Referee 1

We are grateful to the referee for their comments and time. Below is the list of our revisions in response to the comments and our reply.

1. In equation 2 why aren't the authors using a dressed mass for the electron?

This is an interesting question. We calculate the minimal viscosity in Ref. [24] using two key parameters in condensed matter phases: the Bohr radius and Rydberg energy setting the characteristic values of the interatomic separation (on the order of Angstroms) and the cohesive energy (on the order of several and up to 10 eV). The Bohr radius and Rydberg energy depend on the bare electron mass.

A dressed mass can substantially differ from the bare mass (from 0.01 of bare mass in semicondutors to 100 bare masses in heavy fermions), however this is often related to electron mobility (in particular of electrons lying close to certain parts of the Fermi surface) where electrons are viewed as quasiparticles and acquire a dressed mass. This dressed mass can also depend on a specific experiment, e.g. electronic transport and magnetic properties may involved different dressed masses.

One could in principle refine our calculation and use the dressed mass appropriate to our effect, however this would be challenging because dressed masses due to interaction effects are known in fairly simple solvable models only, whereas in Ref. [24] we considered systems with nontrivial electron interaction effects including hydrogen bonding where full quantum-mechanical calculation is needed to account for the effects of interaction and screening.

There is often a compromise between having a simplified model and ensuing universality of the resulting equations and a more refined model where universality is lost in favor of increased precision. Our main intention in Ref. [24] was a demonstration of the characteristic value (order-of-magnitude estimation) of minimal viscosity, hence we used the bare electron mass.

2. I found the usage of $\geq$ in places like eq 4 and 5 grossly misleading. For instance in defining ρ the authors drop implicit factors. Surely the $\geq$ should be an approximate statement where there is a O(1) factor that cannot be determined. This makes the discussion around eq 5 questionable in my opinion.

We agree that these are approximate statements, and have now added the term "approximate" in relation to our inequality twice: in the sentence preceding Eq. (4) and in the sentence preceding Eq. (5). We have also substituted “=” by “≈” in several places in the paragraph before Eq. (4). Finally, we have removed a sentence related to the factor $\frac{1}{4\pi}\left(\frac{m_p}{m_e}\right)^{1/2} \approx 3$” after Eq. (5) since this factor is comparable to other approximations we have made.

3. In the discussion the authors make an interesting observation that the electric charge cancels out as does the interparticle separation and refer the reader to [24]. I went through [24] and did not appreciate the logic used to arrive at this result. For starters, the way that the cancellation of the electric charge happens appears to involve a ratio of two quantities (below eq 9 in [24]). Since either of these quantities could involve a dressing factor, there could easily be a residual factor arising due to some screening effect. Why is this necessarily O(1)?

The effect of dressing on the charge cancellation is interesting. As mentioned earlier, our calculation did not involve dressed mass values since we aimed at a more universal, albeit simplified, picture. The charge cancels in minimal viscosity $\nu_m \propto Ea^2$ because the Rydberg energy $E \propto e^4$ and Bohr radius $a \propto 1/e^2$. However, an easier way to see why charge does not enter the final equation for the minimal viscosity ν_m is to look at Eq. (13) in Ref. [24] which gives a characteristic energy of electromagnetic interactions in condensed matter phases: $E = \frac{\hbar^2}{2m_e a^2}$. This is a well-known energy of the electron localised at distance a, derived using the uncertainty principle. The same equation (same uncertainty principle) applies to any particle, hence no charge features in it. Now, the minimal viscosity in Eq. (11) is $\nu_m \propto Ea^2$, and using the above equation we see that neither charge nor inter-particle separation enter the minimal viscosity; only $\hbar$ and particle mass do.

We have added a new discussion of this point in the first paragraph of Discussion section.

4. Is there a direct holographic calculation of kinematic viscosity? To clarify, not using known results for eta,s as in eq 9 but via a direct calculation of response.

In a neutral relativistic fluid Eq. (9) is an exact statement. The kinematic viscosity is exactly the momentum diffusion constant and the latter is $D = \eta/\chi_{PP}$ where the momentum susceptibility is $\chi_{PP} = \epsilon + p = sT$. If the fluid

