Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Superradiance in cold atomic gases used confined optical geometries is at the heard of a rapidly developing research area. In particular in conjuction with long lived clock states it could be an alternative route towards a new superior frequency standard.

One of the foundational papers demonstrating the possible combination of clock atoms and hollow core fbires was published by some of the authors a few years ago. Here they have gone significantly beyond previous work, demonstrating and analyzing superradiance on an actual clock transition in great detail and precision. This renders this possibility from a theoretical concept to a practical route to continue.

The work is sound, well presented and the results look solid and convincing. The experimental achievements are impressive and go well beyond their previous setup and related work:(Solano, Pablo, et al. Nature communications 8.1 (2017): 1857).

The theoretical model is adequate and fits fairly well to the data including the multimode character of the dipole-dipole interaction. This, howeverm some recent other theoretical work on this setup, which seems not be known to the authors but should be mentioned: (Ostermann, Laurin, et al, New Journal of Physics 21.2 (2019), 025004.)

In addition in many experiments it was noted recently that superradiance is very often tied to subradiance (Cottier, Florent, Physical Review A 98.1 (2018): 013622, or Zoubi, H., EPL (Europhysics Letters) 90.2 (2010) 23001). It would be interesting to comment why subrandiance was not noticed or did not play a role here.

In summary, apart from the above minor comments I recommend publication of this work

Reviewer #2 (Remarks to the Author):

In this paper the authors study the collective emission of many atoms, initially prepared in the excited state, into a multimode fiber. They show that the coupling efficiency into the various fiber modes increases with the atom number and can be astonishingly large, approaching unity if all modes are considered.
The superradiant emission is one of the important results of this paper. The authors mention a 55 times enhancement, corresponding to a 0.38micorsecond burst. It would be important to discuss this feature with the corresponding experimental data in a figure (Figure 5 contains numerical data only). Also, it would be very important to show experimental data how this time scales changes with the atom number and density.
The density dependent frequency shift as shown in Fig 3d is another important result of this work. But it needs to be compared the results obtained by the same group in 2014 [25]. Are the results in this paper different or more complete than those of [25]? A close look to the low density limit might indicate a nonlinear density dependence (a kink in the red data point)? Also, when extrapolating a line for the black data points, it does not seem to intersect the 0 frequency shift at low density. It would be relevant to compare on the same plot experimental and numerical data points.
It is also important to clarify the difference in the frequency shift between frequency shift of the driving laser for maximal emission or frequency shift in the emitted light. Another clarification which needs to be done is the difference (or similarity) to dipole-dipole coupling shifts occurring in the low excitation limit [e.g. Nature Communications 7, 11039 (2016)].
Another important aspect concerns possible interpretation of quantum features this work. As the system is initially inverted, one might expect quantum correlations between atoms to develop

during the collective emission process. Without asking for a full quantum treatment, it would be important to know whether such correlations would be captured by the MBE used in this paper. If so, it would be important to discuss the role of such non classical correlations? If not, it would be nice to discuss to what precision this experiment can rule out such quantum correlations.
With these comments, I consider that this work is not yet suitable for publication in Communications Physics. Without need for further experiment a more detailed presentation and discussion would allow this work to be of interest to a specialized community.

Reviewer #3 (Remarks to the Author):

Please see the attached file for comments for Authors.

Reviewer #3 (Remarks to the Author):

The manuscript by Okaba et al presents a thorough experimental and theoretical/numerical studies on the superradiance of lattice-confined atoms in a hallow core fiber. Superradiance effect has been a widely discussed topic, finding its importance in E&M, quantum optics and quantum communications. Due to the development of photonic waveguides/cavities over the past decades, new scenarios for creating spatially extended superradiant samples become possible. One of the prominent examples is the so-called time Dicke superradiance, which is presented in this study and is induced and guided by a multimode hallow core fiber. The spatial and spectral property of the emission field is studied by coupling the superradiant emission to a single mode fiber and also probed with a homodyne detection method to extract additional frequency/phase information. Overall, the authors present a superradiant study that is timely, and is in principle suited for publication in communications physics. I believe the authors have done a careful job in presenting valid results. My main concern, however, is that the manuscript itself is written in a convoluted way and isn't very easy for readers to grasp the full picture. This would greatly hinge the impact of this paper. Below are some of my comments that I think the authors should consider in the manuscript revision. First, regarding the main results:

