a succinct statement for why the comparison between rapidity distributions obtained from different approaches is not achieved already? It would be a shame that the local rapidity measurement is promised in a separate work, rather than used here to settle some of the ambiguity about the temperature/rapidity distribution.*

The referee is correct: as we explain in the last paragraph of section 6, we do not exclude the possibility that discrepancy between the GHD predictions and the density profile after expansion of the slice is due to spurious edge effects associated to our slicing procedure. We thus change the title of this section and put "Local rapidity distribution within the boundary".

We agree that it is a pity that we did not compared the initial rapidity distribution obtained fitting the boundary profile to that obtained using alternative techniques. We already discuss this issue in our answer to the point 1 (c) above. The most relevant alternative method to extract the initial rapidity distribution would have been to implement the protocol of [Dubois et al., PRL **133**, 113402]. One reason why we did not perform this task is that the studies of the boundary profile have been done in the lab *before* we took the data of the paper [Dubois et al., PRL **133**, 113402].

4. *Concerning clarity*

(a) *- Page 5, below Figure 2: "the solution of (3) is invariant along rays of constant velocity x/t". The stated solution is rather difficult to visualize. The authors can maybe consider describing this property of the solution in terms of rescaling the profiles on to one curve. This may also make Figure (4a) easier to understand.*

Thank you, we have rephrased this in the paragraph just before Eq. (4).

(b) *- Page 10, bottom: "The chemical potential is adjusted so that the initial linear density is the linear density in the region $x > 0$ measured before the boundary broadening". I did not understand this statement. Could the authors reformulate the sentence?*

We modified the sentence and hope it is more clear.

(c) *- Page 10 to 11: Where reference [31] is given, it would be helpful to provide a short qualitative statement why this method provides the rapidity. Because the interaction energy is released during expansion and what is observed is the asymptotic velocity? Also for explaining the slicing method, it may be helpful to show a distribution such as Figure 2, and describe what is sliced out.*

We added a sentence that says that "after a sufficiently long 1D expansion one indeed expects that the density profile and the velocity distribution of the atoms become equal to the rapidity distribution", and we cite a paper where this is derived from the Bethe-Ansatz form [Campbell2015].

It is of course true that, during this expansion, the interaction energy is released but we do not think it is the fundamental point nor that it is sufficient to understand the physics. So we prefer not to put this argument. For instance, consider the case of a hard-core Bose gas. Then the interaction energy is vanishing, both before the expansion and

after. Nevertheless, the shape of the momentum distribution of the particles changes a lot during the expansion and it converges towards the rapidity distribution.

We modified our Figure 2 so as to display a typical slice. In the caption we now explain why the occupation ratio (and therefore also the rapidity distribution) should be highly asymmetric in the slide. We now refer to Figure 2 at the beginning of Section 6, when this idea is discussed for the first time.

(d) *- Page 12, Conclusion: "The density profile is not very far from that predicted..." "Noticeable differences..." The two consecutive statements are in apparent contradiction. Can the authors provide more careful examinations and more explicit statements in the conclusion?*

The referee is correct. We propose to keep "not very far" but to remove "noticeable".

5. *It is stated the sharpness of the boundary is mainly limited by the imaging resolution for the pushing beam. Could this be verified by imaging the cloud after pushing? Is the cloud imaged with the same resolution, or better?*

The cloud is imaged using the same objective as for pushing beam. Thus the resolution of the imaging cannot exceed that of the pushing beam. In fact we can only have a lower resolution when imaging the border since we have to wait for at least 1ms between the pushing beam and the image, so that removed atoms had time to fly away and so that the lines on the CCD camera that received photons scattered during the pushing beam, could be shifted. During this 1ms time interval, the boundary begins to broaden.

6. *Page 7-9: One would think it is possible to incorporate data at different times to acquire better analysis (short times have small separation but higher SNR, longer times have lower SNR and larger separation). And the authors have specifically shown already that they have data for multiple expansion times. Have the authors considered doing so, or is there a reason that profiles at shorter times cannot be used?*

The referee is correct: we could have fitted simultaneously the border profiles $n(\zeta)$ recorded at different times. However the for the data set used to analyze the border profile, we did not record profiles and different times. Moreover, we do not think it would have been a large added value to do so: the difference between the boundary profile and the profile expected at zero temperature is clearly significant even without such a procedure (see Fig. 4(b)) and the smaller difference between the measured profile $n(\zeta)$ and the one expected for a cloud at thermal equilibrium are also significant even without this procedure.

