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Review text:

The cca 80 years of development of quantum mechanics may be characterized by the sharp contrast between the robust stability of its abstract principles and the permanent emergence of dramatic changes in their concrete interpretation and use. The Mostafazadeh's extensive review of what he calls pseudo-Hermitian quantum mechanics may be read as one of the most recent reconfirmations of this permanent truth.

The brief history of development of this subfield of research has a well defined starting point, viz, the Bender's and Boettcher's thoughts-provoking 1998 letter [32]. In it, the authors conjectured the reality of spectrum of certain Hamiltonians (explicitly, of $H = p^2 + g(x)x^2$ with $g(x) = (ix)^\delta$ and with $\delta \geq 0$) which *seem* manifestly non-Hermitian (note and emphasize: in an irrelevant, unphysical, auxiliary, "friendly" as well as "false" representation $\mathcal{H}^{(F)}$ of the Hilbert space of states) but which are, in fact, Hermitian (note and emphasize: in another, physical, "standard" though a bit "sophisticated" representation $\mathcal{H}^{(S)}$ of the Hilbert space of states [this notation convention is mine]).

Also the start of activity of Ali Mostafazadeh in the field has a strictly defined date [viz., my own seminar "Recent renewal of interest in non-Hermitian Hamiltonians in quantum mechanics" (Koç University, May 3, 2001, 4.45 pm)] and motivation (viz., "a critical assessment" and "a lucid and precise formulation of a unitary quantum theory based on a non-Hermitian Hamiltonian" – in this sense, his present review paper is just a climax and summary of his efforts documented by the list of his own more than 40 papers reviewed and listed in References).

Needless to say, it is always admirable and exceptionally useful when somebody like AM decides to write an extensive review paper. This is a hard work, just partially rewarded by the implicit privilege of promoting the author's own views and interpretations of the whole story. On the side of readers, for a compensation, let me recommend, therefore, a complementary parallel reading of the perceivably older and, in many a respect, "re-discovered" review [207] by Scholtz et al [who would rather call the subject a "quasi-Hermitian representation of Quantum Mechanics" in the message which I also recently retold in MZ, "Three-Hilbert-space formulation of Quantum Mechanics", SIGMA 5 (2009), 001, arXiv:0901.0700], or of the equally important alternative recent review-style long summaries of the subject by Bender [30] and/or by Dorey et al [80].

Every author of the review of a similar quickly developing branch of physics always takes the risk of finding his text getting obsolete after a few months (note, e.g., that AM hanged the first version of his review on web almost two years before it appeared in print). In the present case one may notice, for example, the current and unexpectedly quick shift of emphasis from the concept of "quasi-Hermiticity" (i.e., from the study of the "hiddenly Hermitian" observables exhibiting real spectra, as almost exclusively covered by the AM's text) to the problems of phase transitions (often called a "spontaneous breakdown of PT symmetry", strongly experimentally supported and motivated) and to the intensified theoretical study of the "horizons" (i.e., e.g., of the Kato's "exceptional points" in optical systems – cf. the pioneering paper [134]). One might also mention here the emergence of new perspectives (e.g., in classical electrodynamics - cf. [176]) and of many fresh results on open systems as reported during the tenth PHHQP international workshop [http://www.pks.mpg.de/ phhqpx11/] (this meeting proved largely dedicated to the truly experimental applicability of the theoretical and mathematical concept of pseudo-Hermiticity *beyond* its hiddenly Hermitian, real-spectra-exhibiting subdomain).

I do not deny that globally I admire the AMs work as presented in refs. [138] + [140] - [180] as well as in the present review. I would also endorse the vast majority of his assertions. In more detail, I would certainly see many things slightly differently, nevertheless. *Pars pro toto*, I missed the comment on the role of the "charge" $\mathcal{C}$ in making the metric unique. Also my own presentation of the whole formalism would not start from the definition of " a unitary quantum system ... [where] one modifies the inner product of the Hilbert space" but rather, in the spirit of Scholtz et al [207], from the fundamental role played by the Riesz bases and by the Dyson operator (which may be easily made time-dependent). In this sense I even completely disagree with the unnecessarily narrowed perspective and conclusions presented in chapter 7. My own point of view may be found described in MZ, "Time-dependent version of cryptohermitian quantum theory", Phys. Rev. D 78 (2008) 085003.

This being said, the AM's review must be recommended as an obligatory reading. In a compact and mathematically disciplined form he covers a big portion of the basic ideas and techniques in the field. On purpose he avoided virtually all of the formal results (notably, on exactly solvable models) and speculative ideas (including, regrettably, also all of the underlying extensive mathematics of Krein spaces) as not having, by the author's opinion, any direct or concrete implications for the development of quantum mechanics. In the same vein, just a few references are provided on supersymmetry or weak pseudo-Hermiticity. No coverage is provided of the recently quite popular cryptohermitian systems on lattices or graphs (for a complementary reading in this direction see MZ, "Fundamental length in quantum theories with PT-symmetric Hamiltonians II: The case of quantum graphs", Phys. Rev. D. 80 (2009) 105004).

The acceptance of all of the above-mentioned limitations is quite natural - the space provided by the Journal makes even the coverage of some of the selected subjects very sketchy (cf. e.g., just the four lines devoted to the MHD systems). One should rather underline the high quality of the "tours de force" of the report (e.g., chapter 4 on the calculations of metric operators, or the compact chapter 6 which clarifies may subtleties of the quantum-classical correspondence). At the same time, interested readers should read, e.g., MZ, "Scattering theory using smeared non-Hermitian potentials", Phys. Rev. D. 80 (2009) 045009 (containing also further references) in order to fill one of the most serious gaps in the presented theory and to see the feasibility of the extension of the pseudo-Hermitian quantum mechanics to the dynamical regime of the causal and *unitary* scattering in which the conservation of a global or asymptotic flow of probability is achieved in spite of the manifest non-Hermiticity of the Hamiltonian in $\mathcal{H}^{(F)}$.