The main discussion begins from a section called "Superradiance ringing". The result (Fig. 2) isn't at all a conventional superradiant emission signal (i.e. enhanced decay), but contains multiple oscillations in the emission envelope. It is later understood that this "ringing" comes from the fact that the SR field oscillates between the lowest order fiber mode and the higher order modes (Fig. 6), where only the former couples into the single mode fiber that is responsible to the signal presented in Fig. 2. I was confused by several time periods described in this section, including the pumping period, and a "burst" time scale. It isn't clear at all what it means by the 1/sqrt{e} width of the burst, which I thought refers to the width of the first SR burst peak. The envelope however doesn't look exactly like a Gaussian. There are also no more discussions on the second and the third bursts which are part of the "ringing" signal. Several questions arise whether they come from population trapping or re-excitation as the SR field propagates within the cloud. I can't seem to understand this after going through the whole paper especially after reading the discussions surrounding Fig. 6.

As a reader, it wasn't clear to me that there is SR until I look at the total decay signal reported in Fig 4a. After this figure, it becomes clearer to me that SR decay is primarily contained in the first 1 microsecond after the pump shuts off. The ringing signal (second and third bursts) we have been seeing in Fig.2 is parametrically amplified by a local oscillator and contains much less energy than the first SR burst. Unfortunately, the authors didn't describe any of these because the purpose of fig4 is only for "the efficiency of SR".

So my first significant comment is: why don't the authors present the most straightforward signal Fig.4a first? It would be much easier for the readers to understand what SR in this system is all about. Then the authors can continue to discuss about the "ringing effect" in Fig. 2, followed by frequency shift Fig.3. In addition, I think a careful discussion on the cause of ringing, including discussions addressing my above comments/questions may be considered. Additional discussions on the ratio of energy contained in the second and the third bursts and their significance may be discussed. Otherwise, why should we, as readers, bother knowing about the ringing effect that contains very small energy?

My second significant comment regards the discussion on the "Frequency Shift". The first part of this section contains technical discussions on how frequency shift is extracted, while the second part discusses about the conditions that may affect the shifts. My honest suggestion is that the technical part should either be rewritten in a clear manner or should all be moved to the Methods section. Reading only the main text, I have no clue how the “purple” curve in Fig.3c converts into the “blue” curve. It doesn’t serve a stand-alone and meaningful purpose helping us to understand the extraction of frequency shift (although I do greatly appreciate the cleverness of the extraction method).

I’d rather the authors save the space to expand the discussion on why there is a density-dependent frequency shift. As motivated in the introduction, the readers may want to know about its physical origin: Is this due to collective Lamb shift or is it from something else?

Another smaller comment regarding the presentation of the paper is that, the discussion on the pre-filtered signal (like the red line in Fig. 2b) may be pushed to Methods or SI. Knowing the time delay or the original signal shape doesn’t help the readers understand better the SR effect in a hallow core fiber.

Besides the above significant comments, I have following minor ones that I think the authors could consider:

1. The description between lines 37-41 is a result of atoms superradiantly coupled to the field but not the cause. I believe it is due to the standing wave nature of cavity field that SR of multiple atoms in an extended cloud becomes possible.
2. What it the temperature of the cloud? In line 96, the authors stated the atomic size in a pancake trap. Are those based on the calculation of single atom wave function? What is the motional state?
3. the description in line 100 isn’t entirely correct. Lattice wavelength can be far-off resonant from lambda_0, but can still satisfy Bragg condition. For example, lambda_L=2lambda_0. Incommensurate wavelength or irrational ratio may be a better description.
4. In lines 109-110, the authors claim that the observation is consistent with timed-Dicke superradiance. However, this claim is not substantiated in this section, but rather in Fig. 4b. This is part of the reason why I suggest Fig.4 should be moved forward to Fig. 2.
5. In line 192, how is numerical aperture defined here? Is it related to the atom-photon cross section/mode area or to the cross sectional area of the cloud/mode area? This should be explicitly given. Conventional definition is the former case.
6. The discussion surrounding line 201 may be problematic. I would have thought that \gamma_SR, and the fundamental mode (f) and higher order mode (h) contributions are all “single atom” parameters and are constants. They only depend on the mode property but not on the atom number. The fact that mode energy goes into the higher order mode is due to better directional SR emission that is nonlinear with respect to N. The authors seem to use a definition that includes N-dependence in \gamma_SR. Please clarify.
7. In line 219, please indicate the cause of the loss of selected modes.
8. Lines 268-269 are confusing. It should be improved. It isn’t clear what the central peak of the intensity profile does to mode competition.
9. Higher order mode discussions in line 249 seems to contradict with line 237. The latter states that higher-order modes possess negligible effect on atom-light coupling, while the former suggests they do. How do the authors reconciliate this?

