Reply to the referees for manuscript scipost_202204_00001v1: A splash in a one-dimensional cold gas

Dear Editor,

We thank both the referees for their constructive criticisms, to which we provide a response below. We are submitting a revised manuscript where we have made some changes in response to the comments of the referees. All changes are marked in blue in the revised manuscript.

Regards,
Subhadip Chakraborti, Abhishek Dhar and P. L. Krapivsky

Response to Referee 2.

In this paper, Chakraborti, Dhar and Krapivsky study the splash problem in a one-dimensional cold gas of point particles with binary (alternating) masses. In particular, they study the cascade of activity that results from the excitation of the leftmost particle with a fixed velocity in an otherwise frozen environment. They find (1) a sub-ballistic shock front propagating into the cold gas and separating it from a thermalized region, well-described by self-similar solutions to the Euler hydrodynamic equations, and (2) a non-hydrodynamic splash region formed by recoiled particles moving ballistically in the opposite direction. The authors offer heuristic derivations of the main scaling relations relevant for this problem, as well as self-similar solutions of the second kind to the Euler hydrodynamic equations governing the thermal part of the shock wave. They complement these analytical results with extensive molecular dynamics simulations of the microscopic model, finding excellent agreement with predictions in all cases.

In my opinion this is a very nice paper, as otherwise usual for these authors. I find the results reported in this paper very interesting and relevant for a broad community of physicists interested in the transport properties of low dimensional systems. The paper is well-written, all results are explained first at the heuristic level and then supported with hydrodynamic solutions, together with numerical simulations confirming the overall picture. Moreover, some of the results reported are intriguing, as e.g. the fact that the splatter asymptotically concentrates most of the energy in the system, with the energy of particles in the positive half-line (x¿0) decaying algebraically in time. The presence of a hydrodynamic, sub-ballistic shock front coexisting with a non-hydrodynamic ballistic splatter also seems intriguing.

For these reasons, I recommend the publication of the present manuscript. I have however a number of comments and questions that may help to improve the manuscript:

Comments and Questions

1) After reading in detail the paper, I keep wondering: Does dissipation play any role in the splash problem? In particular, do you expect thermal conduction and viscous dissipation effects to play a role in this problem? The authors mention the blast problem of Refs. [16,17] and its description in terms of the Taylor-von Neumann-Sedov (TvNS) self similar solution to the Euler equations. There it was shown that dissipative corrections were important near the blast core region. Can something similar happen in the splash problem? If so, what type of corrections do you expect in this case? A comment on this issue would be more than welcome.

Our response: Indeed, after discovering of the importance of the dissipation in the blast problem in the core region, we initially suspected a similar phenomenon in the splash.

In the blast problem the core is a region with a large temperature gradient and this leads to the fact that dissipative heat conduction terms are important in this region. On the other hand, in the splash problem, there is no such region. The initially excited particles and an inreasing number of other particles have their velocities frozen after a finite number of collisions and keep going away from the bulk. They then form, what we call the splatter, a non-hydrodynamic region. The bulk region $x > 0$ thus does not have any region with high temperature gradients and so nowhere do the dissipation terms become significant. The deficiency of the Euler framework for the blast problem is most obvious after noting that it gives INFINITE temperature at the origin. Dissipation cures this. Nothing diverges in the splash problem.

We have now added a sentence on this point in the conclusion section.

2) My next comment is directly related to the previous question. In particular, it is also well-known that transport is anomalous for this 1d particle system, with a heat conductivity that diverges as a power law of the system size. Does this anomalous transport properties affect in some way the results described in this paper?

Our response: This is a an important but very difficult issue. The divergence of thermal conductivity suggests that the correct form of the heat conduction terms, in the hydrodynamic equations in one-dimensional systems, should have a non-local form. We believe that for the blast problem, such non-local equations might explain the core profile (not understood at the Euler level) completely. On the other hand, our results for the splash suggest that these terms do not play a role on the mean profile in the long time limit. These could play a role on more detailed questions such as deviations from local equilibrium (see response **3** below).

