Response to Referee reports to Manuscript RSI: MS #RSI23-AR-02290

Dear Editor,

first, we like to thank the reviewers for their careful reading of the manuscript, and their valuable feedback. Here is a point-by-point response to the reviewers' comments and concerns.

From reviewer #1:

Comment 1:
The cavity noise spectrum is a result of the environmental noise. The author should clarify the environment for the test reported in Fig. 6. For example, if the cavity was under UHV or exposed to air, and how well the assembly was isolated from vibrations.

Response:
The measurements were conducted in an UHV environment. The cavity rests within a glass cuvette, mounted onto our main vacuum chamber which operates at 1E-9mbar. The vacuum chamber is placed on a conventional optical table stabilized from external vibrations by pneumatic isolators (Newport I-2000). Due to space constraints in the glass cuvette, we did not mount the cavity on another passive isolation stage, thus the cavity is still coupled to vibrations from the vacuum chamber. Any components on the optical table that might introduce noise into the chamber (cooling fans, loose cables attached to the vacuum chamber) were switched off, removed from the table, or clamped down tightly.

We add this information at the end of the ``Cavity alignment'' section to ensure clarity as suggested.

Comment 2:
Different parts of the assembly have different thermal expansion coefficients. Thus, a change in the ambient temperature can drift the cavity alignment. Have you studied the long-term drift of the cavity alignment?

Response:
During our experiments, we observed a drift in alignment on the order of tens of minutes, which we attribute to thermal drift. The slow drift allowed us to implement an algorithm to automatically correct the cavity alignment. Long term drift has been thoroughly studied and is shown in our group's previous paper [Nguyen et al., 2018; reference 12 in the manuscript].

Comment 3:
On the first page the authors say: "To place neutral atoms at the cavity center, we use a magneto optical trap and a dipole trap in an ultra-high-vacuum (UHV) environment". This reads as if they have actually loaded atoms into this cavity. However, this is not the case in the manuscript. This needs to be rewritten to make it unambiguous.

Response:
We clarified the first paragraph of the "cavity design" section to address this ambiguity. We are currently in the process of placing atoms in the cavity but this is outside the scope of this work.

Comment 4:
The authors claim "The design significantly lowers the mechanical noise compared to our previous implementation [12]". This is an interesting point. But they have not provided any evidence. Ref [12] did not use the same figure of merit as the current manuscript. Thus, direct comparison is not possible for the readers. This needs to be elaborated.

Response:
As we do not have a similar figure of merit for an in vacuum noise measurement with that old system, we removed this statement.

Comment 5:
The authors have assumed that all mechanical noises primarily shift the resonance frequency of the cavity. However, a transverse misalignment of the mirrors can affect the mode matching to the desired fundamental mode and change the excited cavity modes. In other words, in the transmission spectrum of the cavity, noises and misalignments can change both the amplitude and the position (frequency) of the fundamental mode. It seems that the authors have ignored any changes in the amplitude of the transmission spectrum peak of the fundamental mode. This needs clarification and needs to be justified.

Response:
In our calculations, we assume that mode-decoupling from the desired cavity mode due to transverse mirror displacements is negligible during the measurement window. To verify this, we scanned a probe beam to observe the fundamental mode transmission spectrum of the NC cavity, and a near-planar cavity with the same support structure separately. We observed no significant difference in the fluctuations of the transmission peak values, thus we expect cavity resonance shifts to be the main contribution to the noise measurements. We added this information for clarification to the Cavity Stability section in the revised manuscript.

Comment 6:
The manuscript describes the mechanical properties of the cavity assembly. The authors should consider adding some optical responses of the cavity to the manuscript. For example, the transmission spectrum and the mode waist size. They also mention the mirror reflectivity of 99.5% and the expected finesse of 627. But they should also provide the measured values of the finesse or the cooperativity based on the waist size.

Response:
After the assembly and placement in UHV, the finesse and linewidth of the cavity at a critical distance of d = 1.06(5) um were measured to be 323(8) and 42(2) MHz, respectively. We added this information in the revised manuscript in the second paragraph Cavity Stability section.

Comment 7:
Why there are two piezos in Fig. 3, while the structure in Fig 2c shows three piezos? It seems they used two piezos for the test reported in Table 1. Is this correct?

Response:
The cavity structure is as shown in FIG.2 and contains three actuators for all measurements. FIG.3 only displayed two of the three actuators for clarity of the actuator's bases positions on

the cavity frames. In the light of the comment, we modified Figure 3 to display all three actuators.

From reviewer #2:

Comment 1:
It is not clear from the paper at which cavity length (or critical distance d) the cavity is characterized. The sentence ``This value is chosen as it ... will allow greater tip-tilt tuning as the cavity approaches concentricity'' suggests that the cavity is or will not be operated at d = 7.8 um. As a consequence the following questions arise.

a) What is the expected cavity mode diameter at the mirrors for the cavity length that was used in this work / will be used to achieve the coupling strength quoted in the one before the last paragraph?
b) Are there significant (with respect to the finesse) losses due to the finite mirrors size for the cavity at those lengths?
c) Would there be significant change in those losses from the transversal mirrors displacement, measured in this work?
d) See question 2.b

Response:
The value of d~7.8 um during the gluing of the mirrors is chosen such that it is half of the piezo's travel range. As the cavity approaches the concentric point (as d approaches 0), each piezo is able to travel ~7.8um in both directions, therefore increasing the cavity's tip/tilt adjustability. For this paper, the measurements were conducted for a critical distance of d = 1.06(5)um, corresponding to 3 FSR from the concentric point. The specific critical distance value at which the measurements were conducted were added to the revised manuscript.
At a critical distance d = 1.06(5)um, the cavity beam waist w at the mirrors is estimated to be w~0.37mm. The clear aperture diameter of the mirrors is specified to be 7.4 mm from the manufacturer. Due to the large clear aperture provided, the finite mirror size and any transversal mirror displacements measured in this work do not significantly impact the losses.