Response to reviewers' comments

We thank the reviewers for the helpful comments. We have revised our manuscript according to the comments by the reviewers.

Reviewer #1 (Remarks to the Author):

Superradiance in cold atomic gases used confined optical geometries is at the heart of a rapidly developing research area. In particular in conjuction with long lived clock states it could be an alternative route towards a new superior frequency standard.

One of the foundational papers demonstrating the possible combination of clock atoms and hollow core fbires was published by some of the authors a few years ago. Here they have gone significantly beyond previous work, demonstrating and analyzing superradiance on an actual clock transition in great detail and precision. This renders this possibility from a theoretical concept to a practical route to continue.

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The theoretical model is adequate and fits fairly well to the data including the multimode character of the dipole-dipole interaction. This, however some recent other theoretical work on this setup, which seems not be known to the authors but should be mentioned: (Ostermann, Laurin, et al, New Journal of Physics 21.2 (2019), 025004.)

Reply:

We thank the reviewer for pointing out this excellent theoretical work [New J. Phys. 21, 025004 (2019)], and we have cited it in the revised manuscript (ref. 39).

It should be noted that (1) The system considered in this theoretical work is the 1D atom chain. This is different from our experimental system, where each lattice site is occupied by ~10-100 atoms coupled with the long-range dipole-dipole interaction. Due to the relatively large lattice constant, the long-range dipolar interaction among the atoms in different lattice sites are negligible; and (2) In this theoretical work, the variables associated with the fiber modes are eliminated under the adiabatic (Markov) approximation. Instead of directly investigating the behavior of light field propagating inside fiber, the collective decay of the total excited atoms is focused on. However, as illustrated in [Phys. Rev. A 89, 023616 (2014)], due to the nonlocal character of the collective dissipation, the photon emission from an ensemble of interacting atoms does not depend proportionally on the dissipative time evolution of the excited-atom population.

We have added the relevant discussion in the revised manuscript (lines 230-234).

In addition in many experiments it was noted recently that superradiance is very often tied to subradiance (Cottier, Florent, Physical Review A 98.1 (2018): 013622, or Zoubi, H., EPL (Europhysics Letters) 90.2 (2010) 23001). It would be interesting to comment why subrandiance was not noticed or did not play a role here.

Reply:

We thank the reviewer for this comment. The radiation from a many-atom system includes both superradiant and subrandiant processes. However, the subrandiant signal is deeply buried in the superradiance, hardly being observed. As demonstrated in [Phys. Rev. Lett. 116, 083601 (2016)], even using a far-detuned laser to pump the atomic gas, the subrandiance is still 10^{-3}~10^{-4} times weaker than superradiance.

Nonetheless, the sensitive heterodyne-detection scheme applied in our work will allow accessing orders of magnitude weaker subradiant process, which commonly falls well behind the SR process, by extending the measurement time sufficiently longer than γ_0^{-1}. We have added the relevant discussion (lines 133-136) and references [Phys. Rev. Lett. 108, 123602 (2012), Phys. Rev. Lett. 116, 083601 (2016)] (ref 35, 36) in the revised manuscript. In this research work, we mainly focus on the superradiance. Identifying the subradiance from the lattice-confined atoms inside the fiber will be described elsewhere.

Reviewer #2 (Remarks to the Author):

In this paper the authors study the collective emission of many atoms, initially prepared in the excited state, into a multimode fiber. They show that the coupling efficiency into the various fiber modes increases with the atom number and can be astonishingly large, approaching unity if all modes are considered.