3) I'm curious about how hydrodynamic profiles relax behind the shock front, specially in the asymptotic far tail. In studies of shock wave propagation for the same model excited with a piston moving at constant velocity (see Ref. [14] in the paper), it was found that hydrodynamic profiles relax algebraically far from the shock front, in contrast with standard hydrodynamics predictions. Can something similar happen in the far tail of the hydrodynamic region for the splash problem? What are the predictions obtained from the scaling solution of the second kind to the Euler equations in this case? What is the observed asymptotic relaxation in the molecular dynamics simulation results? It would be interesting to include a comment on this issue in case the authors have enough data in this region.

Our response: We thank the referee for raising this interesting point: The shock is an obvious place where hydrodynamics fails, one needs at least the Boltzmann equation description and perhaps even more microscopic approaches. And the tail probably exhibits interesting behaviors.

However, as far as we can see, we do not observe the departure from hydrodynamic predictions, of the kind seen in Ref[14] where the system with a moving piston was studied. We believe that the constant energy injection in the case of the piston problem could lead to the differences with our set-up. For example the piston case has a ballistic shock front which spreads diffusively, while in both the splash as well as the blast problem that we consider, we have a sub-ballistic front which does not spread with time. The constant energy injection could be a possible reason for the strong deviations from hydrodynamic behaviour observed in [14]. Our scaling solutions (second kind) of the Euler equations are obtained numerically and we see very good agreement with our simulation results except of course for the splatter particles which are anyway not described by hydrodynamics.

In our earlier study on the blast, we also looked at the departure from local thermal equilibrium (Ref [17]) but did not find any significant deviation. Studying tails is more subtle and we leave this for the future. As mentined above, we also believe that the piston problem where they were studied [14] and problems with constant injection of energy are better settings for this question.

4) Mass ratio: In order to compare analytical predictions with molecular dynamics results, the authors choose particular values for the light and heavy particle masses, $m = 2/3$ and $M = 4/3$, which result in a mass ratio $\mu = 1/2$. The value of this mass ratio affects relaxation timescales in the alternating hard point gas, with $\mu = 1/2$ corresponding approximately to fastest relaxation. Is this the reason why the authors choose μ? What happens for other mass ratios?

Our response: It is true that we chose $\mu = 1/2$ has a fast relaxation. We have verified the scaling results shown in Fig. (5) for two other mass ratios, $\mu = 1/3$ and $1/10$, and found that the scaling exponents remain the same, only the prefactor α depends on μ as expected. These are not all presented in the paper but we now mention this at the beginning of Sec. (4). We presented some results for the mass ratio of $\mu = 1/3$ in Fig. (4b) where our prediction on the dependence of mass arrangement was accurate for both sets of mass ratios.

Minor comments:

1) The exponent δ characterizing the growth of the hydrodynamic region in the splash problem takes a value $\delta = 0.6279...$, smaller than the equivalent exponent in the blast problem (2/3). Since both problems essentially differ in their symmetry properties, is there any symmetry argument supporting $\delta < 2/3$?

Our response: We are not able to think of any symmetry argument. However, note that the most basic TvNS formula in 1d, $R \sim (Et^2)^{1/3}$, along with the fact that the energy in the system leaves with the splash particles, already suggests that we should have $\delta < 2/3$. This is the content in fact of Eqs. (4) and (6) in the manuscript.

2) For the interested reader, and in order to make the paper more self-contained, it would be nice to have more details on the derivation of Eq. (5) using dimensional analysis (without having to resort to Ref. [16]).

Our response: We have now explained the origin of Eq. (5) [Eq. (6) in current version] in the paragraph before this equation.

3) Typo: "bbut" right after Eq. (5).
Our response: We have corrected this.

