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Subject: Report on doctoral thesis (PhD candidate Julia Amoros-Binefa)

To whom it may concern,

It has been a real pleasure to read and evaluate the PhD thesis of Julia Amoros-Binefa, entitled "*Real-time optimal quantum control for atomic magnetometers with decoherence*."

This work addresses a broad and timely set of topics at the interface of quantum metrology, open quantum systems, optimal (classical) control theory, and experiments with atomic ensembles. The results, which have already led to three manuscripts (one published in New Journal of Physics and two recently submitted to the arXiv), represent contributions at the research frontier of these fields. They are of clear relevance both from a fundamental and a practical perspective: on the one hand, by establishing ultimate precision bounds for atomic magnetometers in the presence of decoherence; on the other, by proposing concrete control strategies that enable one to approach these limits in realistic atomic magnetometers.

Beyond the significance of the results themselves, the thesis also demonstrates an excellent effort in carefully presenting the necessary background to fully appreciate the derivations and implications of the main findings. The candidate provides thorough explanations and a clear personal perspective throughout, which greatly enhances both the readability and the pedagogical value of the manuscript. I can confidently say that I learned a great deal while reading this work, and I am certain that I will return to it as a valuable reference for myself, my students, and my collaborators whenever I need to revisit the concepts it so clearly and comprehensively covers.

The thesis is structured as follows. The first three chapters provide background material: Chapter 1 covers preliminary notions of probability theory and quantum mechanics; Chapter 2 presents an extended introduction to classical estimation theory, with particular emphasis on Bayesian filtering; and Chapter 3 focuses on continuously monitored quantum systems. The subsequent chapters are devoted to the candidate's original contributions, including the derivation of ultimate bounds for frequency estimation in the presence of decoherence, as well as the development and assessment of control and estimation strategies designed to attain these bounds in realistic atomic magnetometers.

In the following, I will discuss each chapter separately, presenting their content in more detail and adding, where appropriate, remarks and questions that I hope the candidate will be able to elaborate on during the defense. I will also provide a number of minor comments, including corrections of typographical errors and small suggestions for improvement. As I mentioned above, I sincerely hope that this thesis will be made publicly available in some form, as it could serve as a valuable reference for researchers approaching these topics or already working in the field. For this reason, I believe that even these minor comments and suggestions may prove useful.

Chapter 1.

In the first chapter, the candidate presents the essential background material. The exposition begins with probability theory, introducing the main definitions, theorems, and proofs concerning: i) random variables; ii) stochastic processes; and iii) stochastic (Itô) calculus. The final part of the chapter is devoted to quantum mechanics, with a particular focus on open quantum systems and finite-dimensional (spin) systems. I especially appreciated the excellent balance between restricting the discussion to the necessary concepts and providing precise definitions and complete proofs where appropriate. With regard to this chapter, I have only a few minor remarks and suggestions:

- pag. 4, line 9: "Wiener function" should read "Wigner function"
- pag. 8, def. 1.4: To avoid any confusion, I would suggest to use a different symbol for the domain, instead of $\mathcal{X}$; in the definition 1.3 just above, the same symbol has been used to denote the whole set of possible values of the random variable.
- pag. 9, def. 1.8: in this definition, I think that one should not introduce and fix the intervals $[a, b]$ and $[c, d]$ before Eq. (1.13). What I mean is that Eq. (1.13) should hold for any x and y in the whole sets of values, such that, once we fix ANY intervals $[a, b]$ and $[c, d]$, then (1.14) holds too.
- pag. 15-18, Sec. 1.2.2: I do appreciate the way Poisson increments and Poisson processes are introduced. However in the whole discussion, one always considers a time-independent rate λ. However, when dealing with continuously monitored quantum systems via photodetection, the rate will be typically time-dependent, as it depends on the expectation value $\langle \hat{L}^\dagger \hat{L} \rangle_c$ on the conditional state (see e.g. Eq. (3.45)). In this case, the probability distribution of the corresponding Poisson process cannot be written as a simple Poisson distribution, as in Eq. (1.53). I am not suggesting to provide the full description with a time-dependent rate, but I believe that a final comment about the fact that the rate can be time-dependent and on its consequences, would be helpful here.
- First line in Sec. 1.3.9: it looks to me that there is no enough introduction on what one means with LG system. It was mentioned in the introductory part, but here it comes a little bit "out of the blue". Maybe just a couple of lines of introduction on it could be useful and enough for a potential reader to understand the line of reasoning.
- pag. 52, Eq. (1.230): the equation can be more easily written in terms of the Wigner function of the $\hat{O}$ operator, as it is clear from Eq. (1.231).

