Reply to Report 1 by Referee 1:

1. **Q:** *The authors present the phase diagram for their holographic model at Fig 1. The presentation is catchy but has the an inconvenient. It is hard to imagine what does N^2 mean at that stage of the paper. It is only defined several pages latter in Eq (13). The authors should refer to this equation and give at least an informal definition of N^2, explaining at least what does the sign of N^2 stands for. In this way potential readers can profit from the presentation.*
   A: We agree that that the definition of N^2 should be, at least referenced, earlier. We amendend the caption of Fig. 1, as well as the text in the second paragraph on p. 3 in this respect.

2. **Q:** *In Section 2.3.1 the authors mention the c-theorem. I guess they read c from the effective AdS radius in the IR. Is that correct? They also mention that it satisfies a c-theorem. Can the authors recall the ingredients for a holographic c-theorem? Do all proofs rely on NEC? Maybe a reference (Myers-Sinha?) and a clarification on why the c-theorem still works will be helpful.*
   A: We agree that the way we originally mentioned the c-theorem in the main text is ambiguous. Nonetheless, the referee is right: We read off the fixed point values of the central charge from the radius of the AdS fixed point geometry. The holographic c-theorem relies on unbroken Lorentz invariance along the RG flow, as well as on NEC. Note, however, that violating NEC does not logically imply that the central charges at the UV/IR fixed points cannot incidentally fulfill $c_{UV} > c_{IR}$. The situation at zero temperature is as follows: The RG flows depicted in Sec. 2.3.1 always interpolate between two UV/IR fixed points, which in all cases are AdS. In the PT symmetric phase, the geometry along the RG flow is real, NEC is fulfilled, and hence the c-theorem holds. At the exceptional point, as observed already in [1], the scalar decouples and the geometry is AdS_4. Thus, there is no notion of non-trivial RG flow in this case. On the other hand, in the PT broken phase, both the interpolating geometry as well as the bulk energy-momentum tensor become complex. Due to the complexity of the bulk energy-momentum tensor, we cannot even make sense of the NEC eq. (31). However, even in this case, at the UV/IR AdS_4 fixed points, the geometry is real and the energy-momentum tensor vanishes, and the central charges that are read off from the effective AdS_4 radii fulfill $c_{UV} > c_{IR}$. We changed the presentation at the end of Sec. 2.3.1 accordingly.

3. **Q:** *In the description of phase I in page 10. Is there an interpretation for the sign of $\langle O \rangle$?*
   A: The sign of $\langle O \rangle$ can be easily interpreted in linear perturbation. The partition function with a small source N in euclidean signature is $\left\langle e^{\int d^d x N O} \right\rangle \approx e^{-\frac{1}{2}\int d^d x N G_{OO} N}$ with positive Euclidean correlator $G_{OO} > 0$ [E. Witten, Adv.

Theor. Math. Phys. 2 (1998) 253-291]. Therefore, if we turn on a positive source (namely $N > 0$) in phase I, we have a negative expectation value $\langle O \rangle = -G_{OO} N < 0$.

4. **Q:** *In the description of phase II in page 11 the authors claim "these two branches are both unstable in the sector of scalar (A_x, φ) perturbations". I think there is a typo there. At k=0 it should be A_t instead of A_x. This is consistent with the computations of appendix B. An instability on the A_x sector would imply a non-analyticity in the upper complex plane of $G_{J_x J_x}$, which would imply a violation of the sum rule for the conductivity.*
   A: Yes. It is a typo, and we thank the referee for spotting it. We have fixed it. The instability is in the $\{A_t, \varphi\}$ sector for $k = 0$. As shown in App. B, at $k \neq 0$, all modes will couple to each other, and the instability will move to the full $\{A_t, A_x, \varphi\}$ sector.

5. **Q:** *Is there a hint of an end point for this unstable backgrounds? Or a reason to imagine that there will be no static end point to this instability?*
   A: In [1,38], no such end point to the instability was found. However, one could guess that due to the instability in the $\{A_t, \varphi\}$ sector for $k = 0$, a static solution with unbroken translation and rotation invariance should develop a finite charge density. However, notice that while the background satisfies $\phi = \bar{\phi}$, the unstable mode does not satisfy $\delta\phi = \delta\bar{\phi}$, but $\delta\phi = -\delta\bar{\phi}$ in (96). This hints that the end point of the unstable mode could have $\phi \neq \bar{\phi}$. In contrast, from (25), we find that a static solution can always be transformed into the gauge $\phi = \bar{\phi}$ in (27). So we expect that there is no static end point to this instability. This situation is not unusual for non-Hermitian dynamics. Actually, in further numerical analysis we found the same situation in the complexified rotor model: starting from an unstable saddle point and going along an unstable direction, ϕ and $\bar{\phi}$ could travel to another saddle, undergo periodic motion, or flow to infinity. We amended the discussion on Page 20 accordingly.

6. **Q:** *For solutions with complex temperatures, is there a physical reason for presenting the solutions in terms of $|T|$, or it is just for presentation? Also is some of the phase diagrams the axe is labeled by T, while I imagine it should be $|T|$ to make sense with phase III solutions. See for instance Fig 1, Fig 14.*
   A: We agree that in all figures that show phase III, the temperature axis should be labelled $|T|$. We changed Fig. 1 accordingly. The reason why we plot everything in terms of $|T|$ is as follows: As shown in Fig. 3d, the two complex conjugate branches in phase III have complex conjugate temperatures as well, such that their $|T|$ is the same, and hence, plotting everything in terms of $|T|$ is sufficient. Basically, since the complex temperature in phase III depends only on a single real parameter N^2, ReT and ImT are not independent, and hence it suffices to plot everything as a function of $|T|$. Moreover, the solution in phase III converges to the the zero temperature solutions in the PT broken phase of [1] by taking the limit $|T| \to 0$ solely, which implies that $|T|$ is the correct combination to plot against.

