Peer Review File

Manuscript Title: Many-body cavity quantum electrodynamics with driven inhomogeneous emitters

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

The paper by Lei et. Al. reports a new cavity QED phenomenon that is unique to inhomogenously broadened ensembles. In this effect, a resonant laser that strongly drives the systems induces an atomic coherence, leading to destructive interference which results in a narrow transparency window. By measuring the decay rate of the atomic ensemble at different excitation powers, the authors observe a transition from super-radiant emission to increased contribution from the sub-radiant Hilbert space. This new phenomenon provides a new tool to generate coherent radiative effects in an inhomogeneous atomic medium. I recommend publication once the following points are clarified.

1) Based on the intuitive explanation the authors provide, the sharp dip observed in Fig. 2 results from a combination saturable absorption for on-resonance ions, plus destructive interference for the off-resonant ensemble. Both of these effects, however, also exist when the laser is detuned from the peak of the atomic absorption. The destructive interference is not perfect in this case, but should still be present because there is significant atomic population on both the red and blue shifted ions relative to the laser. I would thus expect a significant modification of the reflectivity even off resonance. Why is the effect only present right at the resonant condition?

2) The authors indicate that the cavity mediates direct photon exchange between the different atoms. But in Eq. S17, the authors assume the that the <a\sigma> = <a><\sigma>. This approximation appears to throw out direct exchange of excitation between ions. So is such direct exchange needed to observe CIT?

3) The theoretical model introduce by the authors to explain the dynamics of the system is a bit unconvincing. The authors model six atoms resonant with the cavity. That seems like a very different system than what they actually have. Is there a way to improve this model?

4) It would be instructive to see what happens to the dynamics of the system when the laser is detuned from the minimum peak.

Referee #2 (Remarks to the Author):

I find the manuscript by Lei et al. very well written, and clear in most of its parts. From what I read,

the key findings of the paper are a new collectively induced transparency and its implications for many-body sub- and super-radiance. This is certainly interesting but a lot of the anticipated applications and impact of these findings are just stated at page 1, and never elaborated in the main text. So I will assume that CIT and the results centered around page 5, are the key findings of this work.
Now, I appreciate that a solid-state implementation of cQED problems offer new perspectives and certainly the results contained in the manuscript are worth of publication in a very good journal, but I don't see how they scale up to 'Nature' level.

After all, we are still talking of a model (eq 2) which is extremely well studied in quantum optics and which is shown here to enter a new interesting regime at strong drive.
I see this punchline as incremental: (1) there is nothing truly many bodysh here (the model is still collective, although I appreciate the rich decay dynamics they find outside the Dicke manifolds); (2) as I said, prospects for outreach of their findings is just anticipated, not truly worked out; (3) the actual merit, in my opinion, is indeed the new experimental setup the authors realize and fit amazingly well.

A few points of confusion through the text:

— why do the authors say the most cavity QED is done with small number of identical emitters? I would say we have a lot of works and experiments with several constituents in a collective regime

— the definition of strong drive should be spelled out clearer and more prominently, I had to navigate through the Appendix to understand what they were talking about. It is also not completely transparent how this better achievable in this platform wrt to pre-existing ones

And some further speculations:

— the model the authors study is regarded as a way to simulate BCS superconductors in and out of equilibrium. See for instance A. M. Rey's group papers (building on almost 15 years of previous literature in the cond mat/ math phys community). What can the authors infer from their results that connect to this physics? Probably there is no direct relation since they work in the strong dissipative limit, but it would be nice to see a few lines of comment on this.

Referee #3 (Remarks to the Author):

The manuscript by Lei et al. reports on experiments on collective nonlinear interactions in an ensemble of solid-state emitters in an optical cavity. The central claim is the observation of a transparency effect in the reflection of the ensemble, which the authors coin "collectively induced transparency." The experiment is backed by a theoretical model building on an extension of the Tavis-Cummings Hamiltonian. The manuscript is generally well-written although some important details are dealt with in a somewhat cavalier manner, in particular data analysis and theory. However, I remain concerned about both novelty and correctness of the claims as outlined below.