The superradiant emission is one of the important results of this paper. The authors mention a 55 times enhancement, corresponding to a 0.38 micorsecond burst. It would be important to discuss this feature with the corresponding experimental data in a figure (Figure 5 contains numerical data only). Also, it would be very important to show experimental data how this time scales changes with the atom number and density.

Reply:

We thank for the review's comment to highlight the superradiant decay dependent on number of atoms. We have inserted Fig.2b (experimental) and Fig.5c (theoretical) to illustrate the dependence of the decay rate γ_{bw} (the reciprocal of the temporal width) of the first SR burst on the number of atoms N. The result shows a linear relation $\gamma_{\mathrm{bw}} \propto N$, manifesting the superradiance. We have also added the relevant discussion in the revised text (lines 136-139, 225-227, 286-289, 402-403 and 436-437).

The density dependent frequency shift as shown in Fig 3d is another important result of this work. But it needs to be compared the results obtained by the same group in 2014 [25]. Are the results in this paper different or more complete than those of [25]? A close look to the low density limit might indicate a nonlinear density dependence (a kink in the red data point)? Also, when extrapolating a line for the black data points, it does not seem to intersect the 0 frequency shift at low density. It would be relevant to compare on the same plot experimental and numerical data points.

It is also important to clarify the difference in the frequency shift between frequency shift of the driving laser for maximal emission or frequency shift in the emitted light.

Reply:

Thank you for the appreciation of Fig. 3d. In order to interpret the results (1) we have introduced the error bars in Fig. 3d to address frequency measurement uncertainty (lines 366-368), and (2) we mentioned the possible occurrence of frequency chirping in the emitted light (lines 156-160). These effects may cause frequency uncertainty of 10 kHz-50 kHz which make it delicate to discuss the issues raised by the referee, such as, slight nonlinear dependence and non-zero intercept at $\rho = 0$. Detailed discussion on the frequency shift will be described elsewhere along with the further discussion on chirping. Nevertheless, the measured coefficient of the density shift $1 \times 10^9\ \mathrm{Hz/cm^{-3}}$ is consistent with our previous work [Nature Commun. 5, 4096 (2014)].

We have added the relevant discussion, which includes "the difference in the frequency shift between frequency shifts of absorption/emission" in the revised manuscript (lines 163-166).

Another clarification which needs to be done is the difference (or similarity) to dipole-dipole coupling shifts occurring in the low excitation limit [e.g. Nature Communications 7, 11039 (2016)].

Reply:

We thank the reviewer for this comment. In our experiment, for $N = 2 \times 10^5$ atoms confined in the unexpanded lattice, the mean occupancy of each site is about 10^2 and the interatomic separation (in the *x*-*y* plane) is 300 nm, leading to a collective Lamb shift (caused by the exchange of virtual photons) plus Lorentz-Lorentz shift of about γ_0. The frequency shift observed in our experiment well exceeds either of them. Indeed, the resonant dipole-dipole interactions (RDDIs) between the atoms in the same lattice site primarily contribute to the observed density-dependent frequency shift.

We have added the relevant discussion in the revised manuscript (lines 170-175).

Another important aspect concerns possible interpretation of quantum features this work. As the system is initially inverted, one might expect quantum correlations between atoms to develop during the collective emission process. Without asking for a full quantum treatment, it would be important to know whether such correlations would be captured by the MBE used in this paper. If so, it would be important to discuss the role of such non classical correlations? If not, it would be nice to discuss to what precision this experiment can rule out such quantum correlations.

Reply:

We thank the reviewer's comment for discussing the role of interatomic correlations. In our Maxwell-Bloch formalism, we neglect the quantum correlation between atoms for the reasons: (i) The SR behavior computed under this approximation matches well with the experimental results, which infers validity of neglecting the interatomic correlation to describe the present observations. (ii) The limited computer memory restricts the system size that could be numerically simulated. The maximum of system size reaches N=10^5 and the simulation time is about one week. In principle, the MBEs can be extended to involve the interatomic correlation, e.g., [New J. Phys. 21, 025004 (2019)]. However, the solvable size of the system will be further shrunk when considering the interatomic correlation.

