Revision: SciPost Physics/2504.03411v1

Title: "Competition of light- and phonon-dressing in microwave-dressed Bose polarons" by Georgios M. Koutentakis, Simeon I. Mistakidis, Fabian Grusdt, Hossein R. Sadeghpour, Peter Schmelcher

Dear Editor-in-charge of SciPost Physics,

We would like to thank you very much for your handling of the above manuscript submitted to SciPost Physics. According to the referee reports we have performed all the suggested changes in the revised version of our manuscript. In the following you can find our detailed reply to all of the comments of the referees. A list of changes is also appended following our point-by-point reply to the referee's comments.

On behalf of the authors,

Georgios M. Koutentakis

Report #1 by Arturo Camacho Guardian (Referee 1) on 2025-5-18 (Invited Report)

The referee writes:

The manuscript "Competition of light- and phonon-dressing in microwave-dressed Bose polarons" by G. Koutentakis and co-authors studies the quasiparticle properties of a spinor impurity coupled to a scalar BEC confined in a one-dimensional harmonic trap. The internal states of the impurity are coupled by a light-field, one of the states is interacting whereas the second one is non-interacting. The authors adapt their numerical approach ML-MCTDHX (an ab-initio numerical approach) to study this system, this technique has been developed quite extensively in the group of some of the authors.

The topic of the manuscript is timely, the methodology robust, the results interesting and overall the manuscript is well-written. Before giving my final recommendations please find below my requested changes and questions.

Our response:

We thank the referee for their appraisal of our work and suggestions which helped us to provide further clarifications and improve our presentation. In the following, we provide a detailed response to all comments and append a list of changes.

The referee writes:

Requested changes

1. Some figures are quite crowded and difficult to interpret. For instance, in Fig. 1(a) and (b), it is hard to distinguish the differences between the plots, understand the role of N=2, and determine whether impurity-impurity interactions are present. The arrows in Fig. 1(b) are not particularly useful and add to the confusion.

Our response:

In the revised version of the manuscript we have splitted previous Figure 1 into two separate ones (now Figures 1, 2). Additionally, we provide a magnified view of the resonant region around $\Delta = -E_{1\uparrow}$ for $N = 2$ as panel (c) in Figure 1 such that the interaction shift (which is negligible in the discussed scales) becomes more visible.

The referee writes:

2. Perhaps I missed something, but one of the main conclusions seems to be that the

residue increases with light-dressing. However, I couldn't find a figure that clearly demonstrates this. In fact, most of the results show a residue close to 1, so I struggle to understand how the impurity is dressed at all. (see next point)

Our response:

We thank the referee for this comment. Notice that one of our main findings is that the residue is captured by the experimentally accessible quantity $|\langle \hat{\boldsymbol{S}} \rangle|$. The behaviour of the latter within the different approaches we employ reveals information about the dressing of the polaron and the overlap of different involved states. This quantity when expressed in terms of the residue $Z(\Omega_{\mathrm{R0}})$ and Z_{eff} always results in a value of $0 < Z < 1$, indicating a dressed well-defined polaron. The limiting case $Z = 0$ indicates the absence of a quasiparticle since the many-body state is orthogonal to the free impurity state and $Z = 1$ marks the absence of a quasiparticle state because in this case the impurity state is independent of the bath. Neither of these scenaria are realized in our results, except for $\Delta \gg -E_{1\uparrow}$, where the non-interacting spin-$\downarrow$ state dominates, and thus $Z = 1$. To explicitly demonstrate the above and provide a direct connection to the corresponding many-body overlaps, in the revised manuscript we utilize the residua of the spin-projected many-body wavefunction. We have made several changes in the text to clarify these points and added a new Appendix C containing Fig. 9, where the residua of the spin-projected many-body wavefunctions are discussed (see also the list of changes).

The referee writes:

3. I also found the notation somewhat confusing, particularly in Section 3. My understanding is that the system can be described by a simple two-level model consisting of the non-interacting impurity state and the polaron state (in the limit of vanishing light-matter coupling). These two states then couple to the light field and hybridize. If this is correct, why is the residue not simply given by Eq. (7)?