7. *It appears the authors' bipartite quench operation and the resultant boundary dynamics is closely related to the work on transport "Characterising transport in a quantum gas by measuring Drude weights", arXiv:2406.17569, by P. Schüttelkopf et al. I am interested whether the authors can make a comparison to the situation where the other side is not vacuum, and comment on what different things can be learned. in that work, the estimated equilibrium temperature appears consistent with the observed/GHD dynamics. Can the authors explain why there appears to be such discrepancy here despite the experimental platforms are similar?*

Thank you for pointing this out. Indeed, this recent work also realizes a bipartition protocol,

with a method completely different from ours. We have added a sentence in our introduction to refer to this work.

In [arXiv:2406.17569] the bipartite quench protocol corresponds to two semi-infinite systems at different densities. For a cloud initially at finite temperature, however, the local occupation factor distribution would present a single discontinuity, as in the protocol investigated in our paper. Thus, in this respect, the protocol in [arXiv:2406.17569] is not very different from the one investigated in our paper. However the protocol in [arXiv:2406.17569] shows an interesting physics absent in our protocol: in the situation of [arXiv:2406.17569], in the merging region and on the side of the lower density gas, one would expect to observe a double-peaked rapidity distribution.

Now let us comment on the Referee's point that "in that work, the estimated equilibrium temperature appears consistent with the observed/GHD dynamics". The Drude weight investigated in [arXiv:2406.17569] is actually equal to

$$D_{nn} = \frac{n}{2m} \tag{1}$$

where $n$ is the linear density of particles and $m$ their mass. It does not depend on temperature.

Formula (1) is a simple consequence of Galilean invariance. The Drude weight is linked to equilibrium fluctuations of the particle current through (see e.g. formula (2) in [Ilievski and de Nardis, Phys. Rev. Lett. 119, 020602 (2017)])

$$D_{nn} = \lim_{\tau\to\infty}\lim_{L\to\infty}\frac{\beta}{2\tau L}\int_0^\tau dt\,\langle J(t)J(0)\rangle$$

where $J(t) = \int_{-L/2}^{L/2} j(x,t)dx$ with $j(x,t)$ the local particle current at time $t$. Galilean invariance implies that the particle current is the momentum density. Therefore the integrated current is

$$J = \frac{1}{m}P$$

where $P$ is the total momentum. $P$ is time-independent, so the Drude weight is

$$D_{nn} \;=\; \frac{\beta}{2m^2 L}\left\langle P^2\right\rangle \;=\; \frac{\beta n}{2m^2 N}\left\langle P^2\right\rangle$$

where $N$ is the total number of particles. To evaluate the fluctuations of $P$, one can rely once more on Galilean invariance. Under a Galilean boost $v$, the total momentum and energy transform as

$$P \to P - Nmv$$
$$H \to N\frac{mv^2}{2} - mvP + H.$$

In particular, since the boost is a symmetry transformation, $N\frac{mv^2}{2} + mvP + H$ has the same spectrum as $H$. This implies $\mathrm{tr}[e^{-\beta(H - m\frac{\lambda}{m\beta}P + Nm\frac{\lambda^2}{2m^2\beta^2})}] = \mathrm{tr}[e^{-\beta H}]$. Then

$$\begin{aligned}
\left\langle P^2\right\rangle &= \left(\frac{\partial^2}{\partial\lambda^2}\log\mathrm{tr}[e^{-\beta H+\lambda P}]\right)_{\lambda=0}\\
&= \left(\frac{\partial^2}{\partial\lambda^2}\log\mathrm{tr}[e^{\beta Nm\frac{\lambda^2}{2m^2\beta^2}}\,e^{-\beta(H-m\frac{\lambda}{m\beta}P+Nm\frac{\lambda^2}{2m^2\beta^2})}]\right)_{\lambda=0}\\
&= \left(\frac{\partial^2}{\partial\lambda^2}\beta Nm\frac{\lambda^2}{2m^2\beta^2}\right)_{\lambda=0} + \left(\frac{\partial^2}{\partial\lambda^2}\log\mathrm{tr}[e^{-\beta H}]\right)_{\lambda=0} = \frac{N}{m\beta}.
\end{aligned}$$

This gives formula (1) for the Drude weight.