Comment 2:
Partially related to the last question. Authors state that “Close to the concentric point, transverse positioning noise will dominate the deviation of the cavity resonance from the atomic transition.” This statement appears to be central for all that follows in the manuscript. I recommend authors substantiate that statement with a brief explanation or literature.
a. If the statement above is true and d is known it looks like the measured effective cavity length variation could be converted to the actual mirror displacement or tilt, a value that could be useful for a specialized reader.
b. If the cavity was not characterized at the length that it is aimed to operate for the strong coupling, what will be the effective change dL, once the length of the cavity is increased to the operational one?

Response:
The main point is that as we approach the concentric regime, the cavity mode becomes more tightly focused. Consequently, the cavity becomes increasingly sensitive to transverse displacement compared to when it is further away from the concentric point. This heightened sensitivity leads to fluctuations in cavity resonance and transmission. Therefore, achieving the

near-concentric regime requires addressing transverse stabilization in the cavity structure. For simplicity, we represent all mechanical effects on the cavity resonance as effective fluctuations of the cavity length delta L_C. We tried to clarify this with rewriting the end of the "Stability requirement" subsection in the revised manuscript.
Using geometrical arguments, one can map the rms cavity length change to a rms transverse shift. A length change of delta L_{C, rms} = 0.36(2) Angstrom along the cavity axis corresponds to an rms transverse displacement of delta h_{C,RMS} = 12 nm. We tried to clarify this in a re-written Cavity Stability section.
The critical distance of d = 1.06(5)um is the target operation regime for our coming experiments, as it offers the best compromise in terms of theoretical atom--cavity coupling strength for a single atom, g_{th} = 2pi*12.5 MHz and our current cavity linewidth of 42(2) MHz.

Comment 3:
The method to characterise the cavity noise with the error signal of the laser that is locked to the same cavity raises several questions.
a. Authors state that the laser is “loosely locked” using integral feedback. I strongly recommend authors adding the information on the effective cutoff frequency of the feedback loop resulting from that particular integral controller. The interpretation of the Figure 6 is virtually impossible without the knowledge, in which part of the spectrum the lock is expected to track the cavity’s length.
b. The laser frequency noise is measured separately and presented in Fig. 6 (yellow curve), however authors do not clarify if this particular curve is measured while the laser is locked to the cavity under study. The answer to this question can significantly change understanding of Fig. 6.

Response:
In this paper, we tuned the strength of the integral part down such that it has a sub-Hz cutoff frequency. We clarified this in the Cavity Stability Section in the revised manuscript. For the measurement of the laser noise, as stated it was measured independently from the NC cavity, therefore it is not locked to the cavity while its frequency noise is measured. The intention is to provide a baseline noise level for comparison with the cavity noise. We tried to clarify this better by explicitly "...independently from the NC cavity setup,..." in the Cavity stability section of the revised manuscript.

Comment 4:
I find the notation “root mean square” to be more informative than “total noise”, used by authors. In fact in the current version only the algebra index “rms” allows the reader to be sure what the “total noise” exactly means.

Response:
We quote the noise always in root mean square, but want to emphasize that part of the noise energy is contained in certain spectral bands. We tried to address this better in the revised manuscript, and give a more specific reference in various locations.

Comment 5:
As the authors characterise the mechanical vibrations, It would be useful for the other researchers if authors could specify at least roughly how the whole setup was mounted. In particular, was there any active or passive vibration isolation? Was the setup placed on a

suspended optical table? Was additional care taken to damp resonances in the mechanical support and/or isolate the setup from the sources of the mechanical noise?

Response:
The measurements were conducted with the cavity in an UHV environment. The cavity rests within a glass cuvette, mounted onto our main vacuum chamber which operates at 1e-9mbar. The vacuum chamber is placed on an optical table stabilized from external vibrations by pneumatic isolators (Newport I-2000). Due to space constraints in the glass cuvette, we did not mount the cavity on a passive isolation stage, thus the cavity is still coupled to vibrations from the vacuum chamber. Any components on the optical table that might introduce noise into the chamber (cooling fans, loose cables attached to the vacuum chamber) were switched off, removed from the table, or clamped down tightly. We will add this information at the end of the ``Cavity alignment'' section to ensure clarity as suggested. We significantly expanded the end of the cavity alignment subsection to elaborate on this in the revised manuscript.

Apart from that, we also noted that we used a wrong notation for quoting uncertainties in experimentally measured values, hence a number of corrected values in the revised manuscript. We also tried to harmonize the text to American spelling.

With this, we hope to have addressed the comments of the referees, and look forward for your consideration.

With Best Regards on behalf of all authors,

Christian Kurtsiefer