Our response:

We thank the referee for this interesting question. While the two-level model is adequate for understanding the basic behavior of the system in terms of energies and populations of the spin-states, it is not able to correctly predict the behavior of more complex observables pertaining to the many-body state of the system. This can be readily seen by the fact that within the two-level system we expect $Z_{\uparrow} = Z$ (interacting spin state) and $Z_{\downarrow} = 1$ (non-interacting spin state) since it involves only the states $|\Psi_{0,+1/2}\rangle = |\Psi_{B+1\uparrow}\rangle$ and $|\Psi_{0,-1/2}\rangle = \hat{a}^{\dagger}_{0\downarrow}|\Psi_{B+0\uparrow}\rangle$. However, this is not the case within the many-body treatment as demonstrated by the newly added Figure 9 in Appendix

C. Therefore, since the bath-impurity states within the spin-$\uparrow$ and spin-$\downarrow$ states change with respect to different system parameters (such as the detuning), the residue cannot be captured by the population of the spin-states given by Eq. (7). Comments along these lines have been added in the main text along with a new Appendix C in the revised manuscript (see also the list of changes).

The referee writes:

4. My main concern relates to the novelty and significance of the results, which connects to the previous comment. It appears that the light-matter coupling can be described by a simple two-level system, and that, in the explored regime, no particularly intriguing phenomena emerge. From what I understand, the main observable effect comes from Fig. 1(d), where there is a "drop" in S from 0.5 to 0.492—this seems rather small. Could the authors comment on the physical relevance of such a small difference? Is it experimentally observable? Also, did the use of the ML-MCTDHX method require any non-trivial extensions for this study?

Our response:

In view of the answers provided to questions 2 and 3 raised by the referee, we hope that they will be convinced about the interesting physics arising beyond the regime of validity of the two-level approximation, which we elucidate within the effective Hamiltonian model. Another focal point of our analysis is, of course, the competition of light and phonon polaron dressing as captured by the effective Hamiltonian model. On the other hand, achieving sensitivity of the order of $|\delta S| \lessapprox 10^{-3}$ or smaller is experimentally viable todate within Ramsey spectroscopy, see for instance the recent work by J. Etrych, *et al*, Phys. Rev. X **15**, 021070 (2025). Certainly, we anticipate increasing impact on the dressing toward the stronger attractive interaction regime which we aim to undertake in a future investigation.

Turning to the ML-MCTDHX approach, the one used herein corresponds to an extended implementation of an effectively scalar bosonic system coupled to an interacting spinor impurity. This required a few new technical implementations and optimizations regarding observables, especially regarding the fidelity calculations. The specifics of the structure of the wavefunction of the used setting are discussed in some detail within Appendix A and appropriate references are also provided.

The referee writes:

5. On page 13, the authors refer to the $1/k^4$ scaling related to Tan's contact to support their claims. I don't fully understand this argument. As far as I know (though I may

be mistaken), this scaling is a high-energy feature usually captured only with non-perturbative methods. While I agree that ML-MCTDHX is an ab initio approach, it is unclear to me how it could include the relevant two-body physics (e.g., Feshbach resonance physics) necessary to reproduce the $1/k^4$ scaling.

Our response:

We thank the referee for their comment. Here, we need to highlight that Tan's contact is only dependent on the scattering length and the two-body correlations emerging due to it. Regarding polaron physics, it already appears within perturbation theory see Table 2 in [F. Scazza, *et al*, Atoms **10**, 55 (2022)]. ML-MCTDHX has indeed access to this quantity since it takes all two-body correlations into account in a non-perturbative manner.

To avoid any possible confusion we amended the corresponding passage in the text. We now highlight the presence of additional short range contributions due to the bath-impurity interaction that are absent within the effective Hamiltonian treatment (see also the list of changes).

The referee writes:

Minor comments:

A. On page 2, second paragraph: it reads quasiparticletheories — a space is missing.

Our response:

We thank the referee for spotting this typographical error. It has been eliminated in the revised version of our manuscript.

The referee writes:

B. On page 2, the authors cite Refs. [69–82] as related to their previous work. Since this manuscript builds on those studies, could the authors clarify the relevance of each reference for the present work? They begin to do this on page 3, but the discussion could be made more explicit.

Our response:

We agree with the comment of the referee. To circumvent this issue, in the revised manuscript we have added a relevant discussion to highlight differences between the effective potential approaches used in the aforementioned referenced works.

The referee's recommendation:

Publish (easily meets expectations and criteria for this Journal; among top 50%) Our

response:

We once more thank the referee for their suggestions that helped us to improve our presentation, and also for recommending our work for publication.