Chapter 2.

In the second chapter, the candidate addresses the topic of Bayesian estimation and control. I found this chapter particularly enjoyable and instructive, as I learned a great deal while reading it. The candidate has done an excellent job of distilling the material from standard references in the field (such as Ref. [29]), while still providing sufficient detail and proofs to enable the reader to follow the reasoning thoroughly. Moreover, the discussion offers a clear introduction to key methods such as the Kalman filter (KF), the extended Kalman filter (EKF), and linear-quadratic-Gaussian (LQG) control. My remarks and suggestions regarding this chapter are only minor.:

- pag. 56, Eq. (2.1): the equation should read $p(\theta \mid y) \propto p(y \mid \theta)p(\theta)$, i.e. the likelihood has been wrongly written as $p(\theta \mid y)$. This is clearly a typo, as the whole chapter is based on the correct formula for the Bayes formula. Unfortunately it has been placed at the beginning of the chapter, and I strongly recommend the candidate to correct it.
- pag. 64, Fig. 2.1: what about plotting also the prior distribution, along with posterior and likelihood?
- pag. 67, below Eq. (2.34): I would avoid here to refer to a specific physical parameter (Larmor frequency) in this chapter, as the whole discussion is in fact general.
- pag. 68, second paragraph: I am possibly missing something, but am not sure if I agree with the sentence: "*The measurement model given by Eq. (2.36) relates the state vector to the observation vector and outlines how the measurement updates the state of the system*". It looks to me that Eq. (2.36) is actually telling the opposite: how the measurement result depends on the system x_k (and on the measurement noise r_k).
- pag. 77, about Eqs. (2.92) and (2.93): I would suggest to add another version of these equations substituting the Kalman gain, so that it is made explicit that the equation is quadratic in $\Sigma(t)$ (very minor suggestion).
- pag. 83, Eq. (2.124): keeping the same symbol $K(t)$ for the "old" and "new" Kalman gain is somehow confusing. Probably I would avoid to use $K(t)$ for the old Kalman gain in thie equation, and thus just write it as $\Sigma(t)H^T(t)R^{-1}(t)$.
- pag. 85, Eq. (2.130) and following proof: I have to admit that I was a little bit confused about over which probability distribution one is averaging in Eq. (2.130), as denoted by $\mathbb{E}[\,\cdot\,]$.
- pag. 99, Eq. (2.210): in the second line I think that all the $\mathbf{x}(t)$ should be $\widetilde{\mathbf{x}}(t)$

Chapter 3.

In the third chapter, the candidate introduces the topic of continuously monitored quantum systems, presenting a clear and detailed derivation of the stochastic master equations for continuous photodetection and homodyne detection. Particular care is devoted to explaining the approximations underlying these derivations, which I greatly appreciated. I may be somewhat biased in saying so, as this is a subject I am already quite familiar with, but I nonetheless found the chapter especially valuable: the candidate has succeeded in putting together material from different sources into a coherent, comprehensive, and very well-structured discussion. I have also appreciated the section where it is explained why polarization spectroscopy can be described by the SME corresponding to homodyne detection.

There is however only one aspect that, from my perspective—and although not essential for the purposes of the thesis—I was expecting to see discussed in this chapter. I will briefly outline it here. While there may not be much for the candidate to elaborate on, more than what I am going to write here below, it could still be a fruitful point for discussion during the defense.

