nature portfolio

Peer Review File

Mach-Zehnder atom interferometry with non-interacting trapped Bose-Einstein condensates

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The manuscript „Mach-Zehnder atom interferometry with non-interacting trapped Bose Einstein Condensates“ describes an experimental sequence that indeed implements a Mach-Zehnder-like sequence for space-wise separated, trapped matter wave packets of K-39.

The outstanding achievement is the ability to adjust the atomic interactions via a Feshbach resonance throughout this sequence, in this case tuning them close to zero, realizing an ideal gas-like situation.

This combination and level of experimental control has been a long-standing goal for over a decade and the work clearly merits publication in Nature Communications. This being said, there are several issues that prevent publication in the current form and need to be addressed.

The work presented here is embedded in a global effort that started over 20 years ago, and many beautiful results have been obtained by authors, as well as the groups of Ketterle, Reichel, Schmiedmayer, Steinhauer, Oberthaler, Klempt, Treutlein, just to name a few. Some of the claims of novelty and "for the first time" should be checked carefully against this history, more specifics will be given below. Explicitly, the ability to perform x-axis rotation operations on the Bloch sphere is a necessary and demonstrated operation, the claimed uniqueness should be revised.

We identify two main points of general criticism:
• The role of interactions is somewhat mystified and the used terminology is ambiguous. The terms "decoherence" and "dephasing" are used (synonymously?) to describe the "harmful" effects of interactions. The authors should differentiate, especially in view of the fact that an echo sequence is applied. In the abstract as well in the main text, interactions are described as "strong" without any reference to other scales, like tunnel couplings or potential barrier heights or temperature. One of the groundbreaking works, highlighting the interplay of interactions and coherence time is Ketterle/Pritchard PRL 98, 030407 from 2007 (demonstrating 200 ms in an interacting system). This should be cited and the 200 ms be set as a benchmark, questioning the claim of an improvement by two orders of magnitudes.
From an experimental perspective, the tuning of interactions is described poorly and indirectly via the applied magnetic fields. Only towards the end of the 3rd page, the reader learns about the used field values. The link between field and scattering properties is fully missing. A plot of \Xi(B) = \Xi_el(B) + \Xi_dd over the relevant range should be provided in the Methods section. Plotting Fig. 4 in terms of scattering parameter instead of the rather technical magnetic field should be considered. We propose to cite New Journal of Physics 9, 223 (2007) D'Errico et al. additional to [41].

2. The "linear beam splitter" is described actively avoiding the established terminology of the bosonic Josephson junction (here presumably in the Rabi regime), which actually has been established by some of the authors. It is difficult to understand the sequence used to produce Fig.2, especially panel c). Which time(s) shown in panel a) are changed to vary the time depicted in panel c)? The fluctuations of the initial population imbalance are huge. It would have been instructive to change the atomic interaction parameter in this sequence to separate technical from quantum effects. This is relevant, as ultimately, the echo-sequence is used to predominantly (?) reverse technical fluctuations, not a potential phase diffusion.

Additionally, we would like to mention:

• We don't understand what is meant by „provides Heisenberg-limited spatial resolution", please explain and provide a reference.
• Why is a non-interacting gas „extremely sensitive to imperfections"? This probably connects to the non-mentioning of the relevant energy scales (tunnel couplings or potential barrier heights, temperature…) mentioned above
• The left panel of Figure 1 is rather confusing. The vertical axis depicts time in the sense of a time-sequence, but also the "integrated profiles of the BEC density" and "trapping potential". In at least two other publications, authors have given a more detailed description of an effective potential provided by a Beat-Note superlattice (https://doi.org/10.48550/arXiv.2504.12831 and 10.1103/PhysRevLett.127.020601). Especially in the PRL by Masi et al. the effective potential and the atomic density are calculated depending on the effect the fast optical modulation (depth) has on the shape of the BEC's wavefunction and the tunnelling between the adjacent sites of the BNSL. Given that tunnelling plays an important role in the beam splitter and recombiner sequence, these details are of great interest to the reader. The depicted wavefunction in blue suggests that the modulation on top of the smooth effective potential is rather strong – is that correct? Ideally, the authors would provide a calculation of the time dependent optical potential through the splitting process, probably in the supplementary information.

• The description „An external homogeneous force provides a linear potential energy that affects each interferometer with an equal energy mismatch ε between the two wells.„ Is very cryptic, reader needs to go to supplementary information to find out that there is an additional off-center beam applied.
• The Fig. 4 caption refers to the methods section [43] to explain, but the histogram announced in the caption of figure 4b is actually missing – there is just one panel
• The inset of Fig. 4 is not clear: is this a simulated (artificially broadened) wave function, or has any tomography been performed?
• The title of this figure is "Interaction-induced decoherence" which sounds as if some fundamental effect is studied. However, the interactions merely turn the predominantly technical fluctuations into phase diffusion – which can be (partially) reversed using echo sequences? Should this really be called decoherence?
• The sequences for the "splitter" and the "recombiner" seem to be different. What is the reason?

Reviewer #2

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #3

(Remarks to the Author)
In this manuscript the authors report on the realization of several Mach-Zehnder atom interferometers based on Bose-Einstein condensates of 39K confined in an array of optical double-well potentials. The potentials are formed by a beat-note superlattice generated with three laser frequencies, which allow for tuning the barrier height between two individual wells. Additionally, interparticle interactions are suppressed by tuning the interaction strength with a broad magnetic Feshbach resonance, allowing the realization of a linear matter-wave beam splitter based on coherent tunneling between adjacent wells. Ultimately, the operation of multiple interferometers enables differential phase measurements and the demonstration of a trapped atom-gradiometer for a harmonic potential, created with an additional vertical laser beam. Furthermore, a spin-echo protocol is employed to mitigate technical dephasing and extend the coherence time of the interferometers to nearly one second. Phase shift and noise are analyzed using a multiparameter maximum likehood method and the experimental results are supported by a theoretical model including interaction-induced decoherence, beamsplitter imperfections and technical dephasing. The supplemental material provides a detailed derivation of the used noise model, the double-well potentials and a technical explanation of the magnetic field calibration, all of which are relevant to supporting the main results.
The manuscript presents a compelling advance for quantum sensing with trapped atom interferometers. By combining coherent tunneling with interaction tuning and a spin-echo protocol the authors address a long-standing issue and achieve significant improvement in coherence time. The manuscript thereby presents a clear conceptual advancement and offers experimental validation of a previously unrealized interferometric architecture with potential applications in fundamental physics and applied sensing, which will be of interest to a broader community in those fields. The manuscript is globally well written, the methods are clearly described, and the results are supported by appropriate modeling and data analysis. However, I have the following remarks and questions, which I think can strengthen the manuscript:

The authors state a 30-fold improvement in coherence time compared to other realizations of trapped atom interferometers. I think a quantitative comparison to these experiments, e.g. as a table, would strengthen the significance of the achieved results while also highlighting potential differences between the experiments conducted.

The first beam splitter is performed by classically raising a potential barrier in the center of the Bose-Einstein condensate. How stable is this method over time? Are there significant shot-to-shot fluctuations in the splitting ratio or long term drifts which have to be taken into account?

Could the authors please comment on the scalability of their approach? While here two double-wells were used in the gradiometer, is it possible to operate a larger number of wells and thereby increase the spacing between interferometers? What are the potential limitations?

