Response Letter for manuscript LQ17413/Seidler

Dear Editor,

We thank the reviewers for their detailed comments. Please find our response to the respective reviewers.

Reply to Referee A:

We address Referee A's concern about the manuscript, especially with regards to whether the results reflect a wide-enough impact over previously reported techniques:

1. **"Could the authors elaborate more on the advantages of using an asymmetric cavity as the dispersive media as opposed to the other conventional methods?"**
   We have added an explanation in the paper (page 1, right col, 2 para) of how conventional methods such as using optical fibres as a dispersion medium is unfeasible for our application as the required fibre length would lead to complete extinction of the photon due to losses.

2. **"Are the authors aware of published work demonstrating a spectral compression down to a narrow linewidth like 8MHz? ... Is this the first report of compression near an atomic bandwidth?"**
   We have added [26] which is a work demonstrating a spectral compression of narrowband coherent pulses using a gradient echo memory. However, the spectral bandwidth they are operating on are narrower than the atomic absorption linewidths (page 1, leftt col, 2 para) and is not as adaptable to different ranges of bandwidths as our method.

3. **"Does the asymmetric cavity have to stay phase locked with the EOM signal? If so, how is this done in the experiment?"**
   We do not require a phase lock. Instead, the cavity resonanace frequency is locked to the center frequency of the photon spectrum, such that it causes dispersion to the transitting photon.

4. **"The authors wrote they attribute the dip in the spectrum of the uncompressed photons to the reabsorption of the generated photons by the atomic cloud. However, later they write that the extracted linewidth of the dip does not match the 5S1/2 to 5P1/2 transition and further work needs to be done. How significant is this feature to the effectiveness of their time-lens method? If it just an artifact from the way they are generating the single photons, or could the source of this feature be related to the technique?"**
   The dip is also observed even if the photons do not pass through the asymmetric cavity and EOM. As this dip is observed regardless of the compression optics, we deem this dip to be a charateristic of the photon pair source, rather a feature related to the compression technique. (addressed in page 4, left col, 2 para).

5. **"Concerning the phase flip being applied right after the first part of the dispersed photon exits the modulator and the second part starts to propagate through it. How did you measure/calculate/confirm this? Is it based on the length of the EOM and time for light to propagate from one end of the modulator to the other? "**
   Yes, it is based on the length of the EOM and time in which light propagates from one end of the modulator to the other. The photon intensity envelope is roughly 80ns long, which corresponds to a distance of about 24m. The active length of the EOM is about 90mm long, which corresponds to a timing uncertainty of 0.3ns. At one time, only a small part of the photon resides inside the

EOM and we can apply different phase shifts to different parts of the photon. We have added this clarification in page 3, right col, 2 para.

6. **"the uncompressed bandwidth is quoted as 20.6(2)MHz in some locations and 20(2)MHz in other places in the manuscript. Which is correct?"**
   Both values of the bandwidths are correct. The 20.6(2) MHz is the bandwidth inferred from the temporal profile, which was used as part of fitting parameters. 20(2)MHz is the bandwidth observed from the spectroscopy using the 2.6 MHz cavity. We made the distinction clearer by describing the respective bandwidths as *"inferred"* and *"observed"* in the text.

7. **"p.g.1: spelling error "makeing" on pg 1, 2nd paragraph"**
   We corrected the speling to *"making"*.

8. **"Fig 4: label black line too... difficult to see (corrected to new)"**
   We have increased the weight of the black line in the diagram.

9. **"pg 4: Fig reference says "Fig. setup" instead of Fig 2"**
   We have deleted the "Fig. setup" as we found it unnecessary.

Reply to Referee B:

We appreciate Referee B's remarks, and his/her recommendation for its publication. In response to the comments, we changed the manuscript as follows:

1. **"While the bandwidth reduction is moderate (a factor 2.5) and accompanied by substantial loss (the overall transmission of the system is 21%), the authors argue that the loss could be improved upon... I would have at least liked to see a discussion in the manuscript whether this method can realistically provide higher brightness than spectral filtering, and by what realistic factor."**
   We have added a paragraph in the *Discusssion* section of the manuscript (page 4, right col, last para) to compare our spectral compression technique to a passive filtering process. We have also evaluated quantitatively how a replacement of the fibre-based EOM with a free-space EOM results in a brightness that is better than passive filtering.

2. **"I assume that the authors have considered a Raman absorption process between magnetic sublevels that could exhibit a smaller linewidth?"**
   We expect the splitting of magnetic sublevels in a field gradient to result in a inhomogenously broadening in the spectral profile, in which a Raman absorption process would exhibit a broadened absorption linewidth, rather than a smaller linewidth observed.

With this, we hope to have addressed the concerns with our manuscript, and look forward for your reply.