4) In the next paragraph below Eq. (21), third line, the authors mention constant α but I couldn't find it in the main text. Similarly, in the second paragraph of section 4 (Numerical Results), a value of $\alpha = 2.08$ in Eq. (6) is mentioned, but there is no α in Eq. (6). The caption of Fig. 3 suggests that α is just the amplitude of shock position with time. Defining α in the main text would probably clarify the discussion.

Our response: We have now displayed Equation (1) which defines α.

5) In the line below Eq. (25), I would suggest to add the explicit numerical value of exponent Δ obtained from exponent β.

Our response: We have added this.

6) Fig. 3: The agreement between the scaling predictions for the shock position, the total energy in the positive half-plane, and the total number of collisions, panels 3.a-c, is excellent for all times explored. However, the agreement for the total momentum in the splatter region (panel 3.d) is only asymptotically good for long enough times. Is there any particular reason for this discrepancy?

Our response: It is not clear to us why $\mathcal{P}_{\text{splatter}}$ data takes longer times to show the expected asymptotic form. For clarity, we have now shown data for longer times and plotted the data for all four plots on the same time range.

7) Caption of Fig. 4, fourth line: The sentence containing "... that the ratio N_+ values obtained from ..." seems difficult to understand.

Our response: We have now modified this sentence.

8) Fig. 5: Please make this figure bigger (maybe a panel with 2 columns and 3 rows would make it).

Our response: We have modified the arrangement of both Figs. 5 and 6.

Response to Referee 1.

Strengths
1) The work is well done and based on analytic calculations
2) The phenomenon described in the paper is intriguing (non-intuitive)

Weaknesses
The main and only weakness I see is the peculiarity of the problem: perhaps too special a setup to draw general conclusions

Report
I am unsure about the suitability in this journal, because the authors deal with a very special initial condition. If the authors are able to justify the general relevance of their study (showing for instance some form of robustness), my doubts would disappear.

Our response: We would like to point out that the problem we treat here falls in the broad class of problems where self-similar scaling solutions appear for dynamical systems, but where exponents do not get fixed by dimensional arguments. This leads to anomalous dimensions and these are referred to as scaling solutions of the second kind.

This has been discussed earlier in the context of phenomena in fluid dynamics such as fluid flow in porous media (discussed for example in G.I. Barenblatt, *Similarity, Self-Similarity and Intermediate Asymptotics* (1979) and N. Goldenfeld, *Lectures on phase transitions and the renormalization group*, chapter 10. In the present work we present a simple example where we start with a microscopic model and the hydrodynamics treatment naturally leads us to consider solutions of the second kind. In our example we are able to make a direct comparison between results obtained from microscopic simulations and from hydrodynamics.

The splash consists of a hydrodynamic and a non-hydrodynamic region (what we call the splatter) and these couple to each other. What is completely new is that we are able to obtained detailed predictions for the non-hydrodynamic region by using the scaling solution. Indeed we do this for a specific problem but this is already quite non-trivial and we believe that this work will generate interest to consider more interesting physical problems such as splashes in higher dimensions.

We have now added a paragraph in the introduction and a sentence in the conclusion which describes the importance of this paper and explains why it is of more general relevance, and has implications beyond the specific case treated in this paper.

Requested changes

Remark 1: Are the authors able to show that this scenario is in some sense robust?

Our response: We hope the explanation given above (and included as a paragraph in the introduction of our paper) is sufficient to convince the referee of the broader implications of our work.

Remark 2: While studying heat conductivity in the diatomic hard point gas, some past papers suggested that the special values of the mass ratio (special beyond the equal-mass case) might display a qualitatively different behavior.
In this work the mass ratio seems to be an innocent parameter" playing no special role. I would like to read a bit more on its role.

Our response: We have verified the scaling results shown in Fig. (5) for two other mass ratios, $\mu = 1/3$ and $1/10$, and found that the scaling exponents remain the same, only the prefactor α depends on μ as expected. These are not presented in the paper but we now mention this at the beginning of Sec. (4). We showed some results for the mass ratio of $\mu = 1/3$ in Fig. (4b) where our prediction on the dependence of mass arrangement was accurate for both sets of mass ratios.