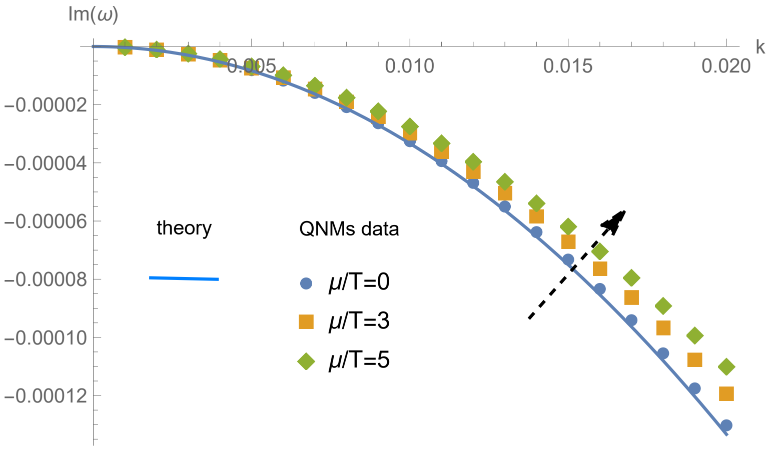

Figure 1: The shear diffusion mode at finite charge density obtained numerically from the direct computation of the QNMs in holography. The line is the theory prediction for one value of the charge. The decreasing of the diffusion constant by increasing the charge is evident.

is charged, the above formula gets modified into:

$$\nu = \frac{\eta}{sT + \mu\rho} \tag{1}$$

with μ, ρ the chemical potential and charge density.

Since both these quantities are positive, the kinematic viscosity in a charged fluid is always smaller than in a neutral fluid.

Taking a simple Reissner-Nordstrom black hole geometry, one could compute this quantity in holography and find perfect agreement with the formula above which comes just from hydrodynamics. The computations can be performed in two different ways:

- By using the Kubo formulas and extracting the shear viscosity from the stress tensor correlator and the quantities appearing in the denominator from the thermodynamic data.
- By direct computation of the shear diffusion mode with dispersion relation $\omega = -iDk^2$ (via a standard QNMs computation) and fitting to a parabolic dispersion relation.

In Fig.1 the results from the holographic computations are shown.

We have added a new discussion about the direct holographic computation in a new Appendix, together with figure 1 and a new footnote 1 to clarify this point in the main text. We have also added a paragraph in page 5 to explain better our approximation scheme and the errors affecting the final result in Eq.(12).

While the premise of this paper is interesting, compared to ref [24], the new result here is to extend the observations in [24] to QGP. From my understanding, the reason this is nontrivial is essentially the usage of relativistic fluid dynamics equations for instance eqs 7-10. This makes the current paper quite a bit different than [24]. As discussed in my report, some of the inequalities are stronger than can be justified, while some of the discussion can be improved significantly. I feel after these changes and clarifications are made, the paper may meet the standards of SciPost.

1. Inequalities should be properly justified and limitations clearly discussed. Various implicit O(1) factors should be pointed out.

We have added terms "approximate" to our inequalities as mentioned earlier and have emphasized the order-of-magnitude nature of our evaluations. We have also substituted "=" by "≈" in several places in the paragraph before Eq. (4).

2. The main point of this paper is to use the result of kinematical viscosity and argue why this suggests that QGP as observed in present experiments are close to crossover. I feel that this needs a separate section and more justification than what is currently provided.

Following the referee's suggestion, we have added a new section in the Discussion (section 3.2) where we discuss the dynamical crossover in more detail.

We would like to add two points here. First, our main result is the observation that the kinematic viscosity of liquids at the minimum is approximately the same as that of the QGP. This is striking, given very different nature of physical systems involved and in particular the 16 orders of magnitude difference between dynamic viscosity and density between the two systems. We hope that this observation, together with our discussion relating the minimal viscosity to the fundamental constants would be interesting to the community and may encourage/stimulate further calculations and thinking.

On the other hand, our discussion of the dynamical crossover in QGP, while interesting to QGP expert community, is an inference from our main

point. This discussion is based on an *analogy*. Analogies may be fruitful and thought-provoking (sometimes they may not be), yet we don't feel its sufficiently developed to make more definitive statements about the QGP phase diagram and crossover points. To do it properly would probably require large-scale QCD lattice simulations, combined with new high-energy experiments. Our hope is that our discussion gives a hint/hunch of what to look for in these simulations. After all, liquids have been studied for very long time, and yet we have discovered the dynamical crossover in supercritical liquids fairly recently. Hence we feel this is good time to draw the analogy and see what can be learned from it. We hope that our current wording reflects our intention: we have been careful not to sound definitive but rather tentative and open, for example we say
" *it will be interesting to explore to what extent the dynamical crossover at the Frenkel line applies to the QGP phase diagram.*"
"*The analogy with liquids, if appropriate to pursue further, would indicate that the currently measured QGP is interestingly close to the dynamical crossover between the liquid and gas-like states.*"
and conclude with
"*the insights regarding the dynamical crossover may be useful and can be further explored in lattice calculations.*"

3. Point number 3 in my report above should be addressed.

We have now addressed it in the first paragraph of the Discussion section as mentioned earlier. We discuss why the charge and interatomic separation do not enter the equation for minimal kinematic viscosity.