Report #2 by Anonymous (Referee 2) on 2025-5-28 (Invited Report)

The referee writes:

This manuscript considers the properties of one or a few bosonic impurities in a 1D weakly interacting Bose gas. The main novelty is that the impurity has two internal spin states, which are Rabi coupled: one internal state is interacting with the surrounding bosons and one is not. By carefully comparing with ab initio numerical calculations, the authors show that two simple analytical approaches based on a two-level approximation and an effective Hamiltonian both provide an accurate description of the system.

The topic of the manuscript is timely and interesting since there is presently a significant research activity exploring the physics of mobile impurities in a quantum degenerate environment. Before the paper is published, I however request the authors to consider the points below.

Our response:

We thank the referee for their appraisal of our work but also their interesting comments and suggestions. Below, we provide a point-by-point reply to all issues raised as well as a list of changes at the end of the reply letter.

The referee writes:

1. Is it really correct to speak about polarons in 1D? The existence of quasiparticles in 1D is not obvious. For instance, in arXiv:2504.17558 it was shown that a mobile impurity in a Fermi gas has zero quasiparticle residue. I presume the same holds for an impurity in a Bose gas? I suggest the authors discuss this in the manuscript.

Our response:

This is a very interesting question. Indeed, in one-dimension the answer depends on the statistics of the host medium. Namely, it turns out that Fermi polarons do not exist in the thermodynamic limit while Bose polarons survive in the case of confinement as in our case. Specifically, in the case of an *interacting Bose gas*, the impurity dresses only $N_{\text{dress}} \sim n_0\xi$ bosons, where $\xi = \hbar/\sqrt{m_B g_{BB} n_0}$ is the healing length. Hence, the many-body overlap remains finite even in the thermodynamic limit. This can also be proven analytically within the Lee–Low–Pines + Gross-Pitaevskii equation (LLP + GPE) approach as it was demonstrated in [A. G. Volosniev and H. W. Hammer, Phys.

Rev. A **96**, 031601(R) (2017)], which can be rewritten as

$$Z = \exp\left\{ \frac{\hbar c}{g_{BB}} \left[2\tanh^{-1}\left(\sqrt{1+\left(\frac{g_{BI}}{2\hbar c}\right)^2} - \frac{g_{BI}}{2\hbar c} \right) \right.\right. \\ + \ln\left(\frac{1}{2}\frac{g_{BI}}{2\hbar c}\left(\sqrt{1+\left(\frac{g_{BI}}{2\hbar c}\right)^2} - \frac{g_{BI}}{2\hbar c} \right)\right) \\ \left.\left. + 1 - \sqrt{1+\left(\frac{g_{BI}}{2\hbar c}\right)^2} + \frac{g_{BI}}{2\hbar c} \right] \right\}, \tag{1}$$

where $c = \hbar/(\sqrt{2}m_B\xi)$ is the speed of sound. Therefore, Z stays finite for any g_{BI} provided $g_{BB} > 0$. In the weak-coupling limit $1 - Z \propto g_{BI}^2$; in the opposite limit $g_{BI} \to \infty$ the residue saturates at $Z \to \exp\big[-(2\ln 2 - 1)\hbar c/g_{BB}\big] > 0$, so the quasiparticle remains well-defined for all interaction strengths, within this approach.

Therefore, in our case where a harmonically trapped gas is used and finite-size effects cut off the infrared divergence reported in [F. Grusdt, *et al* New J. Phys. **19**, 103035 (2017)] the notion of the polaron is well-defined. A corresponding paragraph has been added to the manuscript (Sec. III B) summarizing the above arguments.

The referee writes:

2. In Fig. 1, the case of zero Rabi coupling is labeled as a solid black line. However, I think the line is dashed.

Our response:

We thank the referee for bringing this typographic error to our attention. It has been resolved in the revised version of the manuscript (see list of changes).

The referee writes:

3. I don't understand the formula for the residue Z given in the fourth line of page 8. Doesn't this formula give zero?

Our response:

We thank the referee for bringing up this subtle point. In the revised version of the manuscript, the typographic error in the residue formula has been corrected and, for $\Omega_{\mathrm{R}0} = 0$, it is given by $Z = |\langle \Psi_{B+0\uparrow}|\hat{a}_{0\uparrow}|\Psi_{B+1\uparrow}\rangle|$. Here, $|\Psi_{B+0\uparrow}\rangle$ is the bath state in the absence of an impurity and $|\Psi_{B+1\uparrow}\rangle$ is the combined ground state of the bath and an interacting impurity.