It is stated that the central double-wells are loaded with N = 3000 atoms for the interferometry scheme. Since interaction induced dephasing can effectively be controlled by magnetic Feshbach tuning, can the wells also be loaded with more atoms to enhance the sensitivity of the interferometer?

The authors convincingly demonstrate differential phase measurements between two interferometers, consistent with a gradiometric setup. However, a sensitivity figure is not provided. Including one would greatly strengthen the manuscript as it would elevate the scheme from a proof-of-principle experiment to a functional sensor and allows to contextualize the performance relative to other interferometric schemes.

It is stated that the interferometer operates in the absolute ground state |1,1>, which allows to presicly tune the scattering length around the Feshbach resonance at approximately 350 G. This implies a substantial sensitivity to magnetic fields. Since 39K features broad Feshbach resonance for all mF-states in F = 1, have the authors considered using a different mF-state with a reduced sensitivity to stray magnetic fields? Could environmental magnetic noise limit the application of the scheme to unknown gradients?

Minor spelling remarks:
Page 5: “In conclusion, we report the realization of Mach-Zehnder atom interferometry with trapped BEC, where the linear operations are enabled by the **the** precise canceling of the scattering length.“
Page 5 : “coopropagating“ -> co-propagating
Page 5: “wavelenghts” -> wavelengths

Reviewer #4

(Remarks to the Author)
The manuscript describes an experimental realization of an atom interferometer using a Rb BEC. The beamsplitter operations are achieved through coherent tunneling through a double-well potential formed by optical lattices. The fidelity of the beamsplitter is quite good, but not perfect (~70%).

The authors then describe a gradiometer that uses conjugate interferometers to suppress common mode noise and measure differences in phase accumulated between the interferometers. They use this interferometer to measure the harmonic potential of an additional laser beam. The results agree with expectation, with sensitivity at the part per ~100 level.

The authors then describe a spin-echo sequence, which is used to extend measurement time and possibly increase sensitivity.

Significance of scientific results.

Atom interferometry using tunneling through double-well potentials has been somewhat dormant for a while, so this is a nice update for these type of methods. The experimental scheme used is clever and gets around some of the limitations of the method. The data and analysis look convincing and consistent. Overall, this manuscript describes a significant step forward for this type of atom interferometers, although I have some major reservations with regards to the possibility of this method becoming competitive with other state-of-the-art light-pulse atom interferometry techniques.

1. The measurement shown here has precision at the part per 100 level. It is unclear how could this be improved by more than 9 orders of magnitude to reach part per 10^11 precision of the best atom gravimeters (see fine structure constant and equivalence principle measurements). I suggest the authors clarify the application(s) where this method is superior, otherwise there is strong contradiction with the last sentence in the abstract: “Our interferometer will find applications in precision measurements of forces with a high spatial resolution and in linear manipulation of quantum entangled states for sensing with sub shot-noise sensitivity.”

2. Expanding on question 1, there are significant unknowns in the paper: the main residual noise (“extra stochastic noise”) is not understood so path to improvements are unclear. Entanglement is mentioned as a possibility in the system, but entanglement only becomes useful as a resource one the experiment is at shot-noise, which is not the case here.

3. How well will the magnetic field offset have to be stabilized and gradients controlled to be able to achieve competitive precision?

4. For the spin-echo sequence, it is as if the two interferometer arms are switched. Why does this not cancel the measured gradient along with the noise? Related, I ssugest showing the measured phase for the spin-echo experiment not only noise, same as in Figure 3b. Please compare to the expected harmonic potential values.

In conclusion, I enjoyed reading the article and the experimental realizations, but I have some questions on the technical side and the presentation of this method as a precision sensing technique.

Specific comments and suggestions

The paper is quite well written in general, but there are a few points that could use some improvement.

“Moreover, the quantum-limited size of a BEC provides Heisenberg-limited spatial resolution for sensing applications.”
- It is unclear what “Heisenberg-limited” spatial resolution means. Could you give a number? Earlier sentences claim that a BEC has very narrow momentum spread, doesn’t that naively necessarily mean large position uncertainty per the uncertainty principle?

“Each MZI (see the scheme in Fig. 1) consists of a beam splitter, an interrogation time, and a final beam splitter that maps the accumulated relative phase into a population imbalance.”
- I would move this into a new paragraph. The way that paragraph is written is confusing, I suggest to finish the introduction and only then go into experiment description,

“The energy difference δ can be then estimated by a differential analysis, as illustrated below.”
– This is out of sequence, please move to where this is explained

“The second beam-splitter exploits the linear evolution of non-interacting atoms tunneling”
– it’s unclear to me what the authors mean by linear evolution?

“the interferometric phase acquired by the the j-th interferometer.” – interferometer arm

“As shown by the previous analysis, σtech is the major source of noise that limits the operation of our gradiometer close to Bmin, thanks to our high control of the scattering length.”
- Strange wording, perhaps: “Near Bmin, interactions are minimized and the primary source of noise is σtech.”
“by the the precise canceling of the scattering length.”

“Our work demonstrates a long-sought achievement in the ultracold atom community – pursued for over two decades.”
- This is a very subjective statement that I really doubt, I suggest removing it.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have carefully addresses all our queries as well as the ones of the other referees.
They have found a good balance between providing sufficient "depth" and maintaining accessibility to a broad readership.
I believe this reviewing iteration has improved the quality of the manuscript and now recommend it for publication without reservation.
Congratulations on the fine work!

Reviewer #2

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #3

(Remarks to the Author)
The authors have responded to all my comments and remarks satisfactorily. They have revised the manuscript where necessary and clarified ambiguous statements. Considering the comments of the other reviewers and the corresponding responses as well, I recommend publication of the revised manuscript.

Reviewer #4

(Remarks to the Author)
The authors accept some of the suggestions, particularly related to statements about putting the work into context and explaining the significance. However, there are a couple of points that I feel were not addressed sufficiently.

1. The authors explain nicely in the review the type of application of this interferometer: measuring potentials with um scale resolution. This, however, still doesn't come through in the paper. The words "high spatial resolution" appear in the abstract and introductory paragraph. Could a scale be given next to that? In addition, can this be compared with similarly tuned mechanical systems, like membranes?

2. The authors also explain some of the limitations of noise and drift in page 13 of the review. Why is this only made available to the authors and not included in the supplementary materials of the manuscript? Figure 1 seems important to

add...

3. The spin-echo phase is still not included in a plot, I would still be very interested to see it, not only noise.

Otherwise, the paper is a nice result and can be published.

Reviewer 1

The manuscript "Mach-Zehnder atom interferometry with non-interacting trapped Bose Einstein Condensates" describes an experimental sequence that indeed implements a Mach-Zehnder-like sequence for space-wise separated, trapped matter wave packets of K-39. The outstanding achievement is the ability to adjust the atomic interactions via a Feshbach resonance throughout this sequence, in this case tuning them close to zero, realizing an ideal gas-like situation. This combination and level of experimental control has been a long-standing goal for over a decade and the work clearly merits publication in Nature Communications. This being said, there are several issues that prevent publication in the current form and need to be addressed.

We sincerely thank the Referee for recognizing the significance of our results, which they describe as outstanding, and for highlighting how the level of experimental control we have achieved has been the subject of research efforts for well over a decade. We are grateful for the Referee's assessment that our manuscript "clearly" deserves publication in Nature Communications. Below, we address each comment raised by the Referee.