To discuss the role of quantum correlation in superradiance, one has to consider two distinct situations. (1) A small system with a size of smaller than the radiation wavelength. Since the interatomic separation is much shorter than the radiation wavelength, the virtual-mediated dipolar interaction can be induced, affecting both the energy level (Lamb shift) and the spontaneous emission rate of an atom. The behavior of one atom strongly affects other nearby atoms due to the light-induced dipolar interaction. In this case, the quantum correlation between atoms plays an important role in photon emission and must be considered. (2) One-dimensional system with a size much larger than the radiation wavelength. The spontaneous emission may be enhanced from one end to the other end of the sample, giving rise to a burst of radiation. In a large sample, the behavior of an individual atom cannot affect the behavior of the macroscopic system. In this case, one may apply the "mean-field" model by neglecting the quantum correlation between atoms. Actually, this point has been illustrated in [Phys. Rep. 93, 301-396 (1982)]. The quantum correlation between atoms is not a necessary condition for the generation of superradiance.

In summary, comparing the experimental measurement and the numerical results derived without involving the interatomic correlation, we find the role of quantum correlation between atoms is negligible. We have inserted the relevant discussion in the revised Supplemental Materials (lines 186-189).

With these comments, I consider that this work is not yet suitable for publication in Communications Physics. Without need for further experiment a more detailed presentation and discussion would allow this work to be of interest to a specialized community.

Reply:

We have revised the manuscript according to reviewer's comments. We have inserted Fig. 2b and Fig. 5c and relevant discussions in the manuscript. In particular, clear scaling of enhanced radiation dependent on number of atoms, which is added in the revise manuscript according to the reviewer's comment, will be of interest to broad readers.

Reviewer #3 (Remarks to the Author):

The manuscript by Okaba et al presents a thorough experimental and theoretical/numerical studies on the superradiance of lattice-confined atoms in a hallow core fiber. Superradiance effect has been a widely discussed topic, finding its importance in E&M, quantum optics and quantum communications. Due to the development of photonic waveguides/cavities over the past decades, new scenarios for creating spatially extended superradiant samples become possible. One of the prominent examples is the so-called time Dicke superradiance, which is presented in this study and is induced and guided by a multimode hallow core fiber. The spatial and spectral property of the emission field is studied by coupling the superradiant emission to a single mode fiber and also probed with a homodyne detection method to extract additional frequency/phase information. Overall, the authors present a superradiant study that is timely, and is in principle suited for publication in communications physics. I believe the authors have done a careful job in presenting valid results. My main concern, however, is that the manuscript itself is written in a convoluted way and isn't very easy for readers to grasp the full picture. This would greatly hinge the impact of this paper. Below are some of my comments that I think the authors should consider in the manuscript revision. First, regarding the main results:

Reply:

We thank the reviewer for his positive comments on the thoroughness, timeliness and care of the work. We have revised the manuscript according to the reviewer's concerns.

The main discussion begins from a section called "Superradiance ringing". The result (Fig. 2) isn't at all a conventional superradiant emission signal (i.e. enhanced decay), but contains multiple oscillations in the emission envelope. It is later understood that this "ringing" comes from the fact that the SR field oscillates between the lowest order fiber mode and the higher order modes (Fig. 6), where only the former couples into the single mode fiber that is responsible to the signal presented in Fig. 2. I was confused by several time periods described in this section, including the pumping period, and a "burst" time scale. It isn't clear at all what it means by the 1/sqrt{e} width of the burst, which I thought refers to the width of the first SR burst peak. The envelope however doesn't look exactly like a Gaussian. There are also no more discussions on the second and the third bursts which are part of the "ringing" signal. Several questions arise whether they come from population trapping or re-excitation as the SR field propagates within the cloud. I can't seem to understand this after going through the whole paper especially after reading the discussions surrounding Fig. 6.

Reply:

We agree with the reviewer that Fig. 2 is not similar to the commonly form to show the decay. This is due to the detection homodyne scheme, which allows obtaining amplitude and phase information simultaneously. However, we understand that such a highly technical point could obscure the main point. Our aim was to validate the results by straightforwardly describing our experimental procedure.

In the revised manuscript we have:

(1) Changed Fig. 2 to clearly indicate the decay of the first burst and corresponding width, and to identify the pump pulse and successive bursts.