We would like to point out that even in the case of anomalous heat conduction in 1D systems, it is believed that all mass ratios lead to univeral exponents, only that the cases of the mass ratio being close to unity or being very large, lead to long cross-over length scales. See for example one recent work by **Chen, Wang, Casati and Benenti Phy. Rev. E 90, 032134 (2014)**, where it is shown that all mass ratios lead to the same heat conductivity exponent when one looks at large enough systems.

We have now mentioned in the 1st para of Sec. (4) that our main results do not depend on the mass ratio μ and that we have verified them for two other values of μ.

Remark 3: It is not entirely clear the way the initial condition is selected. Except for the leftmost particle, all are at rest, but what about the initial position?

Perhaps I have missed the point where this point is mentioned; however, even if I am wrong, it should be discussed more extensively, especially to comment about potential differences.

Our response: The positions of the particles are initially distributed uniformly over the positive half-line $x > 0$. This is already mentioned in the paper in several places including the abstract, in the introduction and elsewhere. We have now highlighted in blue color this part in Sec. (2). We have also added now a sentence mentioning that the only interaction potential among the particles is the infinite point interaction. We have tried to make the definiton of the dynamics more clear.

Remark 4: When the parameter beta is first mentioned, it would be useful to briefly anticipate how small it is to help the reader intuition.

Our response: The upper bound $\beta < 2/3$ is intuitively obvious as one expects the shock wave to propagate, so δ [in Eq.(7)] must be positive. Thus $0 < \beta < 2/3$ is expected. The actual β is small. We do not have a heuristic, let alone rigorous, understanding why it is small. In fact, the actual β is so small that, based on numerics, one could have suspected a logarithmic decay like $E \sim 1/\log(t)$. However, our theoretical approach IMPLIES an algebraic decay. This is the virtue of the theory; the smallness of β, however, cannot be guessed (to the best of our knowledge) without actual analysis (an analytical solution of a non-linear eigenvalue problem).

Remark 5: When the function A(mu) is first mentioned, it would be useful to anticipate the expected dependence (this point is connected to point 2).

Our response: Unfortunately, we do not have any clear understanding of the dependence of A on μ. We have numerically evaluated this over a range of values of μ and see a montonic increase of $A(\mu)$ with $\mu \in (0, 1)$. Given that we do not have any understanding, we are not including this in the manuscript.

Remark 6: In Eq. (14), it would be useful to make it more explicit that it is a definition of V.

Our response: We have now done this for V, as well as for the other scaling functions G and Z.

Remark 7: It is not clear how Eq. (15) comes out and what is the meaning of Z.

Our response: We have now made it clear that G, V, Z are the scaling functions and have indicated explicitly, at various places, the equations and boundary conditions which will determine them.

Remark 8: Coming to the numerical part, the agreement is always very good except for panel 3d. The authors should add some comments especially on the amplitude of the deviations.

Our response: We have now plotted the data in all four panels in Fig. 3 over the same time range and it is now seen that at longer times, there is good agreement with the analytical predictions, even for (3d).

Remark 9: Finally, about the splatter particles. I understand that it is utterly difficult to prove rigorous statements. In fact, the authors limit themselves to proposing conjectures.

I am perfectly fine, but I have a curiosity: do they have any idea (perhaps just based on numerics) on the position of the last collision of the particles that progressively enter this region?

Our response: We have now added a proof of conjecture **C2** for the special case $\mu < 1/3$, which is not so difficult. Indeed, the general case is very difficult

On the second issue, we thank the referee for this very interesting question. We evaluated this quantity numerically, but are not able to understand the data in the same way, even heuristically, as for example, for the other quantities in Fig.(6). Roughly, we observe that the position X and time T of the last collision satisfy $X \sim -T^{\delta}$. Because of our lack of any understanding, we are not adding this result to the paper.