Response to the report of the Reviewer-1

We greatly appreciate the constructive remarks and the suggestions made by the referee. These remarks are addressed as follows.

1. The author correctly points out that microscopic derivations of energy superdiffusion have already been obtained for this model. However, he does not comment on these except for saying that the derivation presented here is "simpler". I think it would be necessary to add a more in-depth discussion comparing the previous studies to the current one. This would also be a nice occasion to discuss the advantages of the new derivation. This point is particularly important since part of the derivation relies on some heuristics (Eq. (38)), which is very reminiscent of NLFHD, which can already provide a very concise derivation of some aspect of heat superdiffusion.

Response : We have added the below discussion comparing our study with the previous derivations of the super-diffusive equation (see the second page of the introduction.)

'The procedure followed in [PRE 98(4), 042105 (2018)] involves finding coupled differential equations for the correlation and the temperature fields in open system set-up starting from the equations of the microscopic two point correlations $\langle \eta_i(t)\eta_j(t)\rangle_c$ (subscript 'c' represents connected correlation). Integrating the correlation field provides a non-local evolution for the temperature field. The approach in [Nonlinearity 25(4), 1099 (2012)] also involves analysing the scaling properties of the (microscopic) energy-energy correlation with complete mathematical rigor. In particular it was shown that after space-time scaling the energy-energy correlation function (on infinite line) is given by the solution of a skew-fractional heat equation with exponent $\frac{3}{4}$.'

We also made the below comments on the advantage of our method in the context of the HCVE model and discussed the connection with the NLFHD theory [see the new paragraph before Eq. (38)].

'It is interesting to note that the equation (37) is same as the fluctuating HD equations that one starts with in the NLFHD theory. For generic Hamiltonian system one finds non-linear fluctuating HD equations. For the particular model studied here, the fluctuating equation for the volume field is linear whereas the corresponding equation for the energy field is non-linear. In the NLFHD theory one finds scaling forms for the space-time correlations of the density fields of conserved quantities through mode-coupling solutions. Such solutions in many cases exhibit super-diffusive scaling which, through linear response theory also predicts super-diffusive evolution of small initially localised excitations in the conserved fields. We point out that our starting point is different from the NLFHD theory. We start from the FP equation and seek solution of it that is in the LE form and always remain close to an underlying GE state. As usually done in fluid hydrodynamics, we compute the averages of the conserved fields and the associated currents (related via continuity equations) with respect to these solution. Invoking certain physical assumptions, we have demonstrated that the contribution from the deviations from the LE state to the average current indeed comes from the time-integral of un-equal time correlations of currents at different locations. Note such local current-current correlations are not usually studied in the NLFHD theory, instead one often studies the total current-current correlation. The particularly simple (linear) form of the fluctuating equation satisfied by the volume field in our model allows us to compute this correlation and its time integral analytically, both for the closed and open-system (with reservoirs) set-up. The case for the closed system set-up is discussed in the next, whereas the case with reservoirs attached to the system is discussed in sec. 4.'

2. I think the phrasing of the second line of Eq. (1) is not very clear. In fact, I did not understand what the model was until Eq. (11). It would be helpful if the author could add a line below Eq. (1) to further clarify its meaning.

Response : We have modified the second line of Eq. (1) as follows and clarified its meaning.

$$\begin{aligned}\dot{\eta}_i &= V'(\eta_{i+1}) - V'(\eta_{i-1}) \\ &\quad + \text{stochastic exchange at rate } \gamma\end{aligned} \tag{1}$$

for $i = 1, 2, ..., N$, where $V(\eta) = \frac{k_o}{2}\eta^2$ with $k_o > 0$. The second line in the above equation represents stochastic exchange between η variables across a bond with rate γ, independently for each bond $\{i, i+1\}$.

3. In the introduction, in the paragraph starting with "In the second part of the paper [...]" the author discusses the different behaviour present in a mesoscopic and macroscopic system. By reading the paper, I think the author means that there is a crossover scale N_C, such that, if the system size $N < N_C$, transport is well described by Fourier law, while, if $N > N_C$, transport is anomalous. If so, please phrase this more explicitly directly in the introduction. Otherwise could the author please clarify this point?

Response : We have added the below discussion on the crossover scale N_c in the introduction (2nd page). We also have added new citations [20 and 24] to this discussion.

'More precisely, one would expect to have a crossover length scale N_c depending on the microscopic parameters and the underlying equilibrium state (density, temperature), such that the dominant mechanism of transport is diffusive for systems size $N < N_c$ and the transport becomes anomalous for $N > N_c$. Existence of such crossover behaviour has been reported earlier [19-24].'

4. (Optional) The way the argument is phrased, the author starts from the exact equation (15) and only much later the LE part of the evolution is discussed. I think it would make the manuscript more pedagogical to start with Sec. 3.1 and then discuss how to improve on top of it by taking into account the deviations described by P_d.

Response : We have decided to keep the presentation as it was originally. Somehow, we felt it is better to discuss the general scheme (as presented in sec. 3) and then discuss the contribution from the LE state and the modification from the deviation P_d separately in subsequent sections, 3.1 and 3.2 respectively.

5. The paragraph above Eq. (22) states that the ansatz in Eq. (12) is valid when P_d is small. Could the author please clarify what this means, as precisely as possible after Eq. (12). Which exact assumptions are needed for the derivation to go through?

Response : We thank the referee for this remark. We have now added a discussion clarifying what we mean by P_d small and clearly stated the assumptions that are required for the derivation to go through. Formally the solution in Eq. (11) is exact. However, since we are interested in linearised hydrodynamics, it is sensible to assume that the deviations from the global equilibrium characterized by $\tilde{T}_i(t) = T_i(t) - T_0$ and $\tilde{\tau}_i(t) = \tau_i(t) - \tau_0$ and their space-time variations are small so that the system always remains close to a LE state which is slightly deviated from the GE state. Equivalently, one can say that the deviation P_d, which depends on $\tilde{T}_i(t)$ and $\tilde{\tau}_i(t)$ and their space-time derivatives (see Eq. (16)), is also small. These assumptions, are used later in sec. 3.2 [see discussions between Eqs. (30) and (32)], where we neglect terms involving higher order in deviations as well as higher order in derivatives.

6. Figure (2), panel (b), shows a scaling collapse trying to give numerical evidence for superdiffusive scaling. The data is however not very convincing at the moment, as the time goes from $s = 70$ to $s = 80$. I think it's very hard to determine the dynamical exponent from such a small time window. Could the author please justify why such a small time window is used? If the range cannot be extended (for reasons that should be explained), one way to make the numerical data look more convincing would be to present in an inset or an appendix an attempt at collapsing the same data with different exponents near 2/3 (including 1/2) and show that their agreement is much worse.

Response : We have now updated the scaling collapse plot in figure 2 with new data for larger system size and also over larger time window. With the computing resources available, it was difficult to generate data for system sizes larger than $N = 512$ and for a given system size data for larger time gets affected by finite size effects (as the simulations are done on a ring). We have now provided both diffusive and super-diffusive scaling collapse. For the small k_o case, it is clearly evident that diffusive collapse is better than the super-diffusive scaling (top panel). On the other hand for the large k_o, one can observe that the super-diffusive scaling is better than the diffusive one (bottom panel).

7. Many acronyms are used in the manuscript, and some of them appear very few times, e.g. LR appears only 5 times. It would help clarity to expand the acronyms that are not used often.

Response : We have tried to remove the acronyms that are not frequently used.