(2) Added the N-dependent burst width (as suggested by the reviewer 2) in Fig. 2b, which represents a clear signature of superradiance.

(3) In the text we have put an emphasis on how to read each component in Fig. 2 (lines 124-125, 395-396 and 398).

As a reader, it wasn't clear to me that there is SR until I look at the total decay signal reported in Fig 4a. After this figure, it becomes clearer to me that SR decay is primarily contained in the first 1 microsecond after the pump shuts off. The ringing signal (second and third bursts) we have been seeing in Fig.2 is parametrically amplified by a local oscillator and contains much less energy than the first SR burst. Unfortunately, the authors didn't describe any of these because the purpose of fig4 is only for "the efficiency of SR".

So my first significant comment is: why don't the authors present the most straightforward signal Fig.4a first? It would be much easier for the readers to understand what SR in this system is all about. Then the authors can continue to discuss about the "ringing effect" in Fig. 2, followed by frequency shift Fig.3. In addition, I think a careful discussion on the cause of ringing, including discussions addressing my above comments/questions may be considered. Additional discussions on the ratio of energy contained in the second and the third bursts and their significance may be discussed. Otherwise, why should we, as readers, bother knowing about the ringing effect that contains very small energy?

Reply:

We thank the comments for changing the order of figures for the sake of readability. As stated in line 177, Fig. 4a is obtained after 25000 times averaging of experiments, which totally washes out the ringing feature of SR. Despite this huge averaging, intensity measurements work for deriving conversion efficiency for multimode SR, where our heterodyne technique in unavailable. One of the distinctive features of our work is phase-resolved single-shot measurement of SR that reveals the true behavior of SR without any data averaging. Therefore, we believe starting from Fig.2a, which is raw data without sample averaging, is scientifically right way to present results to the reader.

For the reader who is not familiar with the ringing phenomena, we indicate locations of a pump pulse, 1st SR burst and 2nd …, in fig.2a. in the revision.

As the referee conjectures that SR signal E_SR is amplified by the E_LO to obtain E_SR \times E_LO. This noise-free amplification is the key feature of our experimental investigation, allowing us to observe the SR phenomena without sample averaging. In order to convert the signal to a familiar SR-intensity signal, we depicted |E_SR \times E_LO|^2, in the lower panel in Fig2a. As referee mentions, intensity of the 2nd bust is 1/100 and its contribution is very small. However, this plot demonstrates very high dynamic range of our detection scheme, which allows presenting the results in the logarithmic scale. If this presentation is not familiar to the reader, this, in turn, indicates the novelty of our work.

We, therefore, think order of figures as it is, is scientifically right way to present our observations and novelty of our work to the readers.

My second significant comment regards the discussion on the “Frequency Shift”. The first part of this section contains technical discussions on how frequency shift is extracted, while the second part discusses about the conditions that may affect the shifts. My honest suggestion is that the technical part should either be rewritten in a clear manner or should all be moved to the Methods section. Reading only the main text, I have no clue how the “purple” curve in Fig.3c converts into the “blue” curve. It doesn’t serve a stand-alone and meaningful purpose helping us to understand the extraction of frequency shift (although I do greatly appreciate the cleverness of the extraction method).

Reply:

We appreciate the referee’s sincere suggestion. After carefully considering the reviewer’s suggestion, we have replaced Fig. 3b with the plot of the amplitude and the phase of $V_{\mathrm{RF}}(t)$ and added relevant sentences in the main text to intuitively establish the connection between the frequency shift Δ_{SR} and the phase $\theta(t)$ variation as time. We then briefly mention the analysis of Fig. 3c in the revised text (lines 146-156 and 405-409).

I’d rather the authors save the space to expand the discussion on why there is a density-dependent frequency shift. As motivated in the introduction, the readers may want to know about its physical origin: Is this due to collective Lamb shift or is it from something else?

Reply:

The collective Lamb shift and the Lorentz-Lorentz shift are not the main contribution to density-dependent frequency shift. In our experiment, for $N = 2 \times 10^5$ atoms confined in the unexpanded lattice, the mean occupancy of each site is about 10^2 and the interatomic separation is about 300 nm, leading to a collective Lamb shift plus Lorentz-Lorentz shift of about γ_0. Thus, the frequency shift observed in our experiment well exceeds any of them.

