Re: Report 1 submitted on 2023-04-10 on scipost 2211.01137v2.

Title: "Quasi-localized vibrational modes, Boson peak and sound attenuation in model mass-spring networks"
Authors: Shivam Mahajan and Massimo Pica Ciamarra

Dear reviewer,

We thank you for the additional comments on our manuscript, and have clarified the points of concern in the revised manuscript.
We include in the following a detailed answer to all the remarks and the list of changes.

We hope that our revised manuscript is now suitable for publication in Sci Post.

Your sincerely,
Massimo Pica Ciamarra

List of changes:

1. We made minor changes to improve clarity, as the reviewer's comments suggest we did not properly define some of the quantities we studied.

2. We revised Fig. 7. Following the reviewer's suggestion, we changed the procedure to bin the data and show results for selected values of $f = 0.0$, 0.05, 0.1 and 1.0.

Referee #1:
Although I feel that the article should be published in SciPost, there are still some points that need to be addressed.

> **Reply:** We thank the reviewer for the additional comments which address them in the following.

Requested comments:
Shear modulus fluctuations/correlations: If I understand correctly, the method to extract the spatial map of the shear modulus in both V1 and V2 is the same, i.e. an affine deformation followed by an energy minimization with a stress field coarse-grained on a scale w.
Authors present the scaled σ_w in Fig.4(a) but the curves (especially for f=0) have now changed between V1 and V2. Why so?

> **Reply:** The small differences the reviewer noticed originate from the change in the size of system studied: V1 of the manuscript presented results for $N = 160000$, while V2 focuses on $N = 360000$. The asymptotic values of the normalised fluctuations, γ, do not exhibit any significant size dependence, as shown in Fig. R1.

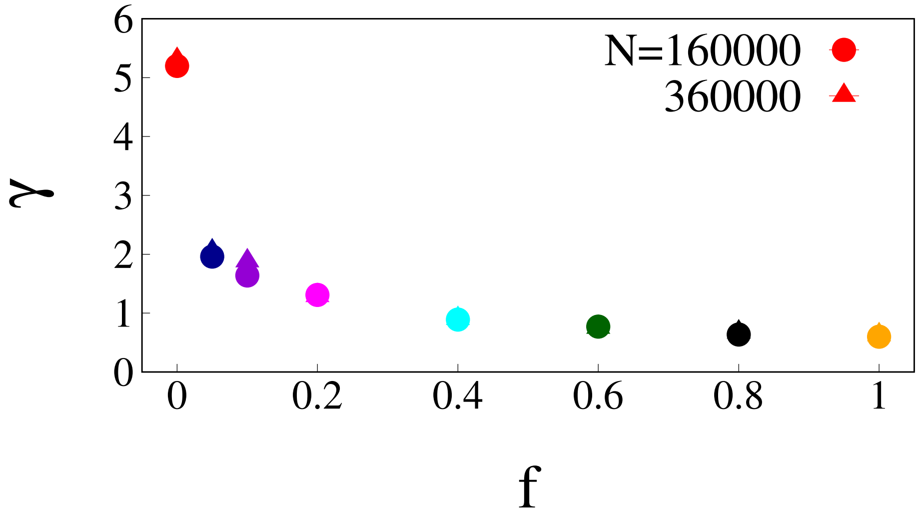

Figure R1: Dependence of the disorder parameter on f, for two values N values.

Authors now report the $P(\mu_i)$, where μ_i is the shear modulus at the particle level. On which scale w is it calculated? Should $P(\mu_i)$ show more asymmetric tails between low and high shear moduli? Does $P(\mu_i)$ exhibit the correct asymptotic scaling for small μ_i?

> **Reply:** As stated in the main text, the figure reports the distribution of a shear-modulus defined at the single particle level, as in H. Tong, S. Sengupta and H. Tanaka, Nature Communications 11, 1 (2020). The same distributions are obtained using a small coarse-graining length scale $w = a_0$.
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> The small asymmetry of the distributions appears to be consistent with those reported in previous studies, e.g. Mizuno, H., Mossa, S., Barrat, J-L, Physical Review E 87, 042306, 2013.
>
> We cannot address the last question posed by the referee, as we are unsure what the reviewer has in mind when referring to the 'correct asymptotic scaling'.

Authors claim that the spatial correlation of μ_i shows long range correlations, which to my knowledge has not been highlighted in PHYSICAL REVIEW E 80, 026112 (2009) and other related studies. I am confused about Fig.3(b) and the discussion around it. It starts by citing Ref.50 which only focuses on stress correlations, although the text reads “long-range correlations in the local shear modulus”, same for Ref.56. Could you please either change this statement or explain it a bit better.