The work presented here is embedded in a global effort that started over 20 years ago, and many beautiful results have been obtained by authors, as well as the groups of Ketterle, Reichel, Schmiedmayer, Steinhauer, Oberthaler, Klempt, Treutlein, just to name a few. Some of the claims of novelty and "for the first time" should be checked carefully against this history, more specifics will be given below. Explicitly, the ability to perform x-axis rotation operations on the Bloch sphere is a necessary and demonstrated operation, the claimed uniqueness should be revised.

We thank the Referee for this comment. We have revised the manuscript, including some important missing references, and phrasing more precisely our claims of novelty.

Among the various types of interferometers based on trapped condensates, our work contributes to advancing the double-well scheme, in which atoms are confined in two spatial modes coupled via tunneling through the separating barrier. This configuration has long faced the challenge of having the interaction energy NU much smaller to the tunneling energy J [see Spagnolli et al. PRL 118 230403 (2017) for definitions of the single-particle interaction energy U and J], where N is the number of atoms. As a result, although coherent Josephson dynamics (i.e., rotations around the x-axis of the Bloch sphere) have been demonstrated, a precise mapping of the relative phase onto a measurable observable has typically required switching off the double-well potential and analyzing the interference pattern of the overlapping atomic clouds during time-of-flight expansion. This scheme has been demonstrated in several works, e.g. by Ketterle, Hinds, and Oberthaler. An alternative approach is through in-trap recombination of the two BECs [G. B. Jo et al., PRL 98, 180401 (2007)], enabling phase-sensitive generation of collective excitations. However, both methods do not rely on transferring the relative phase information to the population imbalance between the two modes, as occurs at the output of a Mach–Zehnder interferometer.

The only exception is the work by Schmiedmayer and collaborators [Berrada et al., Nat. Commun., 4, 2077, (2013)], which is properly cited in our manuscript. Our work belongs to this line of research, and to our knowledge, it is the first to achieve tunability of NU well below J, thus enabling fully linear operations—specifically, rotations of the Bloch vector of the N-particle system around the x-axis of the Bloch sphere, in an interferometric sequence.

The Referee mentions other scientists who have contributed to different types of interferometric schemes, which, however, differ from the interferometer discussed above. These include guided BECs coupled via Bragg or Raman transitions, and trapped BECs coupled by radiofrequency (rf) fields. In such systems, the strong Rabi coupling typically exceeds the interaction energy NU by a large margin, allowing for coherent rotations around the x-axis with high fidelity. We have added references to these valuable works and changed the abstract, the introduction and the conclusions to clarify our contribution in relation to the state of the art.

The role of interactions is somewhat mystified and the used terminology is ambiguous. The terms “decoherence” and “dephasing” are used (synonymously?) to describe the “harmful” effects of interactions. The authors should differentiate, especially in view of the fact that an echo sequence is applied.

We agree with the Referee that the two terms have been used synonymously. As suggested we have modified the main text using dephasing to describe the "harmful" effect of interactions and decoherence as any other source of uncontrolled broadening of the interferometric phase uncertainty.

In the abstract as well in the main text, interactions are described as “strong” without any reference to other scales, like tunnel couplings or potential barrier heights or temperature.

We agree with the Referee. In the previous version of the manuscript we have used the adjective "strong" in order to stress the following considerations. During the beam splitting operation, interaction energy has to be weaker than the tunneling energy to guarantee linear splitting. During the interrogation time, the dephasing due to interaction must be smaller than the energy resolution we want to achieve. In the new version of the manuscript we have removed any statement with generic "strong" interactions and we have clarified the energy scales.

Why is a non-interacting gas 'extremely sensitive to imperfections“? This probably connects to the non-mentioning of the relevant energy scales (tunnel couplings or potential barrier heights, temperature) mentioned above

One of the main limitations of trapped atom interferometry is the instability of the energies of the spatial modes. For instance, in a double-well potential created with optical tweezers, uncontrolled intensity fluctuations of the trapping laser can cause drifts in the energy offset ΔE between the two wells. As a result, the interferometric phase becomes affected by technical noise on the order of $T\Delta E/\hbar$, where T is the interrogation time.

Although interatomic collisions are typically a source of phase diffusion, they can also play a stabilizing role by compensating for slow drifts in ΔE that is, fluctuations that remain constant during a single measurement run (over T), but vary from shot to shot. This compensation arises because, during the initial splitting of the condensate, a greater number of atoms tend to occupy the lower-energy well, leading to an increase in interaction energy that reduces the effect of the energy mismatch ΔE. Another way to describe this compensation mechanisms is to follow the description of Ref. [46] where it is stressed that an interacting BEC is a superfluid that keeps the chemical potential spatially constant. For this reason, when the BEC is split, the two condensates in the two wells have almost the same chemical potential and the average phase difference evolution is not influenced by ΔE.

In our case, where we work with non-interacting BECs, this self-compensation mechanism is absent, and our system is therefore much more sensitive to fluctuations in ΔE. We have tried to clarify this point in the main text with a specif comment.

One of the groundbreaking works, highlighting the interplay of interactions and coherence time is Ketterle/Pritchard PRL 98, 030407 from 2007 (demonstrating 200 ms in an interacting system). This should be cited and the 200 ms be set as a benchmark, questioning the claim of an improvement by two orders of magnitudes.

We agree with the Referee that the work by G.-B. Jo et al. [Phys. Rev. Lett. 98, 030407 (2007)] demonstrates a significant coherence time of 200 ms, and we have now included this important reference in our manuscript. However, we would like to clarify the basis for our claim of a two-orders-of-magnitude improvement over previous results. As discussed in the previous answer, the stronger the interaction, the more efficiently the technical noise affecting the energy imbalance is compensated, effectively reducing the phase decoherence. However, with the interaction energy also being much larger than the tunneling energy it is not possible to realize linear beam splitting operations.

To give a quantitative comparison, in G.-B. Jo et al., the reported chemical potential reaches approximately 6 kHz. In this regime, the relative phase cannot be directly read out in-trap. Instead, the condensates must be released and allowed to expand at much lower densities, with the phase extracted from the resulting interference pattern. Contrarily, in Barreda et al., an in-trap recombination was performed to complete a Mach-Zehnder sequence, but this required a significant reduction of the atom number to bring the final chemical potential down to about 500 Hz, close to the tunneling energy.

For these reasons, we chose to compare our results to earlier experiments in which the entire Mach-Zehnder sequence was performed with atoms fully trapped, and where coherence times were on the order of 30 ms. Nonetheless, we agree with the Referee that such distinctions may be too specialized for a general readership, and that the work by G.-B. Jo et al. should indeed be considered a benchmark for double-well interferometry with trapped BECs. We have accordingly added the citation and revised the text to reflect this broader perspective.

From an experimental perspective, the tuning of interactions is described poorly and indirectly via the applied magnetic fields. Only towards the end of the 3rd page, the reader learns about the used field values. The link between field and scattering properties is fully missing. A plot of $\Xi(B) = \Xi_{el}(B) + \Xi_{dd}$ over the relevant range should be provided in the Methods section. Plotting Fig. 4 in terms of scattering parameter instead of the rather technical magnetic field should be considered. We propose to cite New Journal of Physics 9, 223 (2007) D'Errico et al. additional to [41].