Specifically, when considering a bosonic (continuous-variable) system governed by a Hamiltonian at most quadratic in the quadrature operators, subject to Lindblad operators linear in the quadratures, and continuously monitored via general-dyne detection (such as homodyne or heterodyne), the state remains Gaussian throughout the entire evolution. In this case, the dynamics can be described completely in terms of the first and second moments of the quadrature operators. Rather than working directly with the SME, one can instead derive closed evolution equations for these moments. This approach has been presented in several works (see, e.g., "Wiseman and Doherty, Phys. Rev. Lett. 94, 070405 (2005)"; for more pedagogical treatments, "Genoni, Lami, and Serafini, Contemp. Phys. 57, 331 (2016)"; or, without derivation, Sec. V of Ref. [119]). Remarkably, these equations take exactly the same form as the Kalman filter (KF) equations. The interpretation, however, is slightly different: in the KF, they describe the evolution of the estimated state, while for Gaussian systems they capture the actual dynamics of the quantum state. This correspondence can be understood by noting that, for Gaussian states, the evolution indeed represents the optimal Bayesian update of the state conditioned on the measurement outcomes. This is precisely why, as shown in Chapter 5, for the atomic magnetometer SME (involving only collective operators and in regimes where the Holstein–Primakoff approximation holds), the KF equations fully describe the system's dynamics.

Given the thorough treatment of both the KF and the SME in the thesis, I believe that including such a discussion here would have been a natural and valuable addition.

Here below I will add my other minor remarks/corrections and suggestions:

- pag. 104, second line: the word "system" is repeated twice.
- pag. 113, 4th line below Eq. (3.21): the sentence does not sound correct to me (I believe something is missing).
- pag. 118, after Eq. (3.45): As I already noted in the comments on Chapter 1, I believe it is important to emphasize that, in general, the rate of the Poisson increment associated with continuous photodetection is time-dependent.
- pag. 118, after Eq. (3.48) or at the end of the section: as it has been highlighted in the following section regarding homodyne detection, I would make explictly clear that adding the measurement-based feedback map would work assuming that one does not apply the so-called Markovian feedback (which does indeed correspond to ask that the corresponding Lindblad map is of $O(1)$).
- pag. 131, after Eq. (3.104): it is mainly just a matter of notation and convention, but since you call this quantity the "feedback Hamiltonian", then I would suggest to write it as $\hat{H}_f = \hat{F}\, I(t)$, where $I(t) = dy_t/dt$, such that the corresponding unitary evolution for a time dt is obtained as usual from the formula:
$$U(t, t+dt) = \exp\{-i\hat{H}_f dt\} = \exp\{-i\hat{F}dy_t\},$$
which is indeed the desired result.

Chapter 4.

In the fourth chapter, the candidate presents the first set of original results of the thesis: the derivation of the ultimate precision limit for frequency estimation protocols in atomic magnetometers subject to decoherence (both collective and local, i.e. acting independently on each atom). Rather than beginning with a broad survey of existing results/methods in the literature—such as the CS approach to bounding precision in the presence of decoherence—the candidate chose a more constructive path, gradually building up the derivation and leading the reader step by step to the final result. I find this to be an excellent choice, as it makes the chapter highly accessible and very pleasant to read.

Here below I list my minor remarks and suggestions.

- pag. 139, beginning of Sec. 4.2.1: I would remove the reference to "our earlier work [69]", as collective decoherence is going to be actually considered in this chapter as well.
- pag. 147, two lines below Eq. (4.51): in order to be consistent with the rest of the thesis, I would add $\sqrt{q_\omega}$ in Eq. (4.51), and leave the Wiener increment dW_t with the usual property $\mathbb{E}[dW_t^2] = dt$.
- pag. 151, Eq. (4.71): I admit that I did not really go through the derivation and I was a little bit confused about the notation of V_+^k ant the other similar matrices. I may have missed something trivial, but what does the k-dependence stand for? it is because the matrices depend on $V_P^{(k)}$ or because they are matrix power with exponent k? In the first case, one should probably modify the definitions in the equation below; in the second case I would put some parenthesis, to avoid confusion between exponents and superscripts.