> **Reply:** The reviewer is right in saying that Ref. 50 focuses exclusively on stress correlations. However, the other papers we cite when discussing this point show the presence of analogous correlations in the shear modulus. We refer the reviewer to Fig. 4 in H. Tong, S. Sengupta and H. Tanaka, Nature Communications 11, 1 (2020) for 2D results, and Fig. 6 of our own Phys. Rev. E 103, 052606 (2021) for 3D results. In all cases, the correlation function exhibits an anisotropic power-law decay as the one we report.

The text reads “the anisotropy of these correlations ensures that $\langle\mu(0)\mu(r)\rangle = 0$ at all r”, which quantity is then reported in Fig.3(b)?

> **Reply:** In Fig.3b, we plot the correlation function defined in Eq.3, as stated in the main text. Unlike the usual radial correlation function, this function accounts for the quadrupolar symmetry of the shear modulus. It reads: $C_{\mu_i}(r) = \frac{\langle\mu(0)\mu(r)\rangle-\langle\mu\rangle^2}{\langle\mu^2\rangle-\langle\mu\rangle^2}\cos(4\theta)$.

Authors states “the fluctuations of the shear modulus of N_w particles enclosed in a compact volume are insensitive to anisotropic correlations and scale as if there were no correlations, Eq. 2” Should then the data in Fig.4(a) be flat at all scales w?

> **Reply:** No. According to the CLT, the average $\frac{1}{N}\sum^N \mu_i$ of $N \propto w^d$ iid random variables taken from a distribution $p(\mu_i)$ *asymptotically* approaches a normal distribution with constant variance. How many elements one has to sum for the variance to be constant depends on $p(\mu_i)$. Fig.4(a) is not flat for all scales w as $p(\mu_i)$ (Fig. 3a) is not a normal distribution.

Glassy length scale:
Previous works (including many contributions by the authors) in both 2D and 3D have also put forward the relation between $\xi \sim 1/\omega$, where ω can be viewed as the typical excitation frequency or Boson peak frequency. In the present study ω_{BP} only changes by a factor 2-3, accordingly, one needs to look for a factor 2-3 decrease of the glassy length scale as a function of f. I genuinely do not believe that neither Fig.3(b) nor Fig.6 provide for the evidence that ξ is constant.

> **Reply:** As the reviewer suggests, a relation between ξ and ω has been reported in previous works, and we have reported it in a model 3D glass. However, a variation in the Boson peak frequency ω does not imply a change in ξ. This is so as $\omega = \omega(\xi, \gamma)$: ω varies at constant ξ if γ changes. Indeed, fluctuating elasticity theory, which previous results and those we report suggest accounts for sound attenuation in 2D (up to a prefactor), predicts ω changes with γ at constant correlation length.

As mentioned above I would first clarify what is plotted in Fig.3(b), and then only report f=0 and f=1 for clarity. We cannot appreciate if ξ is constant when plotting all values of f in log-log scale.

> **Reply:** As mentioned above, Fig 3b illustrates the correlation function of Eq. 3. To clarify from this figure that our data do not support the presence of a f dependent length, we focus on $f = 0$ and $f = 1$ as suggested by the reviewer. Fig R2(a) shows that the correlation functions of the local shear modulus reasonably collapse when plotted versus r/a_0. Conversely, if we assume that there is a length scale changing by e.g. a factor of 2 as argued by the review, $\xi(f = 1) = \xi(f = 0)/2$, the collapse is lost, as in panel (b). The result is apparent and clearly appreciable in the current representation of the data. Summarising, our data do not support the presence of a varying length scale.