The referee writes:

4. The authors in general find very small deviations between the exact numerical calculations and the approximate theories. It would make the paper more interesting and suitable for SciPost, if the authors explore if this holds also for stronger impurity-boson interactions - especially on the attractive side where there is no phase separation. In particular, the authors state on page 14 that the effective Hamiltonian is accurate for $g_{BI} < g_{BB}$. Is this obvious for large and negative g_{IB}?

Our response:

We agree with the referee that the prospective polaron properties are very interesting in the strongly attractive regime. However, one should emphasize that such ab-initio simulations are highly demanding since they require a substantially increased variational manifold to ensure numerical convergence especially for strong attractive interactions. Additionally, it is anticipated that an extended effective potential model would be necessary for the characterization of the emergent polaron state. This is indeed a highly intriguing research direction that we aim to undertake in forthcoming endeavors but certainly lies beyond the scope of the present work. Comments along these lines have been inserted in the revised manuscript to clarify the regime of validity of the present effective potential and highlight the importance to study polaron properties at strong attractive interactions in the future (see also the list of changes).

The referee writes:

5. On page 18, the authors state that detunings $\Delta < -3\omega_B$ are suitable for studying the repulsive polaron. This probably depends on the value of g_{IB}. Should the condition involve the polaron energy instead?

Our response:

Indeed the relevant detuning region depends on g_{IB}. Our statement in the main text refers to the $g_{IB} = 3g_{BB}$ case. In order to alleviate any possible confusion, in the revised manuscript, we explicitly state the discussed interaction region and further explain how the value of Δ can be estimated through the effective Hamiltonian approach.

The referee's recommendation:

Publish (meets expectations and criteria for this Journal). Our response:

We again thank the referee for their valuable comments and their recommendation.

List of changes in the revised manuscript:

1) Introduction, last paragraph of page 3. Added a short discussion on past effective potential models "We reveal that the system ... discussed [75]."

2) Sec. 3.1, page 6. Modified Fig. 1, removed previous panels (c), (d) and added a new panel (c) with a magnification of the near resonant region of panel (b). The caption was updated accordingly.

3) Sec. 3.1, page 6. Added a discussion on the existence of one-dimensional Bose polaron "Let us stress here ... one-dimensional confined systems."

4) Sec. 3.1, page 6. Added references to panel 1(c) in the text "however since ... see Fig. (1)". On the same note, in page 7 we modified the sentence "However, since $|E_{2\uparrow} - 2E_{1\uparrow}|$ ... see Fig. 1(c)."

5) Sec. 3.2, page 7. We have combined Fig. 1(c) and 1(d) into the new Fig. 2.

6) Sec 3.2, page 8. modified the symbols in the inline equations of the sentence "To establish the validity ... [Eq. (6), Eq. (7)]" with more accurate equations also a few lines below the correct expression for the residue is used "$Z = |\langle \Psi_{B+0\uparrow}|\hat{a}_0|\Psi_{B+1\uparrow}\rangle|$.

7) Sec 4, page 13. Modified the sentence on Tan's contact, " This behavior can be explained ... effective potential method", to stress the modification of the momentum distribution when a polaron is formed.

8) Sec 4, page 14, Table I. Updated the table with the percentages of change in residue attributed to the change of impurity and bath state. The caption is updated accordingly.

9) Sec 4, page 14. The last two paragraphs of Sec. 4 were rewritten to highlight the behavior of the residue and the different contributions and also to explicitly state that we are confident that the effective potential works only up to the $|g_{BI}| \approx g_{BB}$ case we have checked within ML-MCTDHX.

10) Sec 5.1, page 16. Modified Fig. 6 by removing panels (a$_2$), (b$_2$), (c$_2$), and (d$_2$). These were added to the new Fig. 7 appearing in page 17 of the same section. The caption was updated accordingly for both figures.

11) Page 19, last paragraph of Sec. 5.2, the specification "$\Delta \leq -3\omega_B$ for $g_{BI} = 1.5\sqrt{\hbar^3\omega_B/m_B}$" was added to avoid confusion. Also a few lines below we have added the sentence "As Fig. 8(b) reveals ... for varying" to explain how the regime of the stabilized polaron can be extracted from effective potential calculations.

12) Last paragraph of conclusions, page 20. Modified the sentence "The case of strong attraction ... emerges in this regime." To highlight strong attractive interactions as an important future perspective.

13) Page 15, added the new Appendix C containing the new Fig. 9, with the residue quantities Z_σ directly connected to many-body overlaps.