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The authors try to obtain time-dependent generalizations of a couple of basic theorems describing certain properties of time-independent non-Hermitian Hamiltonians. They apply their general results to a simple quadratic Hamiltonian and mention another application to a different quadratic Hamiltonian which is to appear in a separate publication. Here are my specific remarks.

1. The introduction is too wordy. Parts of it seem redundant or unnecessary. There is no discussion of the concrete motivation for this work. Is there a specific physics problem that the authors' results can shed light upon? Or is this an example of generalize because you can?

2. The authors miss to cite the early publications where time-dependent pseudo-Hermitian operators were originally studied. See form example quant-ph/0306200, quant-ph/0604014, arXiv:0706.1872, and references therein.

3. The authors claim that "The $\mathcal{PT}$-symmetry condition," is "much less demanding than that of Hermiticity, $\cdots$." This is simply false, because $\mathcal{PT}$-symmetric Hamiltonians $H = p^2 + v(x)$, with the standard definition of $\mathcal{P}$ and $\mathcal{T}$, satisfy $H^\dagger = \mathcal{P}H\mathcal{P}$ which means that $H\mathcal{P}$ is Hermitian. Therefore demanding $\mathcal{PT}$-symmetry of H is equivalent to demanding Hermiticity of $H\mathcal{P}$.

4. In page 4 the authors write: "This in a way gives us physical support to extend the scope of pseudo-Hermitian Hamiltonians beyond those invariants under $\mathcal{PT}$ operation." This gives the impression that the scope of pseudo-Hermitian Hamiltonians was previously limited to $\mathcal{PT}$-symmetry which is not true. Pseudo-Hermiticity was introduced in an attempt to provide a general framework that would encompass $\mathcal{PT}$-symmetry and clarify the multitude of claims about its niceties.

5. The authors define time-dependent pseudo-Hermiticity via eq. (2). If ρ is time-independent this reduces to "ρ-pseudo-Hermiticity" which is, strictly speaking, not the same as "pseudo-Hermiticity." The latter demands the existence of an invertible Hermitian operator ρ (which needs not be positive-definite) satisfying eq. (7). Eq. (2) should be identified with the condition for "$\rho(t)$-pseudo-Hermiticity for a TD Hamiltonian" where $\rho(t)$ is a given positive-definite metric operator. This is actually not an unimportant technicality. If we identify TD pseudo-Hermiticity by the condition of the existence of a metric operator $\rho(t)$ such that (2) holds, we essentially put no restriction on the Hamiltonian $H(t)$. This is because we can solve (2) for $\rho(t)$ and determine $\rho(t)$ for every $H(t)$. This is actually done in math-ph/0209014. For this reason the authors must make it clear that they fix $\rho(t)$ first and study time-dependent $\rho(t)$-pseudo-Hermitian Hamiltonians which are defined by their eq. (2). Of course this leads one to the question: "How and why should one fix a particular $\rho(t)$ and study the Hamiltonians that satisfy eq. (2)?"

6. In Section III, the authors write "we assume the symmetry to be TD operator." in which "symmetry" should change to "symmetry generator." I do not understand the reason for this assumption. Indeed calling $I(t)$ a "symmetry" or "symmetry generator" is rather inappropriate. This is because eq. (9) is more of a restriction on $I(t)$ than a restriction on $H(t)$. In practice a quantum system is determined by a given Hamiltonian operator and one solves (9) to find an invariant operator $I(t)$.

7. I do not think eq. (10) defines a dynamical invariant when $H(t)$ is a non-Hermitian Hamiltonian operator. This is actually easy to see. One can write anti-linear invariant of eq. (9) as $\mathcal{T}\tilde{I}(t)$ where $\tilde{I}(t)$ is linear, and use $H(-t)\mathcal{T} = \mathcal{T}H(t)^\dagger$ and $i\mathcal{T} = -\mathcal{T}i$ in (9) to arrive at

$$i\frac{d\tilde{I}(t)}{dt} - H(t)^\dagger\tilde{I}(t) + \tilde{I}(t)H(t) = 0,$$

which is the correct relation for the linear dynamical invariant $\tilde{I}(t)$ when $H(t)$ is non-Hermitian.

8. Why do the authors decompose $I(t)$ as the product of a time-dependent unitary or antiunitary operator $\Lambda(t)$ and a linear operator $\mathcal{U}(t)$? They could simply identify $\Lambda(t)$ with the identity operator and $\mathcal{T}$ for cases where $I(t)$ is linear and antilinear, respectively.

9. I find the analysis of Section V to be based on various ad hoc choices and formal manipulations. The basic physical motivation for the calculations reported in this section is missing. Do the results of these calculations lead to a concrete physical prediction about the behavior of the model the authors consider?

In view of the above remarks I do not find this paper suitable for publication in SciencePost in its present form. However, I am willing to reconsider my decision, if the authors can satisfactorily address the issues I have raised and improve the presentation of their manuscript substantially. They should in particular discuss the impact of their findings in dealing with concrete physics problems.