Indeed, the resonant dipole-dipole interactions (RDDIs) between the atoms in the same lattice site primarily contribute to the observed density-dependent frequency shift observed.

We have expanded the relevant discussion in the revised manuscript (lines 170-175).

Another smaller comment regarding the presentation of the paper is that, the discussion on the prefiltered signal (like the red line in Fig. 2b) may be pushed to Methods or SI. Knowing the time delay or the original signal shape doesn't help the readers understand better the SR effect in a hallow core fiber.

Reply:

We thank the reviewer for the comments. According to the comments, we concentrate on showing the experimental observation of SR in Fig. 2 and put the theory calculation to Fig. 5.

Besides the above significant comments, I have following minor ones that I think the authors could consider:

1. The description between lines 37-41 is a result of atoms superradiantly coupled to the field but not the cause. I believe it is due to the standing wave nature of cavity field that SR of multiple atoms in an extended cloud becomes possible.

Reply:

Common guided modes are the essence of the SR rather than the standing wave nature of the cavity field. To indicate fact, we cited experiments that employ optical guides, which are cited later in the original MS, in the relevant sentence (line 37).

2. What it the temperature of the cloud? In line 96, the authors stated the atomic size in a pancake trap. Are those based on the calculation of single atom wave function? What is the motional state?

Reply:

The temperature is measured to 4.0 μK by the Doppler width of the spectrum, corresponding to motional states $n_{\mathrm{a}} \sim 0.5$ (axial) and $n_{\mathrm{r}} \sim 33$ (radial). The pancake volume is calculated assuming thermal distribution of atoms with the measured temperature. We have added the relevant discussion in Methods (lines 324-326).

3. the description in line 100 isn't entirely correct. Lattice wavelength can be far-off resonant from lambda_0, but can still satisfy Bragg condition. For example, lambda_L=2lambda_0. Incommensurate wavelength or irrational ratio may be a better description.

Reply:

Thanks for the suggestion. We have revised the relevant statement as 'the lattice period $\lambda_{\mathrm{L}}/2$ does not fulfil the Bragg condition for the transition wavelength λ_0' (lines 102-103).

4. In lines 109-110, the authors claim that the observation is consistent with timed-Dicke superradiance. However, this claim is not substantiated in this section, but rather in Fig. 4b. This is part of the reason why I suggest Fig.4 should be moved forward to Fig. 2.

Reply:

Actually, in his pioneering works [Phys. Rev. Lett. 102, 143601 (2009) and Science 325, 1510 (2009)], Scully answered the question: Why the emitted SR photon should go in the same direction as the exciting photon? He attributes this into the timed-Dicke state.

In our experiment, we have checked if there is any portion of SR light coming out from the input end of the fiber after the pump pulse is applied. (The input end is where the pump pulse goes into the fiber.) We find that the SR light always outputs from the output end of the fiber. This observation is consistent with the timed-Dicke SR. This discussion is given in the original MS in lines 109-111 (in lines 111-112 in the revised manuscript).

5. In line 192, how is numerical aperture defined here? Is it related to the atom-photon cross section/mode area or to the cross sectional area of the cloud/mode area? This should be explicitly given. Conventional definition is the former case.

Reply:

The definition of NA in this work is based on the fiber-mode area, that is, NA$\sim\lambda/(\pi w_0)\sim 0.019$ where $\lambda = 689$nm is SR wavelength and $w_0 = 11.8$ μm is the fiber mode radius. We have given the explicit expression of NA in the revised manuscript (line 213).

6. The discussion surrounding line 201 may be problematic. I would have thought that \gamma_SR, and the fundamental mode (f) and higher order mode (h) contributions are all "single atom" parameters and are constants. They only depend on the mode property but not on the atom number. The fact that mode energy goes into the higher order mode is due to better directional SR emission that is nonlinear with respect to N. The authors seem to use a definition that includes N-dependence in \gamma_SR. Please clarify.