We have added the suggested plot in the Supplementary Material where we report the dependence of the scattering length as a function of the applied magnetic field (see Fig. 3). Furthermore, we now mention the tunability of the interaction via a Feshbach resonance already at the beginning of the manuscript, adding the suggested reference.

In Fig. 4 we have preferred to report the uncertainty of the differential phase as a function of the magnetic field for the following reason. As explained to the Referee in one of the previous answers, the lowest decoherence rate might occur for slightly positive scattering lengths that could compensate for instabilities of the energy mismatches ΔE between the two wells during the first beam splitter. This could shift the minimum of the decoherence with respect to the exact zero crossing of the scattering length. Therefore we have preferred to leave the precise measurement of the dependence of the scattering length on the external magnetic field to future work. For example, by using a tunneling beam splitter also at the beginning of the interferometric sequence might avoid this interaction shielding effect working.

The "linear beam splitter" is described actively avoiding the established terminology of the bosonic Josephson junction (here presumably in the Rabi regime), which actually has been established by some of the authors

We agree that we are using a platform analogous to the one used in Spagnolli et al. [Phys. Rev. Lett. 118, 230403 (2017)], where Josephson physics has been explored across the zero crossing of the collisional scattering length for both positive and negative meanfiled energies. Unfortunately, during revision, we inadvertently removed a citation to this work, which is highly relevant to our study. We have intentionally avoided using the terminology of Josephson junctions, which is more appropriate for tunnel-coupled superfluids, since our non-interacting condensates do not exhibit superfluid behavior. Moreover, our beam-splitting sequence has distinct characteristics: the barrier height is rapidly lowered and then raised again, unlike in standard Josephson oscillations where atom transfer occurs at a fixed barrier height.

That said, we agree with the Referee that highlighting the connection with Josephson junction systems is valuable. We have therefore added a clarifying sentence, along with the suggested reference, in the section of the manuscript where the beam-splitting sequence is introduced.

It is difficult to understand the sequence used to produce Fig.2, especially panel c). Which time(s) shown in panel a) are changed to vary the time depicted in panel c)? The fluctuations of the initial population imbalance are huge.

The tunneling atomic beam splitter consists in lowering the central barrier and switching on the tunneling between the two, almost balanced, modes for a specific time such that the left (right) mode has a 50% probability to tunnel to the right (left) one (see Fig. 2a). From numerical simulations we know that the overall beam splitting time cannot be shorter than 20 ms if excitations to higher lying modes must be avoided. As a consequence, assuming linear ramps of the barrier height (are easier to be implemented), the only adjustable parameter is the value of the minimum barrier height V_{min} needed during the procedure. We experimentally determine such value with the following procedure. We start with a condensate fully loaded in the left mode, and by applying the aforementioned sequence, we vary V_{min} until the final distribution of the atomic imbalance is centered around zero (see Fig. 2b). To stress the connection between Fig. 1 and Fig. 2a) it should be clear now that the time 20 ms in Fig. 2a) corresponds to t_{BS_2} in Fig. 1.

Regarding Fig.2 c) the goal of this measurement is to show that the tunneling energies are the same for two neighbouring double-wells. That's why we extend the dynamics as much as possible up to 240 ms. In this measurement the condensate has been initially loaded in the left well by rising the central barrier height and by applying an energy mismatch between the two modes. To trigger the dynamics, at Time=0ms, we start a linear evolution towards zero of the energy mismatch between the minima of the two wells and of the barrier height towards 70% of its maximum value. The ramp lasts 10 ms and during this time the imbalance slightly reduces. For this reason our first measurement of the imbalance is recorded at Time=10 ms and such value set the amplitude of the oscillations.

Regarding the comment of the Referee on the "huge fluctuations of the initial population imbalance" we do not really understand what they are referring to. The uncertainty is the one we typically have in the measurement of the atomic imbalance. If they are mentioning the reduced value of ≈ 0.75, it should be clear from what explained above that this is due to the simultaneous ramping of the barrier height and energy mismatch. Finally we have preferred to look at the gradiometric measurement to estimate the visibility V_j of the interference (see Fig. 3).

These aspects are clarified when discussing Fig. 2 in the main text as well as in the Supplementary Material.

It would have been instructive to change the atomic interaction parameter in this sequence to separate technical from quantum effects. This is relevant, as ultimately, the echo-sequence is used to predominantly (?) reverse technical fluctuations, not a potential phase diffusion.

We don't fully understand this question of the Referee. The tunneling atomic beam splitter needs $J \gg NU$, so interactions are deleterious. When we optimize the beam splitting sequence we start with all the atoms in one of the two modes and even a small value of the scattering length causes macroscopic quantum self trapping [Spagnolli et al. Phys. Rev. Lett. 118, 230403 (2017)], i.e. the atoms don't even reach the $z = 0$ balanced condition.

If the Referee mentions the tuning of the scattering length during the interferometric sequence we remind that this is done later in the paper (see Fig. 4).

One possibility could be to exploit interaction during the first beam splitter. As already stressed this could help to suppress the technical noise. Of course it should be canceled during the second beam splitter. We have decided to leave this kind of investigation for future works because the optimal value of the scattering length would depend on the technical noise level of our apparatus and interaction phase diffusion. Then a precise modeling of the observed residual decoherence is not straightforward.

Additionally, we would like to mention: We don't understand what is meant by 'provides Heisenberg-limited spatial resolution', please explain and provide a reference.

The term Heisenberg limit may be misleading in this context and we have revised the text accordingly. Our intention was to highlight that a pure quantum state (namely a macroscopically-occupied ground state of the harmonic oscillator), is generally more localized than a thermal state, under the same external confinement. Since a pure state saturates the Heisenberg uncertainty relation, we loosely referred to this condition as the Heisenberg limit, intending it as a synonym for the system being in a pure state.

The left panel of Figure 1 is rather confusing. The vertical axis depicts time in the sense of a time-sequence, but also the "integrated profiles of the BEC density" and "trapping potential". In at least two other publications, authors have given a more detailed description of an effective potential provided by a Beat-Note superlattice (https://doi.org/10.48550/arXiv.2504.12831 and 10.1103/PhysRevLett.127.020601). Especially in the PRL by Masi et al. the effective potential and the atomic density are calculated depending on the effect the fast optical modulation (depth) has on the shape of the BEC's wavefunction and the tunnelling between the adjacent sites of the BNSL. Given that tunnelling plays an important role in the beam splitter and recombiner sequence, these details are of great interest to the reader. The depicted wavefunction in blue suggests that the modulation on top of the smooth effective potential is rather strong – is that correct? Ideally, the authors would provide a calculation of the time dependent optical potential through the splitting process, probably in the supplementary information.

We understand the difficulties. We have modified the Fig. 1. Now the arrow of time appears only linked to the manipulation of the lasers' intensities on the left. The space arrow is detached from the time arrow so to the "integrated profiles of the BEC density" and "trapping potential" appear disconnected from the time axis.

All details about the effective potential are reported in the supplementary material, including V_1, V_2, V_3, and V_{eff}. In order to clarify the differences between the real potential and the effective one, we have added, in the supplementary material, the tunneling energies and the atomic wavefunctions as a function of time. We confirm that the fast modulation of the wavefunction of the atoms in the two modes is rather strong. However the analogy between the two configurations is valid due to the similarity of the first two energy levels spacing.

The description „An external homogeneous force provides a linear potential energy that affects each interferometer with an equal energy mismatch ϵ between the two wells.„ Is very cryptic, reader needs to go to supplementary information to find out that there is an additional off-center beam applied.