7. **Q:** *In section 2.4. What does it mean the superconducting phase transition in backgrounds where the $U(1)$ symmetry is explicitly broken? Is it just a second branch of solutions that continuously connects to the HHH when both M parameters are turned off? Is it always the stable branch? what does "cross-over" stands for? Is the free energy and all its derivatives continuous?*
A: As we mention at the end of sec. 2.4 in the draft, at vanishing source deformation, either Hermitian or not, at low enough temperatures, the HHH model undergoes a second order phase transition to a condensed phase in which the $U(1)$ global symmetry on the boundary is spontaneously broken by the generation of a finite $\langle O \rangle$. Second order here means that the free energy and the entropy is continuously differentiable, but the specific heat (the second derivative of the free energy) is not. In [71], the situation of a small Hermitian source deformation of the HHH model was analyzed, and it was shown that the second order phase transition becomes a smooth crossover in the sence that all thermodynamic quantities smoothly change during the crossover. This is the standard situation if spontaneous and explicit symmetry breaking are both present, such as e.g. for the broken flavour symmetry in QCD [Phys. Rev. **175**, 2195-2199 (1968)]. Due to the global complexified U(1) symmetry present in phase I ($N^2 > 0$), we can always use the Dyson map to gauge away the phase of N. Hence, turning on the non-Hermitian source deformation in phase I will not change the property that the second order transition becomes a crossover.

8. **Q:** *About section 3. How does your results (if they do at all) connect with 2104.02428? In particular, the authors of 2104.02428 claim that a) The sum rule is satisfied even in tachionic backgrounds b) They blame for that the local conservation of the current, much alike your Ward identity (26) when evaluated in a static background. c) They give a connection between σ_Q, the imaginary mass and the effective gap.*
A: We thank the referee for pointing out that paper, which was cited as Ref. [40] in the revised version. We would like to point out the following fundamental differences between [40] and our manuscript: 1) The time evolution operator defined in [40] is not the same than the one defined in our work. In [40], it is defined as $e^{iH^\dagger t} O e^{-iHt}$, whereas in our manuscript as $e^{iHt} O e^{-iHt}$. 2) In [40], the analysis was done for 1D systems, whereas our work deals with 2D systems. 3) In [40], the DC conductivity σ_{dc} is always finite. However, because of the condensation, the DC conductivity in our manuscript is divergent. In order to make it finite, we would have to introduce momentum relaxation. 4) As the referee points out, the conservation equation (Ward identity) holds in both papers. However, the fundamental reasons are different. In [40], the Hamiltonian enjoys the $U(1)$ symmetry. In our paper, the non-Hermitian deformation explicitly breaks the $U(1)$ symmetry and the conservation equation holds accidentally on the static solution, c.f. Eq. (26) and the discussion around there. However, the charge is not necessarily conserved dynamically when we perturb the system away from the static solution. 5) The system in [40] has a hard gap, while our system, as typical for

holographic superconductor models, shows a soft gap [73]. Point (4) constitutes the main difference between our result and [40], and we added a comment on this point in the outlook.

9. **Q:** *What is the explicit expression for M in (71)?*
   A: We expressed $\mathcal{M}^2$ as a function of the invariant φ_s^2 in (71), while the expression of φ_s^2 determined by the saddle point equation (65) is too tedious to show and we do not consider that it provides any impactful insight. If necessary, it can be easily obtained by e.g. Mathematica.

10. **Q:** *The free model of Section 4 could easily incorporate a chemical potential. Is there a reason why the authors did not consider that? Will its effect be a simple shift of the mass parameter r?*
    A: We incorporated a chemical potential in Sec. 4, c.f. the discussion around Eq. (65). As the referee points out correctly, introducing a real chemical potential μ basically shifts $r \to r - q^2\mu^2$ in the analysis of saddle points and stability.

11. **Q:** *Finally, I understand that studying interacting systems away from charge neutrality is one of the biggest strengths of AdS/CMT, as other methods usually fail. But I feel that the motivation should be more explicit. What is the state of art in the context of non-Hermitian physics? Or was the general idea of turning on μ to give the sum rule another chance to fail?*
    A: As noted by the referee, the AdS/CFT correspondence allows to study non-Hermitian matter without relying on the quasi-particle picture and away from charge neutrality. This is contrary to the majority of the recent studies on non-Hermitian systems, which rely on the quasi-particle picture [40, 76-82]. For instance in [40], the authors focused on the charge transport at half filling only ($\mu = 0$), and the transport at finite chemical potential remains to be investigated in this model as well. Furthermore, the holographic duality does not suffer from the sign problem, even at finite density.
    Besides the motivation from transport, we also studied the interplay between the PT-symmetric deformation and temperature. In particular, the PT-symmetry breaking at finite temperature is also novel compared to [40] and the holographic works [1,38], where the PT-symmetry breaking was studied at zero temperature only.
    Finally, as the referee points out, switching on finite chemical potential could also lead to an instability in the vector sector that would make the sum rule fail. We however did not observe this in the range of chemical potential and temperatures considered.
    We amended the introduction after Eq. (1).