Peer Review File

Manuscript Title: Distributed quantum sensing with mode-entangled spin-squeezed atomic states

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

I have with interest read the manuscript "Distributed quantum sensing with a mode-entangled network of spin-squeezed atomic states" by B. K. Mailia et al. Below, I provide the details of my review.

Summary of key results
In the manuscript, the authors report on an experimental demonstration of mode entanglement between atomic ensembles trapped within an optical cavity. They demonstrate entanglement between 2 and 4 spatially separate atomic ensembles in the cavity though a collective quantum non-demolition measurement of the atomic spins and demonstrate how the mode entanglement increases sensitivity in atomic clock type measurements and atomic interferometry (2 modes only).

Originality and significance
I do find the results of the authors very impressive, and I enjoyed reading the manuscript. However, I am not really convinced that the originality and significance of the work is suitable for nature. Previous work has already demonstrated how mode entanglement can improve the sensitivity of collective spin measurements (Ref. [28] in the manuscript) and even in a cavity setting where entanglement enhanced atom interferometry has been demonstrated (Ref. [33] in the manuscript). It seems to me that the novelty here is that the authors now extended this to four modes, but I have trouble formulating a clear fundamental novelty in this.
I also fail to see how on the technical side, this work represents a major breakthrough given that the techniques used to generate entanglement i.e., the collective QND measurements and the velocity-dependent stimulated Raman transitions seem to have already been demonstrated in previous work (Refs. [28,32-34] in the manuscript).
In terms of significance, one issue that I see is also that the demonstration by the authors is for a system where the spatial separation between the atomic ensembles is very small (10's of micrometers) where all ensembles are within the same cavity system. In the conclusion, the authors point to interesting applications that all require an extended network of ensembles where mode entanglement can be created between separate cavities. However, I believe that the current experiment is very far from realizing such a thing and I fail to see how the results presented here provides a significant step towards these future applications.

Data, methodology and suggested improvements
I find the approach, validity of the results and the quality of the data satisfactory. I do, however, believe that the presentation can be significantly improved to allow for the non-expert reader to

understand the results. I would recommend the authors to consider the following options.
1) Provide a high-level schematic of their setup such that the reader immediately understands what the physical system is.
2) Better explain the velocity-dependent stimulated Raman transitions e.g., how is the effective wavevector associated with the Raman transition defined (or point to a reference). Also be more specific regarding the statement that the pi pulse is short enough to address nearly the entire velocity distribution of the atom source i.e., what does this actually mean.
3) Add the specific formula for calculating the contrast C in the methods and add more details as to why the exponential decay the observe with constant 0.46 micro sec falls within what is expected.
4) Add more detail as to how the QND detection creates entanglement between the atomic ensembles.
5) Substantiate the claim on page 7 that "the observed network gain is consistent with the measured atom number dependence of squeezing efficacy observed in Ref. [34] for this system.
6) More clearly state what the authors mean when they say they prepare ensembles of e.g., 45.000 atoms per mode. What are the fluctuations here and how do they determine them?

Referee #2 (Remarks to the Author):

Dear Editor,
I have read manuscript "Distributed quantum sensing with a mode-entangled network of spin-squeezed atomic states" by Benjamin Malia et al.

The authors report on the generation of nonlocal atomic ensembles by Raman beam splitter pulses. The atomic ensembles are entangled by performing global QND measurements. They demonstrate three different measurement schemes, where they measure (i) an artificial differential phase between two subensembles, (ii) a squeezing-enhanced resolution of a differential clock measurement for up to four modes and (iii) an acceleration difference measurement between two modes.

The reported results on multi-mode entanglement-enhanced interferometry present a major achievement, which warrant publication in Nature. They are timely, as shown by the recent complementary work from the Thompson group (arXiv:2110.14027). The data and methodology is of highest quality. The manuscript is very well written and a good to read.

However, I have a few comments and questions:
- The manuscript presents the different measurement schemes in series but it would be helpful to guide the reader why which one is chosen. Scheme (i) measures an artificial, finite differential phase but the squeezing enhancement is not evaluated. Scheme (ii) evaluates the squeezing enhancement of a differential clock measurement, but without the artificial phase. What hinders the application of a phase shift in the clock case? How good is the squeezing that is achieved in the clock-insensitive

case? What are the limitations to combine the two?
- Scheme (iii) is not below the QPN limit. Is it realistic that the Raman phase noise is not suppressed although the spatial difference is on the micrometer scale?
- Page 5, last paragraph: \bar{theta}=\delta\theta^(m) is not very self-explanatory and in contradiction to eq. 1
- Page 6, last paragraph. What is the influence of the magnetic field gradient on the contrast?
- Fig.3: It would be helpful to indicate the 4.5 dB improvement.
- Regarding the multi-mode enhancement: There is a conceptual difference between nonlocal entanglement and “just” improved local squeezing on the modes. Is there a possibility to discriminate between these?