Reply:

We thank the reviewer's comment for pointing out this misunderstanding. As indicated by the fundamental mode coupling efficiency in indicated by empty symbols, $\gamma_{\mathrm{SR}}^{(\mathrm{f})}$ can be nonlinear with respect to N while they are almost linear up to 10^5 as shown in Fig. 2b and Fig. 5c. We agree that this effect is caused by the better directional SR emission of the higher-order fiber modes in Group I. We have clarified this in the revised manuscript (lines 219-223).

7. In line 219, please indicate the cause of the loss of selected modes.

Reply:

The calculated loss of the fiber is due to what is commonly refer to as confinement loss. This quantity indicates the light confinement strength of the cladding design. As such, in our simulation other sources of loss such scattering (mainly due to surface roughness of the silica core surround) or bend loss. Furthermore, the fiber guides via Inhibited Coupling mechanism. By virtue of this mechanism, the core mode confinement is ensured by a strong mismatch in transverse phase between the core mode and the cladding modes, and by highly localizing the cladding silica modes within the silica struts. These properties favor a core contour with negative curvature to guide modes with Gaussian-like profile. Hence the lowest loss mode is that of HE_11 (i.e. LP_01 in the linear polarization approximation). The effect of the core contour on the loss of the core modes was exploited in one of our recants work ("Tailoring modal properties of inhibited-coupling guiding fibers by cladding modification [JH Osório, et al.Scientific reports 9 (1), 1376]") to reduce the loss of higher order core modes over the fundamental modes.

We have added the relevant discussion in the revised manuscript (lines 247-248).

8. Lines 268-269 are confusing. It should be improved. It isn't clear what the central peak of the intensity profile does to mode competition.

Reply:

The transverse fiber modes involved in the numerical simulation can be divided into two groups: Group I: the modes with their intensity patterns maximized at the central point, i.e., LP_{01}, LP_{02} and LP_{03}; and Group II: the modes whose intensity patterns do NOT have central peaks, i.e., $LP_{11}^{a,b}$, $LP_{21}^{a,b}$ and $LP_{31}^{a,b}$. Since in the *x*-*y* plane the atoms are mainly distributed in an area with a radius of 1.7 μm, the fiber modes in Group I get enhanced prior to the fiber modes in Group II. In response to this advice, we have revised the corresponding statement in the revised manuscript (lines 243-245, 284-285).

9. Higher order mode discussions in line 249 seems to contradict with line 237. The latter states that higher-order modes possess negligible effect on atom-light coupling, while the former suggests they do. How do the authors reconciliate this?

Reply:

In Line 237, we mean that the effect of high-order modes (e.g., LP_{04}, LP_{05}, ...) in <u>Group I</u> are negligible. This is because the numerical simulation involving, for example, LP_{04} (or higher-order fiber modes), gives the theoretical results inconsistent with the experimental measurement. Thus, we omit these higher-order fiber modes.

In contrast, in Line 249, we mean that more higher modes (e.g., $LP_{22}^{a,b}$, $LP_{32}^{a,b}$, ...) in <u>Group II</u> should be included in the simulation. This is because these fiber modes will be helpful to tailor the emission pattern (in particular, the donuts pattern, see Fig. 5b) within the pumping period. Since not enough higher modes in Group II are included in the simulation, the absorbed energy within the pumping period is larger than the real value, resulting in a reduced coupling efficiency κ derived from the numerical simulation (Fig. 6d) in comparison to the experimental measurement (Fig. 4b). However, due to the limited computer memory and computation time scale, we cannot include many modes in Group II in the simulation.

We have revised the corresponding statements in the revised manuscript (lines 266-270 and 300-302).

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have sufficiently dealt with the referees concerns;

the paper is an important contibution for the future development in this field and should be published in this form

Reviewer #2 (Remarks to the Author):

After reading the reply of the authors to my previous comments, I consider that the most important issues have been addressed in the new version of the paper. Even though some issues remain somewhat open (on Fig. 3d a fit with zero intercept will not work nicely, even with the error bars) and that the paper remains difficult to read, I recommend publication of this work.

Reviewer #3 (Remarks to the Author):

I have carefully read the reply of the authors and the revised manuscript. The authors have carefully addressed my questions and comments. They have also addressed other referees very adequately. The revised manuscript reads clearer. Also, I respect the authors' judgement on the presentation order of their work and find no further comments. I recommend publication.