A report on "Hamiltonian Truncation Crafted for UV-divergent QFTs" by Delouche, Elias Miró, and Ingoldby

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This paper is an significant contribution to the development of truncated Hamiltonian methods in QFT. The authors propose to handle the UV divergent theories by starting with a locally regulated theory with counterterms to the Hamiltonian worked out and then passing to an effective truncated theory in which the local regulator is removed but additional terms are added to the bare Hamiltonian. This paper is a follow up from [13], authored by two of the present authors, and focusses on applications of the method to concrete examples. The paper is of high quality and deserves publication. I would like the authors to address a number of questions I list below in the hope of improving the paper. I will start with more substantial questions leaving some typos and notation issues to the end.

1. In (2.5) $H_{\rm CFT}$ and V are also regulated by ϵ. Only after we truncate the total perturbed Hamiltonian and take the $\epsilon \to 0$ these terms can be replaced by the bare truncated quantities. Perhaps the authors can make this clearer.

2. For the thermal perturbation of the Ising model, on Figure 2 the authors compare the ground state energy as a function of R computed by their numerical method to the exam analytic answer. This quantity is logarithmically divergent and its value depends on the subtraction point chosen. In (B.6) the subtraction is chosen such that a non-extensive term $R\log R$ is introduced. I fail to see how such a term can arise from the numerical side given by the bare Hamiltonian and the correction term (4.3) which is linear in R. Shouldn't such a finite counterterm $\sim R\log R$ be added to the effective truncated Hamiltonian as well to match the renormalisation scheme chosen in the exact answer? I would ask the authors to clarify this.

3. When assessing their method the authors concentrate on the truncation (in)dependence. I think this is quite right. But it can be complemented by looking at the R-dependence of the numerical quantities as well to see how the "physical window" of TCSA is affected. For example one can compare the region of R where the mass gap is constant obtained by the bare TCSA and with the corrections $K_{\rm eff}$. This could be done for the Ising model as that is the only massive perturbation considered in the paper.

4. For the model discussed in section 6 are the couplings g_3 and g_5 present in (6.8) the renormalised couplings? It seems to me the authors take them to be the bare couplings. The distinction is important because we have a coupling renormalisation for this model and it affects the way we label the RG trajectories for example the critical RG trajectory with $\alpha = \pi$? Please clarify this.

5. For the model in section 6 to determine criticality the authors do not present the energy-radius plots (the analogues of Figure 5 for the TIM to IM flow) but instead rely on fitting to an IR effective Hamiltonian at a fixed value of R. Any particular reason for that? Does looking for $1/R$ behaviour of the energy gaps not work well in this model? A comment on this would be helpful for practitioners.

6. Any idea of what can be happening at the other boundary of the PT-breaking region which is near $\pi = \pi/4$. If that is not a critical line can there be a first order transition? How does the mass gap behave there? I guess the authors may need to add the Z_2

breaking blocks in the Hamiltonian to fully investigate this region. Any comments on this would be helpful to the readers.

7. After (3.2) Δ_i are introduced as dimensions of primaries while later in (3.4), (3.5) it is used for descendant states as well. I would suggest a separate notation.
8. two typos in the last paragraph on page 22: "ploted" → "plotted", "parts is equally well" → "parts are equally well"
9. The last OPE in appendix C.1 should read $C^{(1,4)}_{(2,1),(2,1)} = 7/8$. (The same typo is present in [22].)