Referee #3 (Remarks to the Author):

Referee Report of the manuscript
“Distributed quantum sensing with a mode-entangled network of spin-squeezed atomic states” by Benjamin K. Malia et al.

Distributed quantum sensing (DQS) is an emerging concept that deeply impacts the field of metrology and precision measurements. The central idea is to spread a mode-entangled (ME) state among a network of spatially localized sensors. This protocol allows parameters estimation sensitivities that are significantly higher than those achievable with an ensemble of uncorrelated sensors.
The physical reason for the advantage of DQS is the possibility of exploiting the correlations among the measurement results of the different sensors created by the ME state. In an optimized network configuration, these correlations allow a parameter estimation sensitivity scaling as the number of sensors. Conversely, if the quantum state feeding the network is separable with respect to the different sensor modes, the sensitivity can only scale with the square root of the number of sensors. Different DQS experimental protocols have been experimentally realized using squeezed light, photonics GHZ states and single-photon Fock states. However, the demonstration of DQS with a network of Mach-Zehnder (MZ) interferometers realized with cold atoms is still lacking in the literature. Atomic DQS will play a central role in applications ranging from 3D imaging reconstructions and magnetic force gradients measurements to distant clock synchronizations. The results presented here are, therefore, timely and will quite impact the field of quantum sensing and metrology, but will also be of interest to the broader quantum technology community because of the demonstration of precision tailoring and control of quantum correlations in cold atomic systems.
In the work presented here, a non-demolition measurement of a multimode input state creates quantum correlations distributed as input states of two and four atomic MZ interferometers. The authors demonstrate experimentally that the shared correlations provide a crucial advantage in phase estimation sensitivity with respect to the measurement performed with independent sensors. As an application, they consider a network configuration where it is possible to extract a phase from correlated noisy signals.
The results presented in this manuscript are compelling and deserve to be published. However, there are some comments and criticisms the authors should address before a final acceptance.

i) In the abstract, the authors write: “we demonstrate that non-local entanglement between network nodes...” The term “non-local entanglement” is also used in the main body of the text. The problem is that in the quantum science and technology literature, “non-local entanglement” has assumed the precise meaning of Bell correlations. It is clear that the entanglement produced in this experiment is spatially distributed but there is no evidence, nor necessity, to claim that non-local correlations that can violate a Bell inequality are present in the data (a proof of nonlocal correlations would perhaps be possible with a similar setup, but this would be a different work, see the note at the end of ii). I invite the authors to rephrase “local/non-local entanglement” with, for instance, “spatially distributed entanglement” to avoid any possible misunderstanding. As a general comment, notice that Bell nonlocality encloses a class of quantum correlated states that only partially overlaps with the class of entangled states that are metrologically useful.

ii) In the caption of Fig.1, section “Two-mode preparation” (and elsewhere in the main body of the text), the sentence “...the modes are separable here...” is not correct. The Raman \pi – pulse by itself already creates entanglement among the spatial and internal modes. Only after tracing out the internal(spatial) degrees of freedom the reduced state becomes separable in the spatial(internal) modes. In contrast, it is important to notice that the Raman \pi pulse does not create -particle entanglement-. Therefore, the quantum non-demolition measurement performed after the Raman pulse creates particle entanglement and affects the mode entanglement already present before QND. It is the combined action of particle entanglement and mode entanglement that allows sensitivities scaling with the number of interferometers.

[PS. A note regarding points i) and ii): There is actually the possibility to investigate Hyper- and hybrid Bell nonlocality with states manipulated with Raman pulses https://doi.org/10.1103/PhysRevLett.120.050404 . To be sure: I mention this point only to clarify my previous comments further. Bell non-locality is outside the goals of the present manuscript, and there is no need to cite/discuss it in this manuscript.

iii) The manuscript would gain in readability if it specified the observables squeezed by the QND right when QND is first introduced in the text, one sentence would be enough. In the present version, this is done quite later when discussing Eq.(2).

iv) In the caption of Fig.1 (second and eighth line) and in the main body of the text (for instance, in the y axis of Fig. 4,c), the term “J_z = 0” should be replaced with the average “<J_z> = 0”.

v) Please clarify in the caption of Fig. 1 what represents the black wave lines connecting the blue and red paths of the figure.

vi) In fig.2, enlarged region, the 68% confidence interval seems equal for the green and purple data, while I would have expected a smaller uncertainty in the green ME case. Is this just a visual effect, or I miss-understand the figure?

vii) The demonstration that the clock network sensitivity scales as the number of clocks in Fig.3 is quite convincing. The authors might comment that such scaling is far from obvious in a general network configuration in presence of distributed entanglement, since it depends on carefully counting the resources and the specific combinations of estimated phases. This point is discussed for a network of MZ interferometers in https://arxiv.org/abs/2109.09178.

viii) The discussion about differential phase-shift detection is very interesting and might be elaborated further. A problem that should be addressed is the following: when using squeezed states, it is possible to cancel a common noise only within a finite range of phase noise values. This is the consequence of the bending along the Bloch sphere of the squeezed state (as represented in the Wigner distribution). Phase noise cancellation is effective in the current experiment, where the noise range is relatively small. Can the authors show experimentally up to what range of phase noise the approach followed here can demonstrate sub shot-noise sensitivities? If this requires collecting new data and/or is too time/energy consuming, it would be enough to include a sentence in the text to comment about this problem.