Referee 2

To arrive at the estimated QGP viscosity the authors make a number of approximations, most notably neglecting the chemical potential and any temperature dependence of η/s *, the shear viscosity to entropy ratio. They choose the latter to take the universal KSS value* $1/4\pi$*, while generically the quantity is expected to be sensitive to temperature, as the authors themselves point out. While this is a valid first step, it would benefit the manuscript if the authors would comment more extensively on the potential role of temperature dependent effects, and how these may alter their main result. Similarly, what is the expectation once a finite chemical potential is taken into account? As a follow up paper, it may be interesting to examine these points in detail,* *for a more realistic comparison to the QGP.*

We are grateful to the referee for their comments and time. Below is the list of our revisions in response to the comments and our reply.

Indeed, our discussion involves approximation schemes which enable us to make an order-of-magnitude estimations of the kinematic viscosity of QGP. We agree with the Referee and their proposal to improve our result in a follow-on study and make a more realistic comparison to the existing heavy-ions collisions data.

As discussed in our paper, neglecting the effects of the charge density in our approximated formulas leads to an over-estimation of the diffusion constant (kinematic viscosity) by approximately a 14 % (see also below). Given the various $\mathcal{O}(1)$ factors involved in other approximations we make, this does not affect our final result Eq.(12).

We also note temperature effects on the η/s ratio. Following the Bayesian estimation for Pb–Pb collision data in *Nature Phys. 15 (2019) 11, 1113-1117*, the uncertainties introduced are comparable to the various factors dropped in our approximate (order-of-magnitude) evaluations such as Eq. (12).

The temperature and charge density effects can be accounted for when a more precise estimation of the kinematic viscosity, including all the correct numerical factors, will be performed. We agree with the Referee that this analysis could be a good start for a follow up paper and are grateful for their suggestion to do so.

We have added a new discussion of the referee point between Eqs. (8) and (9). We also conclude the paper with saying
"*the insights regarding the dynamical crossover may be useful and can be further explored in lattice calculations.*"

As a second point, the authors discuss the role of the uncertainty relation and throughout the analysis rely on a particle-like description of the QGP. Thus, they use elements of kinetic theory and assume the existence of quasiparticles, which may not be present in the strongly interacting QGP. I would like the authors to comment on when such assumptions fail, and how this may affect their arguments and main results.

The Referee is correct. Historically, the universal bound on viscosity has been loosely motivated using the uncertainty principle which is valid only for quasiparticles, for which a mean free path can be consistently defined. Nevertheless, these bounds have been later proven using holographic methods which do not rely on a quasiparticle description. In some sense, the arguments involving the uncertainty principle must be taken as suggestive intuitive analogies. A nice discussion on these issues can be found in *Nature Physics volume 11, pages 54–61(2015).*

We have added a new paragraph discussing this on page 4 and after Eq. (5).

1. Discuss the consequences of dropping the assumption of negligible chemical potential and $\eta/s \sim 1/4\pi$.

As now discussed in the main text (paragraph after Eq. (8)), a crude approximation for QGP gives $sT \approx 1.8 \times 10^{35}$ GPa and $\epsilon + p \approx 2.1 \times 10^{35}$ GPa. This means that neglecting the effects of the charge density in our approximated formulas will lead to an over-estimation of the diffusion constant (kinematic viscosity) by approximately 14%. Given other $\mathcal{O}(1)$ factors involved in other approximations we make, this does not affect final result Eq.(12) which must be viewed as an order-of-magnitude estimate as discussed in our paper. We have added a new discussion of this point.

2. State more clearly when the authors are assuming kinetic theory, the existence of quasi-particles, whether these are valid assumptions and if not, how they would modify their arguments and result. These points can be addressed in a qualitative way, since a more quantitative analysis would be more appropriate for follow-up work.

Qualitatively, kinetic theory, and in particular the existence of well-defined quasiparticles, is assumed only when we mention the uncertainty principle arguments. The important equation used for our estimation, $\eta/s = 1/4\pi$, can be rigorously obtained, and it was indeed obtained by Kovtun, Son and Starinets using a robust holographic computation which does not rely on the existence of quasiparticles. In other words, kinetic theory is implied as an intuitive/analogous argument related to the bound but is not used in the actual calculation giving our main result Eq.(12).
We added a discussion of this point in the paper after Eq. (5).

We again agree with the Referee that a more quantitative analysis could be a good start for a follow up paper and are grateful for their suggestion to do so.