Manuscript: Tunneling with physics-informed RG flows in the anharmonic oscillator

The paper deals with the application of the recently developed physics-informed renormalization group (PIRG) flow to the quartically anharmonic double well quantum mechanical oscillator as a test to verify the capability of this approach to describe the instanton dominated regime, corresponding to small anharmonicity. In particular, the authors not only employ a novel flow but they also focus on the determination of a new parameter, indicated as a_{inst}, instead of the commonly studied energy gap ΔE.

The purpose of the paper is certainly interesting and it contains original ingredients, introduced to reach the prefixed goal. However, I find the comparison with the existing similar tests quite incomplete, so that the improvement achieved along this new route is less evident.

Here is the list of the issues that, in my opinion, the authors should reconsider:

1) The novel technique, the PIRG, was developed in advance and the explanation of many details is addressed to a group of papers quoted in the references (many of which co-authored by some of the authors of the present paper). This makes the understanding of the paper rather heavy and leaves few points obscure.

1.a) For instance the inequalities in Eq. (15) and the approximation in Eq. (16) should be supported by a direct measurement of ΔE, which is actually reported in Figure 7 and shows only a marginal improvement with respect to the existing literature. Then, just after Eq. (16), the authors say that they prefer to devise the new observable a_{inst}, therefore leaving the reader still questioning about the accuracy of the approximation in Eq. (16).

1.b) Another example is given by Eq. (35), that I find very difficult to derive from Appendix B2, as suggested by the authors, whereas it is crucial in determining Eqs. (37) and (40), which are at the heart of the main numerical test. In order to help the readers, I would suggest to expand on this point in the manuscript.

2) Again on the computation of ΔE in Appendix B1. We notice that the authors probe the instanton dominated region down to $\lambda_\phi = 0.22$ (according to their Eq. (33)) and claim that such highly non-perturbative region, dominated by exponential decay, starts below $\lambda_\phi = 0.4$. They also claim in the Introduction that none of the previous computations - available in the literature - could confirm the exponential decay and that, in addition, already at larger couplings, outside the exponential regime, sizeable deviations from the correct scaling with the couplings have been reported. Moreover, at the end of Section IV they state that, up until now, solving the Wetterich equation deep in the instanton regime ... was unsuccessful.

Now, according to the results shown in Figure 7, three numerical points taken from reference [7], lie below $\lambda_\phi = 0.4$, therefore within the instanton regime. Moreover, in reference [7] one finds two determinations of ΔE below $\lambda_\phi = 0.4$, obtained with the Wetterich equation, that, although with large errors, clearly show an exponential trend with the coupling, especially if the first order approximation in the derivative expansion gets compared with the LPA of the Wetterich equation.

Therefore, one must conclude that the statements of the authors quoted above are too strict with respect to the past results and too generous toward their own achievements. In fact, by looking at Figure 7, both the extension into the instanton regime and the actual determination of ΔE do not seem very much improved by their analysis which, in my opinion, would also require a more accurate analysis of the uncertainties related to the use of different regulators. In fact, according to [7], regulators different from the one reported in Figure 7, do further reduce the distance from the actual value of ΔE. Additionally, unlike stated by the authors, results reported in Figure 7 from [7] for larger λ_ϕ, seem in equal agreement with the actual ΔE as those from PIRG.

3) I understand that my criticism in **2)** concerns a point on which the authors intend to further improve. However, even for the main result of the paper, i.e. Eq. (45), it is evident from figure 4 that their analysis involves values of the couplings down to $\lambda_\phi \simeq 0.3$, which is not very different from the results collected in [7], so that the instanton dominated explored region is essentially the same. Moreover it would be extremely instructive to compare the result in Eq. (45) with the analogous number obtained by the standard (not PIRG) flow, possibly including the uncertainty coming from different regulators, to get a quantitative estimate of accuracy of this approach and substantial comparison with other analyses.

4) Some typos or mistakes to be corrected: **(A)** In the term proportional to λ_ϕ in the central term of Eq. (5.b), $1/\sqrt{2}$ is missing. **(B)** In the list of references of papers that analysed this issue in the past, the paper by A.S. Kapoyannis and N. Tetradis Phys.Lett.A 276 (2000) 225-232 e-Print: hep-th/0010180 [hep-th] should be included. **(C)** The quotation - PT RG from [6] - in Figure 7, left and right, is wrong and it should be changed into [7], coherently with the caption. Also in the Appendix B1 text, when the authors mention - the data taken from [7], Table II -, as far as I understand, they intend to refer to Table 4 of [7].

In conclusion I think that the paper in the present form is not suitable for publication, but it should be reconsidered after some amendments and suggested improvements are properly taken into account.