Reply to referee report 1

We thank the referee for the positive report, recommending publication, and for highlighting the novelty of our approach.

The referee's primary criticism is that the reach of our results has been overstated. This was certainly not our intention and we have endeavored to address this point with some changes to the text. Below we note specific changes we have made to address the referee's comments.

The referee writes:

What is going on in the paper? Up to Eq. (8) the development is completely general, and quite refreshing: at least this referee has not quite encountered it before. But then the authors have to invoke particular limits and rather severe model assumptions to progress towards Eq. (14). The most severe limitation is in the weak electron-phonon coupling limit, a requirement to proceed with the evaluation of the memory function. For instance, it is actually in the dark in e.g. cuprates how strong this coupling is, especially dealing with the highly excited states (high temperature) that play a key role. One infers immediately that one looses generality in this limit. Generically tau is limited by retardation, the fact that the typical atom mass is larger by 5-6 orders of magnitude compared to the electron mass so that it takes much more time for the lattice to start moving than for the electrons. Given the weak e-ph "bottleneck" this retardation scale has completely disappeared from the exposition.

Our response:

The referee is absolutely correct that this is a major simplification of the problem, although we had already highlighted this simplification multiple times in the introduction and in the text. To remove any possible ambiguity we have added a clear statement shortly after equation (8) ("We noted in the introduction... in section 4 below.") that it is certainly possible for there to be no hierarchy between electron and electron-phonon timescales and that in such cases one must necessarily solve the full coupled el-ph problem, and that there may be important physics in the coupled problem that is missed by treating the phonons perturbatively. We also broke the paragraph below (8) into two paragraphs to make it clearer.

It is possible that part of the problem here was our referring to a "small" electron-phonon coupling. Actually the coupling doesn't need to be small. The crucial point is that the el-ph timescale should be smaller than the purely electronic one. We have made changes to the two paragraphs below equation (8) to clarify this fact ("We noted in the introduction... in section 4 below." and "When $1/\tau$ is small... that matters.").

The referee writes:

This is followed by the most severe approximation, assuming Eq. 10 for the e-ph coupling asserting that there is only a "volume" longitudinal coupling. This is no more than a textbook toy model and quite unrealistic especially in this context. Surely, in any real solid the transversal phonons do talk with a similar strength with the transversal shearing response of the electron system. That only the longitudinal

charge susceptibility enters is an artefact of the toy model! But this is in turn the tip of the iceberg. In real solids, especially the chemically complicated ones as of relevance to bad metals etcetera there are zillions of phonons, all having their highly structured momentum dependent couplings to the electrons; for the cuprates see e.g. Johnson et al, PRB 82, 064513 (2010). In fact, it is a recent affair that in conventional metals like the high Tc hydrides it can be claimed that the phonon driven Tc can be computed using DFT band structure but this already takes a huge supercomputer effort to keep track of this plethora of numbers.

Our response:

Again, we agree with the referee's comment. We have added some sentences below equation (12) stating explicitly, that (10) is an illustrative example, that real phonons are complicated, but that the method will certainly apply more generally.

The referee writes:

My recommendation is simple: the pitch of the manuscript should be toned-down a bit, emphasizing the far reaching model assumptions as I spelled out in the above that have to be made to proceed from Eq. (8) onwards to then proceed enthusiastically that even within these confines an intriguing and novel view on the "phonon bottleneck" arises.

Our response:

Hopefully the changes mentioned above address this point. We have absolutely no disagreement with the referee.

The referee writes:

Finally, my eyes discern Fig. 3 and Fig. 8 both addressing the temperature dependence of tau as main results. But in both cases I continue to wonder whether more can be said than is found in the text. The conventional metal result in Fig. 3 does show a pronounced peak in the rate that I have not seen before. Has this peak ever been observed in any experiment? I suspect that there is a logic behind this peak of a general qualitative nature that goes beyond what is found in the text — it may be worthwhile to give this some extra thought.

Our response:

Indeed, this peak has a simple explanation that we have now added to the caption of figure 3. We thank the referee for bringing up this point. We are not aware of experimental studies of the temperature dependence of tau and agree with the referee that this should be studied. The result shown in Fig 3 has been given previously in earlier work (e.g. the paper by Allen, Ref. [3]) but not plotted or commented on to our knowledge.

The referee writes:

Similarly, Fig. 8 showing that 1/tau is smaller in the bad metal than in the Fermi liquid took me by surprise. Beforehand I had expected it to be the other way around. What I get from the text is "just a conspiracy of numbers". But yet again, I am

wondering whether there may be a more general reasoning underlying this surprise. Again, a stimulus to the authors to give this extra thought.

Our response:

It is difficult to draw general conclusions on this fact. We suspect it is just down to numbers, because the spectral weight gets integrated in equation (14), so that even if interactions move the spectral weight around, that by itself doesn't determine whether the integral will go up or down. We added a short comment on this fact in the discussion of Fig 8 (paragraph before sec. 3.5, "In general interactions redistribute... decrease.").