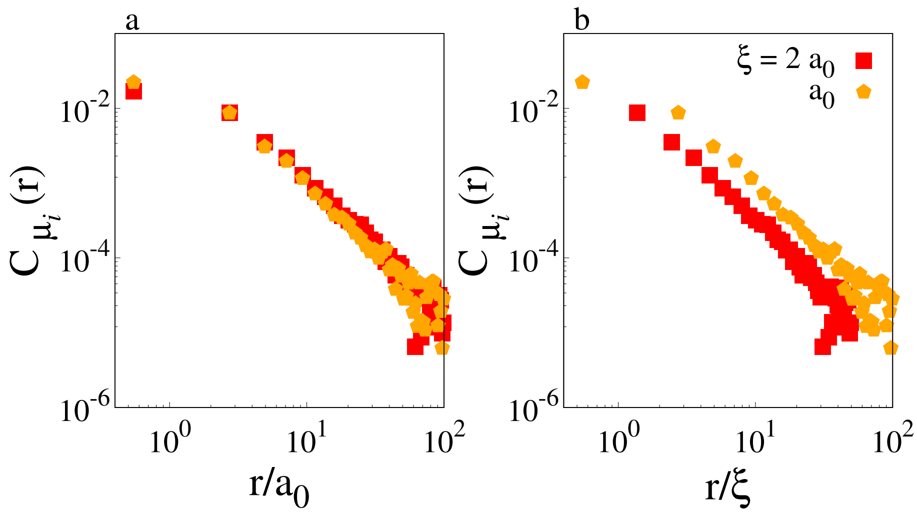

Figure R2: Panel a illustrates patial correlation function of elastic constants as a function of r/a_0. In panel b, we show the correlation function normalized by ξ whose variation is suggested to be of a factor 2-3.

Additionally, Fig.4(a) shows that the data for f=0 converges more slowly than f>0. One expects such a result, as the spatial extent of non-affinities scale with ξ. I find results in Fig.4(a) consistent with a decrease of ξ as f increases as shown in the V1.

> **Reply:** As discussed above, the features of the single-particle shear modulus $p(\mu_i)$ regulate the convergence of Fig. 4a to a plateau. At small f the distribution is more asymmetric, and convergence occurs at slightly larger w.

The quality of the data of Fig.6 is not satisfying enough to draw any conclusion either. Inspecting closely the data, one actually finds a decrease of the modes participation ratio between f=0 and f=1. Again, we are looking for a factor 2-3, which on the y-log scale of Fig.6 cannot be ruled out. I do not understand how the error bars are computed in Fig.6. There is a lot of fluctuation and some points have very large errors and some don't. I do understand the computational difficulty and therefore I surely do not ask authors to improve the statistics, but I would at least only report average Np values for bins that contain at least 10 modes and evaluate the spread as a function of the frequency. One could also only report f=0 and f=1 for clarity if it helps.
I would also add a note of caution in the main text, because the participation ratio is ill defined in 2D due to the log correction, i.e. Np is size dependent.

> **Reply:** The reviewer certainly refers to Fig. 7, which reports data for Np. In this figure, we illustrated error bars for selected points.
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> We have followed the reviewer's suggestion and changed our binning strategy. We now ensure that each bin contains at least 25 data points. If ξ varies by a factor of 2, the Np should vary by a factor $\xi^2 = 4$, assuming the localised modes to be compact. The revised data demonstrate that Np varies by a factor 1.25 at most.
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> The paragraph discussing Fig. 7 already mentioned previous results discussing the dimensionality dependence of Np. We have revised the paragraph to ensure this point is properly conveyed.

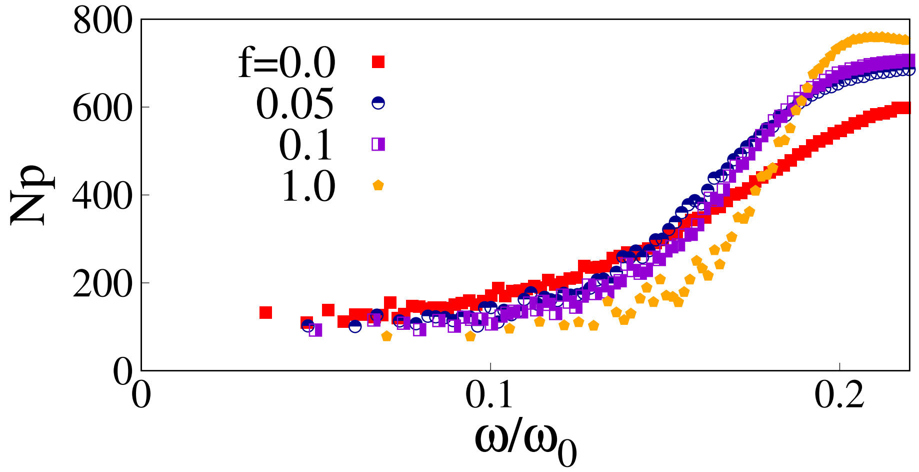

Figure R3: Participation ratio multiplied by $N = 1024$ for $f = 0.0$, 0.05, 0.1 and 1.0. Error bars are of the order of 50.