We have clarified this in the main text adding "A spatial-dependent force realized in our experiment with an additional vertical dipole trap laser, ..."

The Fig. 4 caption refers to the methods section [43] to explain, but the histogram announced in the caption of figure 4b is actually missing – there is just one panel

We have revised Fig. 4(b) in the supplementary and we have added the histogram, which was mistakenly not included.

The inset of Fig. 4 is not clear: is this a simulated (artificially broadened) wave function, or has any tomography been performed?

It is an artificial plot used to illustrate the broadening.

The title of this figure is “Interaction-induced decoherence” which sounds as if some fundamental effect is studied. However, the interactions merely turn the predominantly technical fluctuations into phase diffusion – which can be (partially) reversed using echo sequences? Should this really be called decoherence?

We agree that our use of "decoherence" and "dephasing" was misleading (see discussion above). Accordingly, we have replace "decoherence" with "dephasing" in the title of this figure.

The sequences for the “splitter” and the “recombiner” seem to be different. What is the reason?

The Referee is correct: the two sequences are different. Contrary to the "recombiner" (called by us "Tunneling atom beam-splitter" and described in Fig. 2), the “splitter” at the beginning of the interferometer is implemented in a shorter time, thus minimizing the effect of imperfections. We are planning to realize a Mach-Zehnder interferometer with two "tunneling atom beam-splitters" but we leave this as future investigations.

Reviewer 2

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer 3

In this manuscript the authors report on the realization of several Mach-Zehnder atom interferometers based on Bose-Einstein condensates of 39K confined in an array of optical double-well potentials. The potentials are formed by a beat-note superlattice generated with three laser frequencies, which allow for tuning the barrier height between two individual wells. Additionally, interparticle interactions are suppressed by tuning the interaction strength with a broad magnetic Feshbach resonance, allowing the realization of a linear matter-wave beam splitter based on coherent tunneling between adjacent wells. Ultimately, the operation of multiple interferometers enables differential phase measurements and the demonstration of a trapped atom-gradiometer for a harmonic potential, created with an additional vertical laser beam. Furthermore, a spin-echo protocol is employed to mitigate technical dephasing and extend the coherence time of the interferometers to nearly one second. Phase shift and noise are analyzed using a multiparameter maximum likehood method and the experimental results are supported by a theoretical model including interaction-induced decoherence, beamsplitter imperfections and technical dephasing. The supplemental material provides a detailed derivation of the used noise model, the double-well potentials and a technical explanation of the magnetic field calibration, all of which are relevant to supporting the main results. The manuscript presents a compelling advance for quantum sensing with trapped atom interferometers. By combining coherent tunneling with interaction tuning and a spin-echo protocol the authors address a long-standing issue and achieve significant improvement in coherence time. The manuscript thereby presents a clear conceptual advancement and offers experimental validation of a previously unrealized interferometric architecture with potential applications in fundamental physics and applied sensing, which will be of interest to a broader community in those fields. The manuscript is globally well written, the methods are clearly described, and the results are supported by appropriate modeling and data analysis. However, I have the following remarks and questions, which I think can strengthen the manuscript:

We thank the Referees for carefully reading our manuscript and for the positive general assessment ("compelling advance for quantum sensing with trapped atom interferometers"). Please, find below an answer to all comments raised by the Referees.

The authors state a 30-fold improvement in coherence time compared to other realizations of trapped atom interferometers. I think a quantitative comparison to these experiments, e.g. as a table, would strengthen the significance of the achieved results while also highlighting potential differences between the experiments conducted.

As briefly reviewed in the introduction, there are different realizations of trapped interferometers with BEC. The reported 30-fold improvement refers to a comparison with the only experiment in the literature where the full Mach-Zehnder sequence was completed in a double-well interferometer using tunneling between trapped BECs [Berrada et al., Nat. Commun., 4, 2077, (2013)]. In that work, the condensates were coupled via tunneling through the separating barrier, and the interferometric phase was mapped onto the final population imbalance—all while the atoms remained trapped. Listing the various characteristics and differences between trapped interferometric platforms would be certainly useful but goes beyond the scope of our work as well as the journal's style.

The first beam splitter is performed by classically raising a potential barrier in the center of the Bose-Einstein condensate. How stable is this method over time? Are there significant shot-to-shot fluctuations in the splitting ratio or long term drifts which have to be taken into account?

The control and the characterization of the noise affecting the first beam splitter is a crucial point of our experiment. In the Supplementary Information Fig. 4 (a) we report the measurements of the atomic imbalance z after the first beam splitter over the time scale needed to collect one full ellipse, i.e. approximately 20 minutes. The standard deviation of z (see Fig. 4 b) in the Supplementary Material) is $\sigma_{BS} \sim 0.07$ and, as explained in the main text, it is crucial to determine the amount of uncorrelated phase noise of the gradiometer signal (using Eq. 2 in the main text). In addition we have discovered that larger amplitude drifts occur over longer timescales. For this reason at the beginning of every set of measurements, for each ellipse, we always performed a cycle of experiments to balance again the double-well. This has been done by finely tuning (few MHz) the frequency of the laser at 1120 nm. The origin of short term fluctuations and long term drifts are probably due to air turbulence and temperature, pressure and humidity changes that cause both fast common and slow relative phase shifts of the three lattices, propagating in air for 25 cm to reach the retroreflecting mirror. We have recently performed measurements of these phase shifts implementing an optical Michelson interferometer simultaneously for the three wavelengths. The results of the measurements and the simulated effect of these shifts on the atomic imbalance after the first beam splitter are a good indication of the validity of our assumptions/hypotheses. Further investigations are needed to estimate the effect of other possible noise sources, like magnetic field fluctuations. Vibrations of the retroreflecting mirror are currently negligible as estimated by simulations based on measurements with an accelerometer.

Could the authors please comment on the scalability of their approach? While here two double-wells were used in the gradiometer, is it possible to operate a larger number of wells and thereby increase the spacing between interferometers? What are the potential limitations?

The use of BNSLs allows to operate in principle several double-well interferometers simultaneously. The residual harmonic confinement along the lattice direction is of the order of few Hz, mainly caused by the finite Rayleigh range of the radial beam ($\sim 300\mu$m), collinear with the lattices and responsible of the confinement of the BECs along the directions orthogonal to the lattice beams. So far, a maximum of 8 double wells have been loaded symmetrically in our experiment. The limitation is set by the harmonic confinement (~ 1 Hz) that would prevent balanced tunneling splitting.

Regarding the possibility of increasing the separation between the modes, larger spacing BNSLs could be used, but this would increase the tunneling time. After the splitting we could try to tune the wavelengths of the lattices, but increasing the separation between the modes, for example from 5 microns to 50 microns, would require single mode operation of the lasers, while tuning their wavelengths over tens of nm. This is doable, but not easy.

It is stated that the central double-wells are loaded with N = 3000 atoms for the interferometry scheme. Since interaction induced dephasing can effectively be controlled by magnetic Feshbach tuning, can the wells also be loaded with more atoms to enhance the sensitivity of the interferometer?

