RESPONSE TO REFEREE #5

(i) "methodologically, the optimal control methodology follows standard procedures. The approach is in particular very close to the one used by the same author in recent works (Refs 24, 27), such that it is hard to consider it as a core novel contribution of this work. Can the author argue more strongly for the novelty of the approach proposed?"

I think that the methodology cannot be considered "standard", as I have only used a precedent of it in Ref. 24 (Castro and Sato). I am unaware of any work by any other authors using a similar methodology. And, this work improves over the previously one reported in Ref. 24 in that it addresses the problem of the quantum thermal machine, i.e. it uses heats and works as the key observables. This fact required an extension of the previous method, both in theory and regarding the code.

I have added a couple of paragraphs to the introduction clarifying the novelty of the approach a bit more.

(ii) "the applicability of the optimization procedure to more general problems is not much discussed: how computationally hard it is to go beyond the optimization of a single function and of a two-level system?"

I have added a paragraph, at the end of section 3, regarding this issue.

(iii) “The key motivation and advantage stated by the author to use the proposed method in the two-level example considered regards practical feasibility: smooth control functions are more realistic than quick piecewise-constant ones. However, no connection to potential experimental setups is given, which would provide a more concrete context and realistic constraints. What could be, for instance, realistic frequency cutoffs and driving periods T in a potential implementation?”

It is true that at this point, the work has only been theoretical. But one of the motivations of this work is that, in realistic situations, the experiments do have constraints and limitations -- not only frequencies, but also amplitudes, etc. Regardless of the magnitude of those constraints (that will depend on the particular setup, and I do not dare to estimate here), what we know is that they exist, and therefore a method to include them, such as this one, should be useful.

(iv) "some aspects of the open two-level model used are unclear to me: in how far

is it justified to continuously modulate the system with multiple harmonics, while not changing the description of the dissipative parts? [e.g., by switching to Floquet-Markov master equations, using the nomenclature of Phys Rep. 304 229—354 (1998), in the system's Floquet basis]."

This point has been raised by referee#2 (points 3 and 4); the same response applies here.

" - I appreciate the effort of the author to place the work in a broader context, but I also find the introduction at times distracting: the discussion of the actual contributions of this paper is, in my opinion, rather compressed as compared to the background."

I have changed the introduction, clarifying the nature of the contribution of this work. I am open to reducing the background discussion if necessary, but I personally prefer papers with lengthier and clear introductions.

" - concerning Floquet engineering and optimal control, the author writes, in the introduction, "recently, this author and collaborators have coupled this concept with OCT". However, the use of optimal control in the context of Floquet engineering dates quite back [I can think, e.g., of PRL 113, 010501 (2014)]. Various subsequent works also used similar approaches for optimized Floquet engineering [e.g., PRX 13, 031008 (2023), PRL 126, 250504 (2021)]."

I have changed that line to "recently, this author and collaborators have shown one possible method to couple this concept with OCT (see, for example [31, 32]; other methods have been proposed, see for example [33–35] in the field of quantum simulators". It is true that other authors have addressed the problem of optimizing Floquet systems in various ways.

" - in the same paragraph, the author states "[...] it can be termed as Floquet engineering of QHT". While, on the one hand, this wording might not hurt, it also seems to imply that the joint use of Floquet-engineering and open quantum systems theory in the context of quantum heat engines is a novel aspect of this work. If so, can the author clarify what aspects are novel in this respect?"

The new paragraphs of the introduction (highlighted in red) clarify better, I hope, what is the novelty of the work.