Novelty and correctness: The authors should comment on the relation to Blaha et al., PHYSICAL REVIEW A 105, 013719 (2022), which is a surprising omission in the reference list. In particular Fig. 4c in Blaha et al., where a similar transparency is predicted and measured but explained by deficiencies of the Tavis-Cummings model rather than inhomogeneous broadening. To be concrete, Blaha et al. explains the criteria for the validity of the Tavis-Cummings model of which the most important is (Eq. (9) in Blaha et al.): FSR >> g_N^2/gamma_l. Now, taking an average decay rate of 1/(590µs) from the author's own work (Ref. 41), assuming 50 participating ions (line 249), and g = 2*pi*35e6 Hz (line 655), estimating the FSR from the authors own work on the same device (Ref. 38, Fig. 3d), we arrive at

g_N^2/gamma_l = (sqrt(50)*2*pi*35e6Hz)^2/(1/(590µs)) ~ 1.4e15 Hz

and
FSR = (c/(1527e-9m)-c/(1561e-9m)) ~ 4.3e12 Hz.

Unless something is wrong in the numbers used here, the TC-model is therefore not valid in the regime of the present experiment. It could be that the ad-hoc amendments to the TC-model made by the authors eventually can mimic the more rigorous approach by Blaha et al., but in any case, the novelty of the claims by the authors is elusive because Blaha et al. already report a collectively induced transparency (without naming it so). While the experimental data reported by the authors is clearer than the data in Blaha et al. and this should certainly merit publication in a reputable journal such as PRL or maybe even Nature Communications (in both cases after substantial revisions), the effect is not novel and the model appears to be wrong.

An important aspect of the analysis is the peak counts vs. power (Fig. 3c). A conventional decay curve (Fig. 3b) has the advantage that it can accurately probe the dynamics of an emitter regardless of the radiation pattern, but when only the peak is extracted and plotted, its significance is not clear. For example, the three regimes might be expected for an ensemble of emitters for different reasons than suggested by the authors: I: Linear increase with power; II: Saturation of the emitters (which typically causes the peak value to drop); III: another linear increase with power as many-particle and/or phonon-dressed states are populated. Can the authors rule out such an interpretation?

On a related note about Fig. 3b, the radiation pattern of the ensemble should be expected to depend on power in the superradiant regime, and in that case the experimental geometry/numerical aperture would play a central and change the collected peak counts as a function of power. Can the authors rule this out as well?

The fitting with stretched exponentials appears purely phenomenological, and its use is not convincing. This fit has seven free parameters, and the resulting fit parameters (Fig. 3d) are correlated, which indicates overfitting, i.e., the fitting parameters may not have any simple physical significance. The meaning of fit parameters can only be established using a microscopic model, which is absent.

Reproducibility: The authors do not comment on reproducibility but at least one additional data set for the reflection measured on another sample should be presented in the supplementary.

Minor points:

The illustrations in Fig. 2a, 3f, and 4a are lacking labels on the axes in some cases. While this can be understood in some cases, the “pictogram”-style representation of the Dicke ladder and other states offers very little explanation for a reader beyond a narrow expert community.

The inset in Fig. 4c lacks tick labels.

For Extended data Fig. 9c, the power should be stated.

Author Rebuttals to Initial Comments:

Referee report for “Many-body cavity quantum electrodynamics with driven inhomogeneous emitters”, Manuscript # 2022-08-12465

We thank the referees for carefully reading our manuscript. Here we provide our point by point reply.

Referee #1 (Remarks to the Author):

The paper by Lei et. Al. reports a new cavity QED phenomenon that is unique to inhomogenously broadened ensembles. In this effect, a resonant laser that strongly drives the systems induces an atomic coherence, leading to destructive interference which results in a narrow transparency window. By measuring the decay rate of the atomic ensemble at different excitation powers, the authors observe a transition from super-radiant emission to increased contribution from the sub-radiant Hilbert space. This new phenomenon provides a new tool to generate coherent radiative effects in an inhomogeneous atomic medium. I recommend publication once the following points are clarified.

We thank the referee for the positive feedback about this work and the expertise and attention to detail they provided in their review. We have responded to each point below.