A non-interacting condensate confined in a harmonic trap exhibits an in-trap Gaussian wavefunction, with a width determined by the harmonic oscillator length, independently from the atom number. For potassium atoms in our double-well potentials, this length is on the order of 1μm. Increasing the atom number would enhance the density and thus three-body recombination losses. To give an order-of-magnitude estimate, using the three-body loss coefficient $K_3 = 10^{-29}$ cm^{6}s^{-1} as reported in previous studies, we estimate that under our current experimental conditions, approximately 5% of the atoms are lost within 1 second. Since the three-body loss rate scales quadratically with the atom number, increasing N quickly leads to significant losses.

The authors convincingly demonstrate differential phase measurements between two interferometers, consistent with a gradiometric setup. However, a sensitivity figure is not provided. Including one would greatly strengthen the manuscript as it would elevate the scheme from a proof-of-principle experiment to a functional sensor and allows to contextualize the performance relative to other interferometric schemes.

Although we would like to emphasize the broader significance of our achievement—which sets a long-sought benchmark in the field—we fully agree that the potential applications of our sensor in real-world measurements are of equal importance.

The primary quantum sensing application of the gradiometer demonstrated in this work is the measurement of forces with high spatial resolution. The best way to evaluate the perspectives offered by our system is a direct comparison with the results recently reported in [67], that represents the state of the art in the high precision measurement of forces with high spatial resolution using ultracold atoms. In [67] the most precise measurement of Casimir-Polder forces between an atom and a surface was achieved using a Wannier-Stark atom interferometer operated with thermal atoms. That experiment reached a spatial resolution of 3 μm.

First of all the gradiometer demonstrated in our work could provide improved spatial resolution, set by the size of the BEC itself—potentially below 1 μm.

As reported in [67] the main limitation on the sensitivity of the sensor was the vibrations of the mirror used to realize the optical lattice potential. Using our gradiometric approach, the operation of two double well interferometers, one close and one far away from the mirror surface, could allow for common-mode rejection of trap-induced vibrations.

The second limitation of the work [67] is the quantum projection noise derived by the use of 500 atoms. Such low number is due to the need to reduce the overall atomic density and suppress the interaction induced dephasing, even if the cloud has a size of 3 microns along the lattice direction and 45 microns in the radial directions. Please note that our BEC with tunable interaction has 3000 atoms and a rms size of 1 microns in all the directions and still not affected by interaction induced dephasing. Not only our atomic probe size is smaller but also its shot noise. Finally, the use of a BEC rather than a thermal atomic ensemble opens the possibility for future quantum-enhanced sensing via entanglement, potentially surpassing the standard quantum limit. Such advantage could offer almost an order of magnitude increase in sensitivity in comparison to the aforementioned sensor implementing thermal atoms. Note that our system has the unique possibility to exploit interparticle interactions to create quantum entangled states and later to tune the collisional scattering length to zero to suppress dephasing.

Despite these promising perspectives, our current gradiometric measurements exhibit a large phase noise (see for example Fig. 5) of 0.2 rad already at 70 ms interrogation time. This corresponds to a current differential acceleration sensitivity of 10^{-4}g/shot, to be compared with the one reported in [67] of approximately 6×10^{-5}g/shot.

The origin of this large phase noise has not been identified yet, but it could be due to instabilities of our external potential that is not exactly harmonic and not stable in amplitude. A significant contribution is at very low frequency and we could measure it operating the gradiometer for almost two hours and recording the temporal evolution of the differential phase. Results are reported in

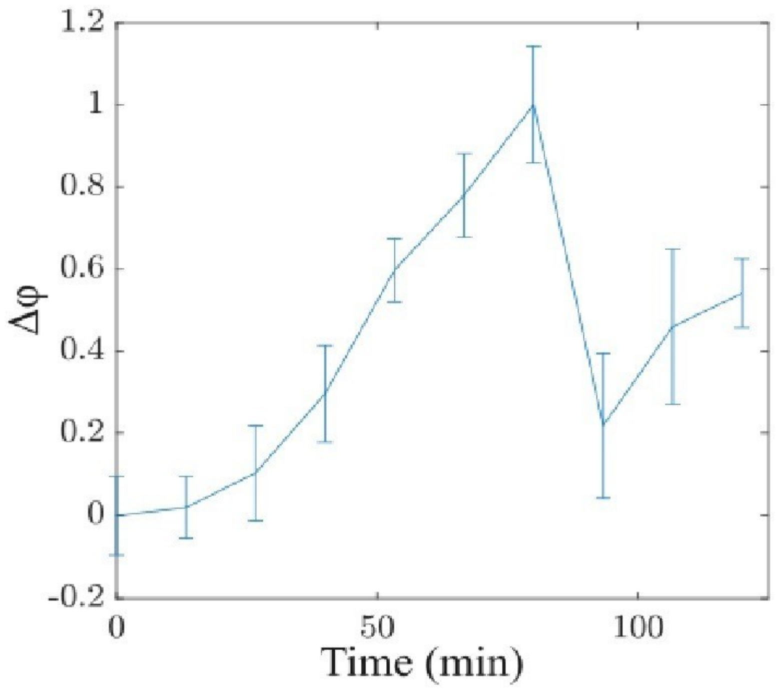

FIG. 1: **Stability measurement of the differential phase $\Delta\phi$.** Considering two neighboring double well interferometers operating in parallel and applying the vertical dipole trap, during the interferometric time T=70 ms, we monitor the evolution of $\Delta\phi$ for two hours leaving unchanged all the experimental parameters.

Fig. 1 of this reply.

Note that $\Delta\phi$, mod 2π, changes from 0.1 to 0.3 rad over the time interval needed to collect a full ellipse, i.e. approximately 15-20 minutes. Although we haven't been able to suppress this noise yet, the spin echo measurements allow to investigate what sensitivity could be achieved once suppressed this noise source. An rms noise of the differential phase of 0.6 rad after 800 ms interrogation time (see Fig. 5 in the main paper) would correspond to a sensitivity of 2×10^{-5}g/shot, already better than what has been reported in Ref. [67].

Besides these promises, we are aware that much has to be done before exploiting our sensor for high precision measurements, including the increase of the repetition rate of the measurements (important step forward has been recently reported in A. Herbst, et al. Phys. Rev. Research 6, 013139 (2024)) and control the effect of external magnetic fields (see the reply to the next question).

It is stated that the interferometer operates in the absolute ground state |1,1>, which allows to presicly tune the scattering length around the Feshbach resonance at approximately 350 G. This implies a substantial sensitivity to magnetic fields. Since 39K features broad Feshbach resonance for all mF-states in F = 1, have the authors considered using a different mF-state with a reduced sensitivity to stray magnetic fields? Could environmental magnetic noise limit the application of the scheme to unknown gradients?

We agree with the Referee that the accuracy of our gradiometer could be affected by spurious magnetic fields. Although the influence of a uniform magnetic field gradient could be canceled by a differential analysis—such as in high-spatial-resolution force measurements close to a dielectric surface, with one interferometer placed near the surface and one far away—schemes to eliminate magnetic forces are still needed.

First of all, the atoms themselves could be employed as sensitive probes of residual magnetic field gradients, enabling cancellation of this noise source.

A second possible route involves the use of radio-frequency (rf)–coupled mixtures, properly chosen to reduce the magnetic moment and the interaction energy of the mixture (see for example J. Sanz, PRL 128, 013201 (2022)).