Alternatively, one can use the first protocol of [arXiv:2406.17569] to derive the same result. This protocol consists in probing the current response to a constant gradient $V(x) = \phi_n x$, corresponding to a constant force $-\phi_n$. Eq. (2) of [arXiv:2406.17569] gives the Drude weight as

$$D_{nn} = \lim_{\phi_n \to 0} \lim_{t \to \infty} \frac{1}{2} \frac{j(x,t)}{t\phi_n} \,. \tag{2}$$

[There is a mismatch of a factor 1/2 in Eq. (2) of [arXiv:2406.17569], as compared to the standard definition. We reinstate that factor 1/2 here.] The effect of the force is cancelled when going to the accelerated frame with acceleration $\phi_n/m$. In that frame the particle current is zero. Going back to the lab frame, we see that the current is $j(x,t) = n(\phi_n/m)t$. This gives again $D_{nn} = n/(2m)$.

We also point out that a recent by Göhmann, Klümper and Kozlowski [arXiv:2506.00686] gives the same arguments for formula (1).

## 2 Answer to referee 2

We thank Referee 2 for their careful reading of our manuscript and for their comments. We are happy to see that they find this work interesting and that it deserves publication. We answer their comments below.

1. *Since this work combines experiments and GHD, it is surprising seeing some previous interesting papers on this matter not showing up in the references. I think they should be added, in particular the first two papers on the matter: -Phys. Rev. Lett. 122, 090601 (2019) - Science373,1129-1133(2021) And the other experimental papers employing the rapidity measurement protocol (I do not mention those already included in the manuscript) -Science 367, 1461 (2020) -Science 385, 1063 (2024) -Phys. Rev. A 107, L061302 (2023) -arXiv:2505.10550 (2025)*

   Thank you for pointing out that we had omitted those references. We added them in the new version, at the beginning of Sec. 3 and Sec. 6.

2. *There is a choice of notation that is confusing, namely the use of "calligraphic v" -or $\nu$, it is difficult to decipher from the pdf- for the filling function (Eq 1), for the effective velocity (Eq 3a) and for the ray x/t (above Eq 4). I think this may be confusing and it would be better introducing different symbols. A notation I have seen several times is $\vartheta$ for the filling function and $\zeta$ for the ray, but other choices are equally fine as long as the clarity is improved.*

   Thank you. We have changed the notation for the 'ray', or velocity, from $v = x/t$ to $\zeta = x/t$ in order to avoid possible confusion with the filling factor $\nu$. We prefer to keep the letter '$\nu$' for the filling factor, as opposed to '$\vartheta$', since we use the letter $\theta$ for the rapidities, so there would be a conflict between $\vartheta$ and $\theta$.

3. *It is the first time I see entropy modifications (see Eq 13 and discussion) to describe experimental data and it is very interesting. The authors add a "one-parameter deformation" associated to the "c" parameter in Eq. 13. The natural question is: why do the authors chose this deformation, and not another equally simple one? It would be nice if the authors could share the thoughts that inspired this choice.*

Thank you for suggesting this. Our motivation for this Ansatz is coming from the fact that we know that the initial state is stationary in the trap, and the class of stationary states in the trap can be parameterized by such an entropy functional. This is a result of Vir Bulchandani, see Ref. [53], which we adapt here. In the revised version, we added a discussion of our motivation for this ansatz in the paragraph Eq. (13).

4. *Below Eq 13, the authors report the value of the fitted temperature and chemical potential obtained in the presence of the deformation Eq. 13. Since this is not a thermal state any longer, I feel that calling these parameters temperature and chemical potential is deceiving and a different notation should be used, or at least a comment should be added.*

We have changed the notations to fitting parameters $a, b$ and $c$, see Eqs. (11)-(13) in the new version.

5. *Bottom pg 10: is the temperature T=560nK obtained with the deformed entropy? If it is so, it should be specified.*

The temperature 560 nK in section 6 is obtained by the thermal fit. With the fact that we no longer use the term "temperature" for the non-thermal fit, we think it is now clear. See also the reply to Referee 1, point 1(e).