1) Based on the intuitive explanation the authors provide, the sharp dip observed in Fig. 2 results from a combination saturable absorption for on-resonance ions, plus destructive interference for the off-resonant ensemble. Both of these effects, however, also exist when the laser is detuned from the peak of the atomic absorption. The destructive interference is not perfect in this case, but should still be present because there is significant atomic population on both the red and blue shifted ions relative to the laser. I would thus expect a significant modification of the reflectivity even off resonance. Why is the effect only present right at the resonant condition?

We thank the reviewer for this insightful question. It is true that there is destructive interference between the red and blue shifted ions relative to the laser, even when the laser is not at the center. However, this cancellation is no longer perfect when the laser is even slightly off the center. Then the question becomes, how much can this slight imperfection raise the cavity reflectivity? The answer to this is related to our ensemble cooperativity; the higher the cooperativity, the stronger the coupling and thus even a small number of ions that don’t cancel can lead to high reflectivity. This is also an intuitive reason why the CIT width is inversely proportional to the cooperativity, as cooperativity gets higher, there is a higher sensitivity to the imperfect cancellation of detuned ions near the center.

To clarify this, we have added to following sentence to line 171:
“*Intuitively, higher cooperativity leads to a larger contribution towards DIR for even a small number of imbalanced ions, effectively increasing the sensitivity to the imbalance near the ensemble center, and narrowing the CIT.*”

2) The authors indicate that the cavity mediates direct photon exchange between the different atoms. But in Eq. S17, the authors assume the that the = <\sigma>. This approximation appears to throw out direct exchange of excitation between ions. So is such direct exchange needed to observe CIT?

It is indeed a good question about what physical mechanism is responsible for the CIT. There are several different possible mechanisms of “direct exchange of excitation between ions” in our system:

1. **Coherent ion-ion interaction** such as electric/magnetic dipole-dipole interaction (flip-flop interaction): this is small for our system, and is neglected.
2. **Coherent photon mediated interaction**: this refers to the exchange of virtual photons between ions, which appears in the first term in eq. S51. Since we work in the resonant cavity regime, or $\Delta_c = 0$, this is not considered in the model, but presents an interesting prospect for future investigations (please see our response to reviewer #2).
3. **Incoherent photon mediated interaction** : this refers to the exchange of real photons between ions mediated by cavity dissipation, and it is not ignored in our modeling of CIT and super- and sub-radiance dynamics. In fact, this is the key to observing CIT as we will elaborate more in the following.

This incoherent interaction between ions through the cavity field influence the CIT as the way shown in $< \sigma_j^- >= \frac{ig_i}{i\Delta_i+\gamma} < \sigma_j^z >< a >$ (Eq. S17), where the cavity field can be rewritten as $< a >= \frac{2}{\kappa}\left(- i \sum_{k=1}^{N} g_k < \sigma_k^- >\right) - \sqrt{\mu}$. This equation indicates two aspects of CIT: First, the phase of symmetrically detuned ions can cancel each other since every ion is interacting with the same cavity field $< a >$. Second, the contribution of imbalanced ions will be strengthened by g^2 to the cavity field. These lead to a narrow CIT in the center of the inhomogeneous line. In that sense, this incoherent ion-ion interaction mediated by cavity dissipation is the underlying mechanism of CIT. Note that in Eq. S17 a mean-field approximation is made to factorize the cavity field and atomic operators. The details of the validity and meaning of the mean-field approximation made have been added to Supplementary section 2.5.4.

3) The theoretical model introduce by the authors to explain the dynamics of the system is a bit unconvincing. The authors model six atoms resonant with the cavity. That seems like a very different system than what they actually have. Is there a way to improve this model?

To clarify, the six-ion model is used more of as a minimal toy model to give an intuitive picture of our understanding of how we traverse the Dicke space, as we can draw a relatively small Dicke space, in order to show what parts of the Dicke space each power regime occupies. We have performed more thorough modeling in the Main text Fig. 3e and Supplementary Fig. S6, where we make use of permutational invariance to simulate a larger number of identical ions (Supplementary section 3.3). We found that there is not a large difference in the qualitative behavior between small and large numbers of ions (Fig. S6). Additionally, in Fig. 3e, the simulation is with 569 total ions, comprising 91 incoherently summed subensembles of various ion numbers and frequencies forming a Lorentzian distribution, which qualitatively captures the behavior of the peak counts. Extended Data Fig. 8 provides our justification for why we believe the simulation method in Fig. 3e is valid.