Chapter 5.

In the fifth chapter, the candidate finally presents the control and estimation protocols based on continuously monitored atomic magnetometers in the presence of decoherence. In particular they first present the co-moving Gaussian (CoG)-approximation, validating it against the results obtained via the actual simulation of the SME in the Hilbert space (for reasonably low values of N). Then they show how to treat the estimation problem via an EKF and present a control protocol, optimized according to LQG-control by exploiting the CoG approximation.

These estimation and control strategies are then assessed by comparing them to a naive (frequency compensation) control strategy, and ultimately to the bounds derived in the previous chapter. This has been done both for the estimation of i) a static magnetic field, ii) a fluctuating magnetic field, and iii) for a noisy waveform estimation, resembling the one obtainable from a magnetocardiogram. The results are very interesting and relevant and they clearly pave the way to the application and assessment of atomic magnetometers for real-world applications in the next future. I have few observations and remarks that I hope the candidate could address during the defense:

- what is the difference between the CoG-approximation and a cumulant expansion approximation up to the 2nd-order?
- The optimal control strategy obtained via LQR design is given in Eq. (5.66), with its derivation detailed in Appendix E.6. Interestingly, this strategy does not depend on the "cost parameter" p_ω, which is introduced to penalize deviations of $\omega(t)$ from zero. I would be curious to hear the candidate's interpretation of this result: why this parameter does not appear in the final control law? While I understand that this is an analytical outcome—and that providing an intuitive explanation may not be straightforward—I believe it would be valuable to reflect on its meaning.
- I just want to be sure that I have fully understood the line of reasoning behind the design of the estimation and control strategy described in Sec. 5.3.3: the idea is that I use the EKF to estimate the parameter, on the other hand, assuming that my control strategy will work well, I suppose that I can consider a "linear-model" where to apply LQG control, and then derive the "supposed to-be optimal" strategy. We then include this strategy in the (nonlinear) EKF, and assess the performance of the corresponding estimation strategy. Is my interpretation correct?
- As shown in Fig. 5.8(d), the EKF appears to overestimate the spin squeezing of the conditional states. Could the candidate provide an interpretation of this effect? Do you have evidence that the overestimation decreases as N increases? If I understood correctly, in the results obtained through simulations based on the CoG approximation for larger values of N, this behaviour no longer appears.
- In Fig. 5.11(d) one observes that the EKF covariance is below the quantum limit. What is the interpretation of this result? Is that because the estimator is biased due to the mismatched parameters given to describe the OU-process?

I have then the following minor remarks and suggestions

- pag. 158, Fig. 5.1: I believe that (blue arrow) and (red arrow) are inverted in the caption of the figure; furthermore in the caption one refers to a panel (b), but there is no panel (b).
- pag. 189, Fig. 5.10: in the caption one reads "after gathering only $\approx 0.01 ms$ of photocurrent data". However in the plot the x-axis goes up to $t = 10\, ms$. In the same caption, the EKF covariance is denoted as (grey) while in the plot is (dashed yellow), and the quantum limit is denoted as (dashed black), while it is (solid black).
- pag. 194, Fig. 5.13: in the caption there is a wrong reference to a SME (??).
- In the same figure above, what was the choice for the magnetic field. Was it static or fluctuating?

To conclude, my overall assessment of the PhD thesis is highly positive, and I therefore recommend that Ms. Julia Amoros-Binefa be granted the opportunity to defend her work in an oral examination. Although I have raised a few questions, these in no way undermine the quality of the thesis; rather, they provide a valuable starting point for further scientific discussion. As noted earlier, I have also suggested a number of minor comments and improvements, with the conviction that this thesis has the potential to become a significant reference for future researchers in the field, thanks to both the relevance of the results obtained and the candidate's substantial personal contribution in developing and re-elaborating the underlying topics. In fact, I consider this thesis to be among the top 5% of those I have had the pleasure to read and review, and for this reason I also recommend it for distinction.

Sincerely yours,

Marco G. Genoni