Another interesting possibility is offered by the use of a different internal atomic state, such as the $F = 1, m_F = -1$ state, for which the magnetic field can be tuned to a so-called "magic" value of approx 100 Gauss, where the magnetic moment vanishes (see Fig. 5 of C. D'Errico et al. New J. Phys. 9, 223 (2007)). At this point, the residual scattering length is only 7 a_0, which is smaller than in rubidium and thus favorable for interferometry. Most importantly for a few hundred atoms it could be possible to explore the occurrence of the revival of the coherence (see M. Greiner, et al. Nature 419, 51 (2002)) after a complete dephasing of the interferometric phase. According to our simulations, we expect that after approximately one second of evolution, our system should have a window of several ms during which the interferometric phase can be measured with shot noise resolution. This measurement is in our priority list of future experiments, because if successful could allow to operate the interferometer with non magnetic BECs and not be limited by interaction induced dephasing.

It is not excluded that in the future BECs of fully non-magnetic atoms could be used in our experimental platform, like strontium BEC characterized by a zero magnetic moment and a rather small value of the scattering lengths, i.e. $\approx -2\ a_0$.

However, at the moment, our platform, where interactions can be easily tuned and where the effect of external magnetic fields can be canceled with a differential analysis, is by far the most versatile system where many proposals could be tested. Just to name a few we remember the realization of a gradiometer with sub shot noise sensitivity or interaction induced manipulation of quantum entanglement with both repulsive or attractive interactions.

Minor spelling remarks: Page 5: “In conclusion, we report the realization of Mach-Zehnder atom interferometry with trapped BEC, where the linear operations are enabled by the the precise canceling of the scattering length.“ Page 5 : “coopropagating“ -> co-propagating Page 5: “wavelenghts” -> wavelengths

Thanks for the remarks. We have corrected the text as suggested.

Reviewer 4

The manuscript describes an experimental realization of an atom interferometer using a Rb BEC. The beamsplitter operations are achieved through coherent tunneling through a double-well potential formed by optical lattices. The fidelity of the beamsplitter is quite good, but not perfect (70%).

We just remind the Referee that we are using Potassium atoms (with tunable interaction) rather than Rubidium 87 (with constant scattering length of $\sim$ 100 Bohr radii).

Figure 2c, where the Referee has extrapolated the visibility of the beam splitter, was not optimized to maximize the visibility and it is shown only to prove that neighboring double wells have the same tunneling frequency. The amplitude of oscillations is lower than the interferometric one (see below) because data in Figure 2c are collected starting from a condensate fully loaded in the left well and lowering the barrier in 10 ms, while the energy mismatch between the two wells is balanced. During this time interval a small fraction of atoms starts to tunnel to the right well reducing the initial atomic imbalance $z \sim 1$ to $z \sim 0.75$. This final value set the amplitude of the following Rabi oscillation. An estimation of the visibility of our interferometer can be extracted from the ellipse reported in Fig. 3(a), that is slightly below 90%.

The authors then describe a gradiometer that uses conjugate interferometers to suppress common mode noise and measure differences in phase accumulated between the interferometers. They use this interferometer to measure the harmonic potential of an additional laser beam. The results agree with expectation, with sensitivity at the part per 100 level.

The authors then describe a spin-echo sequence, which is used to extend measurement time and possibly increase sensitivity.

Significance of scientific results.

Atom interferometry using tunneling through double-well potentials has been somewhat dormant for a while, so this is a nice update for these type of methods. The experimental scheme used is clever and gets around some of the limitations of the method. The data and analysis look convincing and consistent. Overall, this manuscript describes a significant step forward for this type of atom interferometers, although I have some major reservations with regards to the possibility of this method becoming competitive with other state-of-the-art light-pulse atom interferometry techniques.

We thank the Referees for carefully reading our manuscript and for the positive general assessment ("a significant step forward for this type of atom interferometers"). Please, find below an answer to all comments raised by the Referees. We apologize if some of our replies refer to explanations already included in our responses to the other Referees.

1. The measurement shown here has precision at the part per 100 level. It is unclear how could this be improved by more than 9 orders of magnitude to reach part per 10^{11} precision of the best atom gravimeters (see fine structure constant and equivalence principle measurements). I suggest the authors clarify the application(s) where this method is superior, otherwise there is strong contradiction with the last sentence in the abstract: “Our interferometer will find applications in precision measurements of forces with a high spatial resolution and in linear manipulation of quantum entangled states for sensing with sub shot-noise sensitivity.”

The primary sensing application of our interferometer is the measurement of forces with high spatial resolution. For this kind of measurements, free falling atom interferometers are of no use considering that they measure the average force over large interrogation areas. For example 10^{-11}g/shot precision has been achieved in a 10 m drop tower set-up. On the contrary, trapped atomic samples, can resolve atom surface interactions with micrometric spatial resolution. We hope the answer to Referees 2 and 3 (page 13-14), where we describe the possible advantages offered by our system, is satisfactory also for Referee 4.

In addition we would like to comment on “the one part per 100 level precision” stressed by the Referee. After reading such comment we have investigated the origin of such uncertainty and we have found that our apparatus is currently affected by a drift of the differential phase (see above Fig. 1 of this report). Over a timescale of 15-20 minutes, that is the time needed to record a full ellipse, $\Delta\phi$ drifts an amount of 0.1-0.3 rad. This explains why in Fig.3 the single measurements of $\Delta\phi$ have uncertainties of 0.04-0.06 rad, but they are often well above or below the optimal linear fit by 0.1-0.3 rad, i.e. the estimated value of the drift during an ellipse measurement time. Considering that the value of $\Delta\phi$ for T $\approx$ 85 ms, is 20 rad it is expected that the overall relative uncertainty of the trapping frequency measurement is 0.1-0.3 rad/20 rad $\sim$ 1/100. We then realized that the choice of a vertical optical dipole trap to estimate the gradiometer sensitivity was not the optimal choice. Considering the finite waist of the beam, i.e. 50 μm, a shift of the beam with respect to the double well potential center of only 3 μm is responsible for a 1/100 relative change of the harmonic confinement. Such choice was driven by its simplicity and the focus of just demonstration the operations of the gradiometer, and not its ultimate performances. However we think that the spin echo measurement is a good estimation for the sensitivity of the gradiometer in case no drift of the external harmonic confinement occurs. We should already have a competitive sensitivity in comparison with the state of the art for the measurement of forces with micrometric spatial resolution [67] (see details in the reply to Referee 2 at page 13-14). We are planning future measurements with a weak harmonic magnetic potential that can be more stable and less sensitive to misalignment.

2. Expanding on question 1, there are significant unknowns in the paper: the main residual noise ("extra stochastic noise") is not understood so path to improvements are unclear. Entanglement is mentioned as a possibility in the system, but entanglement only becomes useful as a resource one the experiment is at shot-noise, which is not the case here.

As explained in the answer to question one, the first step will be to realize a more stable and uniform harmonic confinement. In this way we should be less sensitive to misalignment and temporal variations of the external harmonic confinement. An analysis extended to three or more double well interferometers could allow to take trace of common trapping frequency variations.

Regarding the comment on the entanglement we agree that we haven't achieved the shot noise limit yet. Hopefully, we will get closer to such limit once our gradiometer will be operated on a more stable and controlled external potential with a reduced number of atoms. Please note that measurements with high spatial resolution using ultra-cold atoms must implement small atomic samples with low atom number to prevent three-body losses. So entanglement is a useful resource when reducing the projection noise by increasing the atom number is not possible. Likely the tunability of interactions can in principle easily allow to create and later exploit entanglement in our gradiometer.