We make these approximations because each transition (A, E or I) in our system includes ~2e5 ions with a frequency inhomogeneous broadening of 150 MHz. This complexity of the system makes it infeasible to simulate the full quantum system, so it is necessary to make some approximations. What types of approximations are made depend on what physics we are looking at and what quantities we are measuring. In the superradiance/subradiance dynamics, we include the correlations between different ions, which limits the number of ions we can simulate. For example, if we were in the low-excitation regime (as some other works operate in), we may truncate the Hilbert space to allow simulation of more atoms. However, our exploration of the full 2^N Dicke space, due to decoherence and dissipation effects, does not allow for this.

4) It would be instructive to see what happens to the dynamics of the system when the laser is detuned from the minimum peak.

The detuned case has been investigated in Extended Data Fig. 9, which shows the peak counts as a function of laser detuning and power. The central idea here is that the CIT acts as a filter function, where a detuned laser is met with higher cavity reflection (compared to a resonant laser), which means less light enters the cavity. Consequently, the S-curve shifts towards higher powers, as expected, and the detuned dynamics reveals qualitatively the same CIT shape.

Referee #2 (Remarks to the Author):

I find the manuscript by Lei et al. very well written, and clear in most of its parts. From what I read, the key findings of the paper are a new collectively induced transparency and its implications for many-body sub- and super-radiance. This is certainly interesting but a lot of the anticipated applications and impact of these findings are just stated at page 1, and never elaborated in the main text. So I will assume that CIT and the results centered around page 5, are the key findings of this work.
Now, I appreciate that a solid-state implementation of cQED problems offer new perspectives and certainly the results contained in the manuscript are worth of publication in a very good journal, but I don't see how they scale up to 'Nature' level.

We thank the reviewer for their appreciation of our work and the expertise and attention to detail they provided in their review. We have expanded more on the concrete impact and anticipated applications of our findings by providing additional experimental data as well as theoretical proposals below.

After all, we are still talking of a model (eq 2) which is extremely well studied in quantum optics and which is shown here to enter a new interesting regime at strong drive.

We would like to emphasize that our system has the following properties. (1) It contains millions of inhomogeneous emitters (2) The emitters are coupled with high cooperativity to a cavity, (3) The entire system is subject to a strong drive (where strong drive here means exceeding weak-excitation limit, which we elaborate on more below). Such a regime of the Tavis-Cummings model has not been well investigated theoretically nor experimentally, evident from the fact that we discover CIT for the first time. We see that the referee has alluded that this new regime shows only incremental progress, however, we would like to explain why this regime containing all the above conditions 1-3 is both fundamentally and practically important to understand, especially as we see unexpected, novel phenomena.

First for conditions 1 and 2, achieving high optical cooperativity in a large inhomogeneous ensemble is of high importance since high bandwidth quantum memories and transducers are based on these systems (Nat. Comm. 13, 6438 (2022), Adv. Quantum Technol. 3, 1900077 (2020), Quantum Sci. Technol. 5, 020501 (2020)). Such a high bandwidth is necessary for frequency multiplexing in quantum memories and for high speed conversion in quantum transducers, and is a figure of merit just as important as the efficiency for these devices. Achieving an optical cooperativity comparable to the state-of-art experiments for only a few solid-state emitters (Science 362, 6415 pg 662-665 (2018)) but now with millions of emitters has brought us into this new regime, where we can observe a new transparency and collective/many-body dynamics due to the combination of inhomogeneity and high cooperativity.

In regards to condition 3, a strong drive (exceeding the weak-excitation limit) is necessary for the operation of the aforementioned quantum technologies. For example, quantum transducers will always require a strong optical pump for efficient conversion, where the efficiency scales quadratically with the pump power. And perhaps more importantly, a strong drive fundamentally changes the physics of the system, by increasing the complexity of the model by expanding the Hilbert space beyond the more well-studied low excitation regime and allowing these novel phenomena to occur, which we must first fully understand.

