Uniwersytet Gdański

International Centre for Theory of Quantum Technologies

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Recenzja rozprawy doktorskiej
Mgr. Alexandre de Oliveira Juniora
z Uniwersytetu Jagiellońskiego
pt. „geometric and information-theoretic aspects of quantum thermodynamics"

The PhD thesis by Alexandre de Oliveira Junior has been prepared under the supervision of dr hab. Kamil Korzekwa. It is in a form of a proper dissertation with 9 sections, which together with an impressive number of 286 references give almost 200 pages of a dense material. Already at the beginning, I would like to emphasize that concerning presentation aspects, the thesis is much above the average. The author took a lot of effort to organize the material in a way that is convenient to read for people both at different levels of „specialization", as well as of different scientific interests. Motivation, results, derivations and examples are always separated in a way which maximizes the flow of the presentation. Moreover, the second section in a nutshell tells the reader what has been done in the thesis, in a smart way comparing the summary of the state of the art with the summary of new results. Then, two background sections aim at summarizing formal concepts which are required to follow the rest of the thesis. Staying with presentation aspects, it is a common practice by the author to use the format in which the margin is bigger than usual, and in many cases contains supporting supplementary material. Again, content which is not crucial, but can be interesting or helpful for some readers is being separated from the main story, in order to keep it going. I like that approach and can only recommend the others to follow it.

The proper part of the thesis contains four independent sections. I am not going to summarize their content, since the author already did it very well and in a very concise way in Sec. 2. However, I have got a feeling that the last section about cavity QED is quite disentangled from the rest of the thesis. In my opinion, while also original, it could be removed from the dissertation without a visible decrease of its very good quality. Also, I am not sure if the PhD candidate worked on this particular project together with his advisor, or it was rather an external collaboration. Let me also note in passing, that a bit of a drawback of the thesis is that all the crucial results have been obtained together with other PhD students. For example, the paper [2] is accompanied by an asterix with a claim that the first author (J. Czartowski) contributed equally with the author of the thesis. We lack a similar asterix in [3], even though it would not be surprising if the situation is reciprocal to that of [2],

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and in the future thesis of J. Czartowski a similar asterix will accompany [3]. A similar situation appears in [6], where the first author T. Biswas was to my knowledge also a PhD student at that time. Notably, there is no asterix-like disclaimer in this paper.

My overall assessment of the content provided in the crucial sections is that the author did (and reported) a very solid piece of research. Its quality, in my opinion, is above the average, so my pre-conclusion is that the author completed his PhD tasks very well and undoubtfully deserves the PhD degree.

However, there are a few points which I would like to comment on, also to give the author some food for thoughts before the defense. First of all, and this is maybe my personal opinion (though I am not the only one), quantum thermodynamics is not yet a mature and well-established field. By this I mean that phenomenological thermodynamics assumes that there are many parameters of the system which we do not control or access, so that we can only operate with very limited information. How to translate this spirit to quantum domain is, in my view, an open problem. Therefore, usual motivation, also raised by the author, is quite information-theoretic. This is neither a problem from the point of view of mathematics, nor quantum mechanics. However, it seems quite delicate a matter when the word thermodynamics is involved in the discussion. Overall, I missed that aspect in the thesis. For example, on the one hand side, the author provides the second law of thermodynamics already on the first page of his thesis. I like the formulation used there, as it contains the word „spontaneous", which for me is the crucial aspect of the second law, when thermodynamics is in the focus. On the other hand, all further results are more general, stemming from the dynamical perspective. Therefore, while formal conclusions are reached, it would be valuable to discuss them „against" the spirit of thermodynamics. For the sake of PhD defence, I will further just bring one such case. In section 5, the thermodynamic arrow of time is discussed, the latter commonly being associated with the second law. How do the findings of the author about the cones really connect to the thermodynamic problem of the arrow of time? Are arbitrary thermal operations a quantum analogue of the most general spontaneous physical processes? Is the sole existence of the cones a signature of the time arrow, or rather, is there maybe something specific in their particular construction, discovered by the author, which has an additional physical meaning? We can suspect that any family of operations will have past and future cones, as well as incompatible regions. So what is „thermodynamically special" about the discovered formulas describing the sets?