Author Rebuttals to Initial Comments:

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Responses to Nature queries and reviewer comments for "Distributed quantum sensing with mode-entangled spin-squeezed atomic states" are detailed below.

RESPONSE TO REFEREE COMMENTS:

Referee #1:

1) Provide a high-level schematic of their setup such that the reader immediately understands what the physical system is.

We have added a high-level schematic in the extended data.

2) Better explain the velocity-dependent stimulated Raman transitions e.g., how is the effective wavevector associated with the Raman transition defined (or point to a reference). Also be more specific regarding the statement that the pi pulse is short enough to address nearly the entire velocity distribution of the atom source i.e., what does this actually mean.

We have added the phrases "k is the difference between the wavevector of each of the counterpropagating beams" and "the linewidth of the Raman transition is larger than the Doppler width" to the main text on page 3.

3) Add the specific formula for calculating the contrast C in the methods and add more details as to why the exponential decay the observe with constant 0.46 micro sec falls within what is expected.

For the formula for contrast, we added the sentence "Specifically, this is twice the amplitude of a sinusoidal fit to the data (as seen in Fig. 4c for the interferometer contrast)." Details for how to estimate the decay time can be found in Parazzoli et al. (Ref 46). We have added this reference.

4) Add more detail as to how the QND detection creates entanglement between the atomic ensembles.

Details on this process can be found in the reference Polzik and Ye (Ref [22]). We add this citation to the end of the sentence "Finally, a (now spatially distributed) QND measurement is performed to projectively squeeze the distributed states…." on page 5.

5) Substantiate the claim on page 7 that "the observed network gain is consistent with the measured atom number dependence of squeezing efficacy observed in Ref. [34] for this system.

We now state the previous result of Ref. [34] on page 7: "(approximately 14 dB for N = 100,000 atoms)."

6) More clearly state what the authors mean when they say they prepare ensembles of e.g., 45.000 atoms per mode. What are the fluctuations here and how do they determine them?

We added the parenthetical remark "the atom number differs between the modes by about 1% in the third case due to imperfections in the separating π pulse, as measured by the QND pulse" on page 5.

Referee #2:

1) The manuscript presents the different measurement schemes in series but it would be helpful to guide the reader why which one is chosen. Scheme (i) measures an artificial, finite differential phase but the squeezing enhancement is not evaluated. Scheme (ii) evaluates the squeezing enhancement of a differential clock measurement, but without the artificial phase. What hinders the application of a phase shift in the clock case? How good is the squeezing that is achieved in the clock-insensitive case? What are the limitations to combine the two?

There are no limitations to combining the two; we demonstrated them as separate experiments to systematically evaluate the sensor's response. To clarify this point, we added the parenthetical remark "(with a maximum variance of 9.5(0.5) dB below the QPN limit)" to the discussion on page 5 to emphasize that this data was acquired with squeezing. We also added the statement "(for simplicity, no magnetic field gradients or artificial phase shifts are present, although they could be added in principle)" on page 7.

2) Scheme (iii) is not below the QPN limit. Is it realistic that the Raman phase noise is not suppressed although the spatial difference is on the micrometer scale?

The 0.1 dB above the QPN refers to the average value of multiple data sets. Some of the individual sets have variance below the QPN. We changed the wording on page 9 to say "the average absolute noise is…." Since we did not extensively study the source(s) of this technical noise, we have rephrased that sentence to say that "…we speculate…."

3) Page 5, last paragraph: \bar{theta}=\delta\theta^(m) is not very self-explanatory and in contradiction to eq. 1

We clarified this equation. Since M=m=1, bar{theta}=delta\theta^(m)/M = delta\theta^(1). We changed the text to read "The two single-mode cases, bar{theta}= delta\theta^(1), experience…"on page 6.

4) Page 6, last paragraph. What is the influence of the magnetic field gradient on the contrast?

We now indicate in the methods that “Adding the magnetic field did not change the contrast by more than the margin of error (1%).”

5) Fig.3: It would be helpful to indicate the 4.5 dB improvement.

We added an arrow to the figure to indicate the 4.5 dB improvement.