3. How well will the magnetic field offset have to be stabilized and gradients controlled to be

able to achieve competitive precision?

Regarding the problem of the sensitivity of our interferometer to external spurious magnetic field gradients we have already answered to Referee 2 at page 15. We hope the answer is satisfactory also for Referee 4. Regarding the level of control of the magnetic field needed to make negligile the interaction induced dephasing, we have to refer to the calculations reported in the Supplementary Informations. If we assume no technical noise during the first beam splitter, i.e. $\sigma_{BS} = 0$ from SI.14 we get a quantum uncertainty of the gradiometric differential phase $\sigma^2_{\Delta\phi} = \frac{2}{N} + N\tau^2$. If we want that the interaction induced dephasing term, i.e. Nτ^2 is smaller than the shot noise limit, i.e. $\frac{2}{N}$, we need $N^2\tau^2 < 2$. From Eq. (3) in the main text we have that $\tau = (0.072 a_s/a_0) Hz \cdot T$. With straightforward calculations we get that for an interferometric time T= 1 sec and N=3000 atoms, this condition is fulfilled if a_s is tuned to zero with a precision of 0.005 a_0. Thanks to the high degree of control of the scattering length on the zero crossing of the Feshbach resonance of 0.6 a_0/G, a stability of the magnetic field of 10 mG is needed. Such level is within reach of our current apparatus (see Fig. 3 of the Supplementary Materials), but this requirement could be also relaxed working with a smaller number of atoms. Please note that this level of phase noise, i.e. shot noise at 1 sec of interferometric time T, is much smaller than what reported in state of the art measurements of forces with micrometric spatial resolution [67] that have achieved maximum coherence time of 150 ms.

To conclude, the level of control of interactions for competitive measurements is not a major issue in our setup.

4. For the spin-echo sequence, it is as if the two interferometer arms are switched. Why does this not cancel the measured gradient along with the noise? Related, I ssugest showing the measured phase for the spin-echo experiment not only noise, same as in Figure 3b. Please compare to the expected harmonic potential values.

The Referee's intuition is correct. The spin-echo sequence cancels the external noise as well the signal from the external harmonic confinement. This is confirmed from the inset of Fig. 5 where a one to one correlation of the outputs of the two interferometers z1 vs z2 is reported. Considering that the differential phase is still sensitive to interaction-induced dephasing, the spin-echo protocol provides an estimate of the sensitivity of the gradiometer in absence of slow drifts of the external harmonic confinement.

In conclusion, I enjoyed reading the article and the experimental realizations, but I have some questions on the technical side and the presentation of this method as a precision sensing technique.

Specific comments and suggestions

The paper is quite well written in general, but there are a few points that could use some improvement.

We thank the Referee for the positive comments. We hope to have addressed all the questions they raised.

"Moreover, the quantum-limited size of a BEC provides Heisenberg-limited spatial resolution for sensing applications." - It is unclear what "Heisenberg-limited" spatial resolution means. Could you give a number? Earlier sentences claim that a BEC has very narrow momentum spread, doesn't that naively necessarily mean large position uncertainty per the uncertainty principle?

Considering the unclear definition of spatial resolution at the Heisenberg limit, which has also raised concerns from Referee 2, we have decided to modify the text accordingly. To clarify, the BEC has an rms width equal to the harmonic oscillator length for a quantum particle in a harmonic potential, i.e. $\approx 1\mu$m, provided a trapping harmonic confinament of ≈ 100 Hz. Instead, as correctly guessed by the Referee, in free falling atom interferometers, BECs with large sizes of the order of few tens of μm allow very small velocity spreads of 20-50 μm/s.

"Each MZI (see the scheme in Fig. 1) consists of a beam splitter, an interrogation time, and a final beam splitter that maps the accumulated relative phase into a population imbalance." - I would move this into a new paragraph. The way that paragraph is written is confusing, I suggest to finish the introduction and only then go into experiment description

The sentence pointed out by the Referee describes one of the main novelty of our work: i.e. that we are able to perform a full Mach-Zehnder interferometer sequence with a trapped BEC, where the phase information is mapped on the population signal. This sentence is not the beginning of the experiment description. We prefer to maintain the current sentence in the introduction.

"The energy difference δ can be then estimated by a differential analysis, as illustrated below." – This is out of sequence, please move to where this is explained

We agree with the Referee and we have removed the sentence as it was indeed out of sequence.

"The second beam-splitter exploits the linear evolution of non-interacting atoms tunneling"– it's unclear to me what the authors mean by linear evolution?

Formally speaking it means that the evolution of the system is described only by rotations of the Bloch vector. This is possible for a single spin or for a system composed by multiple non-interacting spins. In our case this occurs thanks to the cancellation of the collisional scattering length. We have clarified this in the introduction.

"the interferometric phase acquired by the the j-th interferometer." – interferometer arm

The index j refers to the jth interferometer, not to the arm. The interferometric phase is the relative phase acquired between the two arms of the interferometer.

"As shown by the previous analysis, tech is the major source of noise that limits the operation of our gradiometer close to Bmin, thanks to our high control of the scattering length."- Strange wording, perhaps: "Near Bmin, interactions are minimized and the primary source of noise is σ_{tech}." "by the the precise canceling of the scattering length."

Thanks. we have modified the text accordingly.

"Our work demonstrates a long-sought achievement in the ultracold atom community – pursued for over two decades." - This is a very subjective statement that I really doubt, I suggest removing it.

First of all we would like to stress that the other Referees seem to agree on this statement considering their comments. Referee 1 recognizes that "This combination and level of experimental control has been a long-standing goal for over a decade". The Referee 3 states that "By combining coherent tunneling with interaction tuning and a spin-echo protocol the authors address a long-standing issue and achieve significant improvement in coherence time." However we agree with

Referee 4 that not everyone would necessarily agree with this type of statement. So we have decided to revise it to make it more broadly acceptable, while preserving the significance of our results.

Reviewer 3

The authors explain nicely in the review the type of application of this interferometer: measuring potentials with um scale resolution. This, however, still doesn't come through in the paper. The words "high spatial resolution" appear in the abstract and introductory paragraph. Could a scale be given next to that? In addition, can this be compared with similarly tuned mechanical systems, like membranes?

We have specified in the abstract that high spatial resolution means sub-micron spatial resolution. We fully agree that membrane-based sensors provide outstanding spatial locality for short-range force measurements. Our trapped-atom sensor is not intended to compete on spatial resolution, but to offer a complementary measurement platform characterized by different systematic effects. Owing to the well-defined and highly controllable nature of atomic probes, this approach may enable more accurate modelling and a more controlled comparison between theory and experiment at micron-scale distances.

The authors also explain some of the limitations of noise and drift in page 13 of the review. Why is this only made available to the authors and not included in the supplementary materials of the manuscript? Figure 1 seems important to add....

We have revised the supplementary info by adding a section on the Gradiometer Sensitivity with the figure required by the Referee and the corresponding description.

The spin-echo phase is still not included in a plot, I would still be very interested to see it, not only noise.

We would like to clarify that after a spin-echo sequence the expected phase is zero. This is confirmed by the good correlation between the output atomic imbalances of the two interferometers shown in the inset of Fig. 5. We attribute the non perfect correlation to residual phase noise.

We have now explained in text that the spin-echo phase is assumed equal to zero in order to estimate the phase noise from the broadening of the correlated plot in the inset of Fig.5