A few more questions/comments to the author are the following:

1. In Sec. 4.1 the author used „H" for the system Hamiltonian. It would be less confusing to use H_S, since the bath is governed by H_E.
2. I have problems with understanding the example in the box around Eq. 4.10. The sentence below this equation sounds trivial, because U is the exponent of the Hamiltonian H, so it has to commute with H, regardless of the resonance or its lack.

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Also, is H in Eq. 4.10 the system Hamiltonian or the total Hamiltonian (cf. previous question)?

3. Concerning the thermal cones in Sec. 5, my question is about whether the past and future cones do have to have an empty intersection (this property has been used, maybe up to measure zero sets). Formally, I understand why this happens, but from a thermodynamic point of view it is not intuitive. For processes in which the thermodynamic entropy does not change the overall landscape could be different. Somehow, it would look like a state can be both in past and in future cone. I acknowledge the Observation 5.5.1, still, I would like to hear a physically-motivated comment about that.
4. Interpretation of the volumes, like in Eq. 5.23 etc. holds under the assumption that the ball fully belongs to the future cone. While we assume that „q" does belong to this cone, depending on q and \epsilon, the ball could stick outside of the cone (then the interpretation does not hold fully). Also, is it clear whether the above „membership" problem (does the whole ball belong to the cone or not) is in general solvable (e.g. not NP hard)?
5. In Sec. 6 the author used the word „algoritheoremic". What is the meaning of that word?
6. I do not understand the comment after Eq. 6.39. This quantity seems always positive.
7. From and to which spaces do the operation in Eq. 6.41 act?
8. In Sec. 7, why is the covariance matrix taken as above Eq. 7.122?

At the end, even though I very highly rate the presentation of the material, I feel that Sec. 3 has been written with a slightly lower engagement. On pages 1 and 2 of this section we find:

A. „finite or infinite" without the word „dimensional". What is a finite vector space?
B. It is said that Hermiticity ensures real eigenvalues, while it rather ensures real average values of observables.
C. What is a „unidimensional" projector? Maybe a word „rank" would be more appropriate?
D. When on the margin the transformation U(t) is explained, the expansion is not that of time ordering. This is because every integral has the same upper limit „t". Also, it is unclear to me why to denote that operator with „exp_+" instead of \mathcal{T} before the „exp".
E. The sentence above Eq. 3.3 seems incorrect. These, in general, are not the eigenstates of the Hamiltonian.
F. In 3.4 the Lindbladian part looks time independent, while in 3.5 it acquires a subscript „t".

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Podsumowanie

Do najmocniejszych stron pracy zaliczam jej solidność, a także wysokie walory natury matematycznej. Do tego, na korzyść pracy przemawia ponadprzeciętna (a wręcz wybitna) klarowność wywodu i bardzo wysoka dbałość o szczegóły.

Za trochę słabe w rozprawie uważam to, że zabrakło głębszej dyskusji otrzymanych wyników w kontekście termodynamiki, gdyż być może pozwoliłoby to albo ugruntować termodynamikę kwantową jako „coś więcej" niż gałąź kwantowej informacji, albo ją ulepszyć pod kątem założeń metodologicznych. Wreszcie, rozprawa, choć jak już wielokrotnie wspomniałem, jest bardzo klarownie i starannie napisana, wydaje się być w pewnym stopniu rozbudowana na siłę. Uważam, że można by spokojnie pominąć rozdział 8. Warto by też w bardziej precyzyjny sposób wskazać wkład autora w porównaniu z innymi doktorantami zaangażowanymi w badania.

Mimo powyższych drobnych uwag krytycznych stwierdzam, iż całość **spełnia wymagania stawiane rozprawom doktorskim w Art. 187. Ustawy.**

Z wyrazami szacunku

Dr hab. Łukasz Rudnicki, prof. UG

Signed by /
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