6) Regarding the multi-mode enhancement: There is a conceptual difference between nonlocal entanglement and “just” improved local squeezing on the modes. Is there a possibility to discriminate between these?

This points to exciting next experiments. We now indicate this in the conclusion: “In the future, a distributed array of cavities sharing a common QND measurement… would enable entanglement and Bell tests across long distances.”

Referee #3:

1) In the abstract, the authors write: “we demonstrate that non-local entanglement between network nodes...” The term “non-local entanglement” is also used in the main body of the text. The problem is that in the quantum science and technology literature, “non-local entanglement” has assumed the precise meaning of Bell correlations. It is clear that the entanglement produced in this experiment is spatially distributed but there is no evidence, nor necessity, to claim that non-local correlations that can violate a Bell inequality are present in the data (a proof of nonlocal correlations would perhaps be possible with a similar setup, but this would be a different work, see the note at the end of 2). I invite the authors to rephrase “local/non-local entanglement” with, for instance, “spatially distributed entanglement” to avoid any possible misunderstanding. As a general comment, notice that Bell nonlocality encloses a class of quantum correlated states that only partially overlaps with the class of entangled states that are metrologically useful.

As detailed above, we now indicate that Bell tests would be exciting future work. At the referee’s suggestion, we replaced “non-local” with “spatially distributed” and “local” with “spatially localized” throughout the text.

2) In the caption of Fig.1, section “Two-mode preparation” (and elsewhere in the main body of the text), the sentence “...the modes are separable here...” is not correct. The Raman \pi – pulse by itself already creates entanglement among the spatial and internal modes. Only after tracing out the internal(spatial) degrees of freedom the reduced state becomes separable in the spatial(internal) modes. In contrast, it is important to notice that the Raman \pi pulse does not create -particle entanglement-. Therefore, the quantum non-demolition measurement performed after the Raman pulse creates particle entanglement and affects the mode

entanglement already present before QND. It is the combined action of particle entanglement and mode entanglement that allows sensitivities scaling with the number of interferometers.

We replaced "mode" with "spatial mode" here and throughout the manuscript (where appropriate) to avoid this ambiguity. We also indicated when a "correlation" means a "spin correlation" and when "mode-entanglement" means "spin-squeezing."

3) The manuscript would gain in readability if it specified the observables squeezed by the QND right when QND is first introduced in the text, one sentence would be enough. In the present version, this is done quite later when discussing Eq.(2).

We rephrased the introduction to state : "Velocity-dependent Raman transitions create up to four spatial modes … QND measurement is performed to entangle the modes' spins." and "This entanglement enhances the precision of the frequency comparison within networks of identical clocks, each containing 45,000 atoms per mode." We also indicate on page 5, before the introduction of equation (1), that \delta J_z is the squeezed quantity.

4) In the caption of Fig.1 (second and eighth line) and in the main body of the text (for instance, in the y axis of Fig. 4,c), the term "J_z = 0" should be replaced with the average "<J_z> = 0".

Jz = 0 has been changed to <Jz> = 0 throughout the manuscript.

5) Please clarify in the caption of Fig. 1 what represents the black wave lines connecting the blue and red paths of the figure.

We have replaced black waves with purple shadows to indicate distributed entanglement between modes. We now explicitly state this in the caption of Fig. 1.

6) In fig.2, enlarged region, the 68% confidence interval seems equal for the green and purple data, while I would have expected a smaller uncertainty in the green ME case. Is this just a visual effect, or I miss-understand the figure?

The difference in the number of atoms N between the purple and green data make the comparison of errors more difficult since they have different QPN limits. For clarify, we have reiterated the N values in Fig. 2 caption.

7) The demonstration that the clock network sensitivity scales as the number of clocks in Fig.3 is quite convincing. The authors might comment that such scaling is far from obvious in a general network configuration in presence of distributed entanglement, since it depends on carefully counting the resources and the specific combinations of estimated phases. This point is discussed for a network of MZ interferometers in https://arxiv.org/abs/2109.09178.

We now cite this work and have added the sentence "In an arbitrary network, the scaling of \xi_\text{net}^2 with M and N depends on the distribution of resources and the linear combination of variables used." to the methods section on multiparameter estimation.

8) The discussion about differential phase-shift detection is very interesting and might be elaborated further. A problem that should be addressed is the following: when using squeezed states, it is possible to cancel a common noise only within a finite range of phase noise values.

This is the consequence of the bending along the Bloch sphere of the squeezed state (as represented in the Wigner distribution). Phase noise cancellation is effective in the current experiment, where the noise range is relatively small. Can the authors show experimentally up to what range of phase noise the approach followed here can demonstrate sub shot-noise sensitivities? If this requires collecting new data and/or is too time/energy consuming, it would be enough to include a sentence in the text to comment about this problem.

We have added the parenthetical remark “(we can explore larger shift angles in future work)” on page 6.