I see this punchline as incremental: (1) there is nothing truly many bodysh here (the model is still collective, although I appreciate the rich decay dynamics they find outside the Dicke manifolds); (2) as I said, prospects for outreach of their findings is just anticipated, not truly worked out; (3) the actual merit, in my opinion, is indeed the new experimental setup the authors realize and fit amazingly well.

We thank the reviewer for the critical assessment of our work, and below we provide our detailed responses to these questions one by one.

Regarding the many-body aspect of our system: we concede that the definition of many-body is at times contentious and not strictly defined. We argue that strictly speaking, the Tavis-Cummings model is intrinsically many-body. However, it is true that by making some approximations or truncating the Hilbert space, one can consider the system to be collective. Then, we believe the most appropriate question we can ask ourselves is whether the phenomena we observe require many-body modeling or can be simplified as collective. Hence, below we provide a more thorough explanation on why we define our system as many-body. However, we have changed our wording throughout the manuscript to be more conservative, especially in the introduction.

For CIT: since we are using the mean-field approximation and factorizing the ion-ion correlation term (please see new Supplementary section 2.5.4), we agree that one might argue that this is not a "quantum many-body effect". However, we do want to stress that CIT cannot be seen if there is just a single large collective spin coupled to the cavity field. An inhomogeneous ensemble of ions with different frequency detuning is needed and also the key to observe CIT, as we demonstrate in Supplementary section II. Still, due to the application of the mean-field approximation, we consider this to be "collective", hence the name.

For superradiance and subradiance: when we are using Dicke space, it is more clear what is many-body and what is not. For example, let us only consider the first column in the Dicke space where total spin J=N+1 (i.e. the superradiant ladder). In this case, one can truncate the Hilbert space, and in fact can use a collective model where the system can be simplified with a single large spin with total spin J. However, when we start to probe the subspaces with different J (in our system total spin J is not conserved in the existence of inhomogeneity and decoherence), we must consider the full 2^N Hilbert space, and the problem becomes many-body. The experiment and modeling here demonstrate an access to a much broader part of the many-body Hilbert space, providing rich dynamics in our open quantum many-body system.

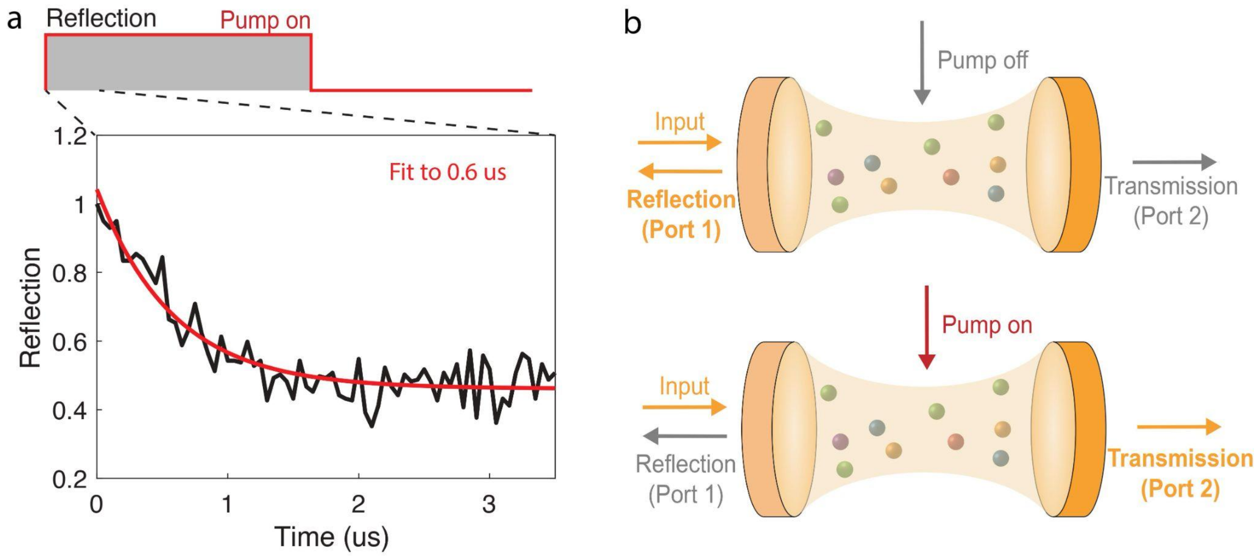
Additionally, we would like to emphasize that we can in principle activate a direct coherent ion-ion interaction in the Hamiltonian by detuning the cavity in order to further explore genuine many-body quantum dynamics in our system. . We elaborate on this more below.

