We thank the Referee for their second report that helped us to further improve our manuscript. Below, please find our response (in black) to all comments, questions, or suggestions raised by the Referee (in blue).

The authors have responded to my points in a satisfactory manner and they have made corresponding changes, that -in my eyes- greatly improve the quality of the manuscript as well as its accessibility.

The physics is interesting and with experimental realizations of such systems within reach this work is certainly timely. The present manuscript sets a foundation for the exploration of two-dimensional strongly-coupled Bose-Fermi mixtures, that future works will be able to draw from. As a result, I can recommend publication of the manuscript in its present from.

Below, I provide additional feedback that I encourage the authors to consider/incorporate if it is feasible and applicable. These points, I think, could enhance the insights obtained from this manuscript, but, if the authors deem these points not applicable or feasible, then my points should not delay publication. The manuscript in present form is already suitable for publication.

Thank you!

My feedback:

I still believe that one of the key results of this work, the condensate fraction differing from the quasiparticle weight, could be illuminated more.

The correspondence in 3D is fairly strong, which suggest that it isn't a coincidence and also begs the question what changes in 2D.

We agree with the Referee that the correspondence in 3D is fairly strong. Still, it is not exact. Focusing on unitarity, Table I reports a comparison between n_0/n_B [data from Guidini *et al.*, PRA **91**, 023603 (2015)] and the polaron residue Z_pol, as calculated either with the non-self-consistent TMA or with the Diagrammatic Monte-Carlo method [data from Kross and Pollet, PRB **91**, 144507 (2015) for both TMA and diagMC]. Data for the condensate fraction are at the lowest concentration ($x = 0.175$) considered in (Guidini, 2015) which, given the nearly exact universality of n_0/n_B at unitarity, can be considered equivalent to $x \to 0$ [see Fig. 3(b) of (Guidini, 2015)].

m_B/m_F	n_0/n_B	Z_{pol}^{TMA}	Z_{pol}^{diagMC}
23/40	0.74	0.80	0.76
1	0.73	0.78	0.76
5	0.60	0.67	0.65

Table I. Condensate fraction and polaron residue in 3D at unitarity for three different mass ratios. The condensate fraction is calculated at $n_B/n_F = 0.175$.

One sees a difference between n_0/n_B and Z_{pol}^{TMA} that amounts to about 6% for equal masses, and that increases by changing the mass ratio, exceeding 10% for $m_B/m_F = 5$. (The difference between n_0/n_B and Z_{pol}^{diagMC} is slightly smaller, but it is the comparison with Z_{pol}^{TMA} that is more meaningful, since Z_{pol}^{TMA} and n_0/n_B are calculated within the same approximation.) This indicates that the supposed equivalence between Z_{pol} and n_0/n_B is only approximate in 3D. It is therefore fully acceptable that such an approximate degeneracy between these two quantities is lifted in 2D.

We have added these considerations in version 3 of our manuscript. See new paragraph "In this respect, reconsidering the 3D case ..." (4th paragraph of Sec. 4.4).

I do not find the argument given by the authors on how „there is no reason why the limit for x->0 of the condensate fraction and the polaron residue Z should coincide“ particularly convincing. Both quantities are well defined in the thermodynamic limit. The field theory does not know about a particle number, it only knows about particle densities. The condensate fraction is obtained at nB>0, while the polaron quasiparticle weight is obtained from the weight of the quasiparticle pole in the bosonic Green's/spectral function at nB=0 (which is also well defined at nB>0). Importantly the quasiparticle weight obtained from the spectral function has the same physical interpretation as the quasiparticle weight in the polaron wave function (Chevy) Ansatz.

In both cases one has taken V->infinity before specifying the chemical potentials (and condensate densities) which eventually yield the corresponding densities nB and nF.

We fully agree with the Referee that both Z_{pol} at $x = 0$ and n_0/n_B at $x > 0$ are calculated and well defined in the thermodynamic limit $V \to \infty$. However, this does not imply that Z_{pol} should coincide with $\lim_{x\to 0} n_0/n_\mathrm{B}$. In particular, for a given BF interaction

$$Z_\mathrm{pol} = \lim_{V\to\infty} \frac{n_\mathrm{B}(k=0; N_\mathrm{B}=1; N_\mathrm{F}/V = \mathrm{const})}{1},$$

which is a different limit from

$$\lim_{x\to 0} \frac{n_0}{n_\mathrm{B}} = \lim_{x\to 0} \lim_{V\to\infty} \frac{n_\mathrm{B}(k=0; N_\mathrm{B}=xN_\mathrm{F}; N_\mathrm{F}/V = \mathrm{const})}{N_\mathrm{B}}.$$

This is what we stated in the previous version of our manuscript. To further clarify it, we have now added Eq. (56) in Sec. 4.4, which summarizes in mathematical symbols why the two quantities are obtained by different limits.

Furthermore, it was my understanding that the universality came from, as the authors note also in this work, the bosons being nearly independent of each other, while interacting with the medium. Thus, it would at least be physically intuitive that the probabilities for a boson to be in a p=0 mode are related in these cases. My understanding was that the correspondence between the quasiparticle weight and the condensate fraction was a reflection of that. So if a universality is observed here, then I would still expect some sort of correspondence between probabilities to be in effect. Thus it would be insightful to illuminate where the „remaining probability“ goes.

