Dear Editor, Dear Reviewers,

We would like to thank you for your time in reviewing our paper and providing valuable comments that led to possible improvements in the current version. We have carefully considered the comments and tried our best to address every one of them. We hope that the manuscript after careful revisions, will meet your high standards. We welcome further constructive comments if any. Below, we provide the point-by-point responses.

Sincerely,

R. Photopoulos and A. Boulet

1) *In order to discuss the validity of their theoretical approach, the authors focus essentially on the ground-state energy as a function of the relevant physical parameter of the many-body Hamiltonian under consideration. However, in several illustrative models considered in their study, other relevant quantities such as correlation functions could be calculated. This would allow a better assessment of how close the N-body ground-state calculated in their approach is to the exact many-body ground state.*

We would like to thank the referee for discussing this interesting aspect. Actually, in principle, our approach allows to calculate correlation functions or to access other observables like the effective pairing gap or the one-body entropy [Phys. Rev. C **95**, 014326 (2017)]. For example, it could be interesting in a future work to compute with our approach the pair correlation function, the charge and magnetic structure factors in order to respectively study pairing, charge and spin correlations on the 2D Hubbard model and possibly compare with exact diagonalization datas obtained on 4×4 clusters [Phys. Rev. B **87**, 115136 (2013)]. That could certainly be a strong insight on the validity of our approach.

It is as well possible to calculate directly the overlap between the exact ground state and the one they calculate in their MBPT approach. For instance, this could be achieved relatively easily in the case of the four-site Hubbard model and even in the case of the Richardson pairing Hamiltonian.

We agree with the referee on this point. However, as mentioned by the referee, this is possible only in some restricted exactly solvable cases (and in the case of non-vanishing ground state energy: thus this seems difficult even for the 4-sites 2D Hubbard model in the strong limit coupling in which $\xi_0 = 0$). Moreover, the overlap could be a strong tool in order to chose a function $F_a(\lambda)$ defined in footnote 2 that allows qualitative reproduction of the GS energy. Instead of displaying overlap, we choose to plot the dimensionless energy E/E_0 that, we believe, is a good compromise to illustrate our developments and, in particular, the point discussed in section III.C.

2) *In Fig.4, which concerns the case of the one-dimensional Hubbard chain, the calculations corresponding to $l = 2$ (third order perturbation) are not shown, why? The authors should present the results the agreement should be better than for $l = 1$?*

In that particular case, the third order in perturbation theory vanishes, i.e. $\gamma_3 = 0$ (cf. table IV and equation (28)). Thus, the third order in

perturbation is equal to the second one.

3) *In the case of Hubbard's four-site model, it would appear, in the attractive case, that agreement decreases as the order of perturbation increases. For example, the agreement between the exact calculations and the MBPT calculations for $l = 0$ is excellent, whereas as the order of perturbation increases, it decreases. Do the authors have an explanation?*

We think that it is due to the fact that the linear divergence in the $U < 0$ regime renders difficult to reproduce what we define as the reduced energy $\overline{E}$. We assume that this is because the choice of the function $F_a(\lambda)$ defined in footnote 2 is not optimal in this case. Same effect as described in the question can also be observed for the Richardson Hamiltonian (for $g > 0$). Additionally, the plotting of quantities that diverge linearly are not very suitable to illustrate and observe convergences or our approach. Thus, when we have linear divergence of the GS energy, we plotted the reduced energy $\overline{E}$ only in such a way to observe convergence or our approach when the order in perturbation increases. Besides, we thank the referee to pointing out this because, by focusing on this case, we have corrected the misprint value of $\overline{\gamma}_1$ in table V.

4) *In Figures 1, 4 and 5, the left and right panels (a) and (b) are the same data plotted as a function of the relevant parameter or its inverse. In my opinion, the authors should choose one of them. There's no need to keep both, there's no advantage in doing so, it doesn't help to better understand their results and the comparison between the exact calculations and MBPT calculations.*

We partially agree because both limits are relevant in our study. Moreover, depending of the community, the GS energy is usually plotted in an arbitrary range of coupling constant (for instance, for Hubbard models, GS energy is usually plotted for $U/t \in [0, 8]$. But we understand the point of the referee, that is why we plot now our results only in the range $\lambda^{-1} \in [-1, 1]$ (figs. (b) and (d)) where λ denote either the coupling constant. We also reduced the range for λ (figs. (a) and (c)) to improve the readability of our results. Using this presentation of our results, the two limits (weak and strong coupling regimes) are clearly identified.