-The curves shown in Figure 14 are for eta=0, however for eta=0 we have that nB(0) is finite. For eta≠0, nB(0) diverges. Could it be that there is some sort of delta function for p=0 that contributes to the fraction of p=0 bosons? Perhaps because in 2D one only has a single factor of p in the measure instead of p^2 in 3D? Do plots like Figure 14a also exist for eta>0? Could it be that there is stronger correspondence for eta≠ 0?

Actually, one expects that a finite repulsion $\eta > 0$ will further deplete the condensate fraction and make the difference between Z and n_0/n_B even larger.

To show it explicitly, we have added panel (c) to previous Fig. 14 (now Fig. 15), reporting the condensate fraction vs BF coupling at $\eta = 0.1$ for three different concentrations.

For completeness and reproducibility of our data, we have then also added a new figure (present

Fig. 14) reporting the corresponding bosonic and fermionic chemical potentials vs BF coupling at $\eta = 0.1$.

-How does the bosonic quasiparticle weight at x>0 (not just at x=0) compare to the condensate fraction?

Actually, the results of Sec. 4.2 (see in particular the discussion of Fig. 9), show already that, around $g = -0.7$, the bosonic quasiparticle weight $Z(x)$ at $x > 0$ is $\gg 1$. It is thus completely unrelated to the condensate fraction, as well as to the polaron residue Z. In order to clarify the latter point, we now discuss in detail how this difference between $\lim_{x\to 0} Z(x)$ and Z originates from the behavior of the boson self-energy in this limit.

See in version 3 of our manuscript the two new paragraphs following Eq. (56), with three new equations (57-59), a new reference (Ref. 125), and a new figure (Fig. 16).

-I may be mistaken about this: Have the authors considered reconstructing the quantum effective action from the renormalizations employed here? In particular the effective potential? After a short, (not very careful) analysis I obtain that $\frac{\delta\Gamma[\phi[J]]}{\delta\phi[J]}|_{J=0} = \phi[0]G_B^{-1}[\phi[0]]$. Where J is the source field, $\phi[J]$ is the source-dependent boson field and $\phi[0]$ is the stationary field at vanishing source, which here is proportional to $\phi[0] \propto \sqrt{\rho}\delta(\ldots)$. It would seem that the Hugenholtz-Pines condition employed here is a necessary condition for the field to be stationary. However, I don't think it necessarily implies that $\phi[0]$ minimizes the effective potential? This could either mean that the value obtained for rho is not unique and there is a second value of rho that fulfills the Hugenholtz-Pines condition, along with the other fixing conditions AND additionally yields a lower value of the effective potential. Alternatively, it could also mean that the field additionally condenses in a different mode, for example p>0 (though I don't think this is the case here). Have the authors considered checking if there is a larger value of ρ that fulfills the Hugenholtz-Pines condition? I would guess that actually computing the effective potential and comparing values is quite cumbersome, but checking whether there is a second solution to the Hugenholtz-Pines condition should be feasible.

Given the non-linearity of the coupled equations for $n_0, \mu_{\rm B}, \mu_{\rm F}$, the possible existence of multiple solutions cannot be excluded rigorously. However, by varying the initial guesses in our root-finding algorithm, we found no evidence of a second solution for n_0 (and μ_B, μ_F) for the concentrations and couplings considered in the present work.

-I believe a formal consideration of the effective potential/ effective action might also yield more insights into the possible correspondence between quasiparticle weight and condensate fraction

Concerning the reconstruction of an effective action and an effective potential from the diagrammatic theory of the present work, we are not convinced that it would really shed light on the problem of the correspondence between the condensate fraction and the polaron residue. Furthermore, it is non-trivial work. It would require reformulating the problem in a functional-integral framework and reverse-engineering an appropriate Hubbard-Stratonovich transformation plus saddle-point approximation with inclusion of fluctuations leading to the same set of diagrams considered here. This could be an interesting separate work, but we deem it beyond the scope of the present work.

-I may be mistaken, but is there a possibility that the used Hugenholtz-Pines condition is only a low-order approximation of a „more“ accurate condition? Eq. 4.10 in Ref. 107 and the text below seem to indicate that Eq. 6.2 of Ref. 107 is only an approximation. Though it seems to be increasingly valid at low density, which however begs the question of whether this would refer to boson or fermion density in this case? I am not sure how this point fits with my previous points, but I thought it might be better to include it regardless.

The Hugenholtz-Pines condition guarantees that the spectrum of bosonic excitations in the condensed phase is gapless (as is also required by the Goldstone theorem). It is held to be exact and has been proven in different ways in the literature. The proof reported in Sec. 9.4 of the book by G. Rickayzen (Green's functions and condensed matter, Academic Press, 1980) is particularly clear and convincing. It is based on a similar proof by Hoehenberg and Martin [Ann. Phys. **34**, 291 (1965)] (see their Sec. VI.D).

We have added these two new references (Refs. 108, 109) in version 3 of our manuscript when introducing the Hugenholtz-Pines condition at the end of Sec. 2.4.