Regarding the prospects of our work: To elucidate the potential applications of our new experimental findings, we have added the following new experimental results, as well as concrete future directions, one of which was inspired by one of the referee's later comments:

New experimental results:

1. One potential application for CIT is a fast, two-port optical switch. We provide a schematic for this in the newly added Extended Data Fig. 10b. Essentially, a transverse pump can act as the on/off switch, and the signal coupled to the cavity can either get reflected (port 1) or transmitted (port 2) depending on the state of the transverse pump. The speed of the switch depends on the CIT dynamics, which we have studied in Extended Data Fig. 10a, where we find a switching time of 0.6 us. While this particular device has not been optimized for this purpose, the switching time, contrast, and bandwidth can be improved with sufficient fabrication and material considerations. In particular, the switching time is determined by how fast the on-resonance ions can be driven to steady-state, evident from the decrease in switching time with driving power. Hence increasing the coupling rate g should shorten the switching time. Considering the fact that we have many ions coupled to the cavity with a relatively small g factor but still contribute to DIT, this can be done by engineering ions at maximum E field location in the cavity. Other ways to improve the response time include choosing faster emitters, fabricating cavities with higher Q and smaller mode volumes. The bandwidth can be narrowed (if a narrower optical filter is desired) by increasing cooperativity as mentioned in the text, where the fundamental limit is the atomic homogeneous linewidth. There is no fundamental limit to the contrast, in principle unit contrast is achievable if the cavity is critically coupled. Our present cavity is

under-coupled, leading to the lower contrast.



In addition to the Extended Data figure and supplementary, we have also added the following sentence to the main text to highlight this prospect (line 359):
"*In particular, we demonstrate a proof-of-principle optical switch using CIT (Extended Data Fig. 10) and posit that with further optimization a fast, high contrast two-port optical switch can be realized*."

2. Insensitivity of CIT frequency with cavity detuning. We have derived the relation between cavity detuning Δ_c and the location of the CIT as $\delta_{CIT} = -\frac{\Delta_c}{\kappa}\Delta_{CIT}$, and experimentally demonstrate this for varying cavity detunings. We find that the absolute shift of the CIT frequency is reduced by a factor $\frac{\Delta_c}{\kappa}$, meaning the fluctuation of CIT due to the fluctuation of the cavity frequency is reduced by this factor, in our case by about 3 orders of magnitude. This makes CIT a more stable frequency reference than the original bare cavity. We have added this to Supplementary section 2.5.5, along with Fig. S4.

Concrete prospects:

1. Engineering photon-mediated spin-spin interactions. Our present experiment operates in the regime where the cavity is approximately resonant with the ions, such that Δ_c is negligible. However, we can detune our cavity such that Δ_c becomes non-negligible, and in that case we refer to Eq. S51 in our Supplementary, where the first term in the Hamiltonian, $\frac{\Delta_c}{(\kappa/2)^2+\Delta_c^2}\sum_j^N g_j\sigma_j^+\sum_k^N g_k\sigma_k^+$ will become relevant. To see if this is numerically feasible, we first note that we are in the dissipative limit, where the dominant rate is the collective dissipation term that is proportional to $\frac{\kappa}{(\kappa/2)^2+\Delta_c^2}\sum_{j,k}^N g_j g_k$. Hence comparing the two terms, we can see that if $\Delta_c \sim \kappa$, these two rates become comparable and could give rise to interesting physics. Such phenomena has been explored, as the referee has mentioned, in cold atoms in PRL 126, 173601 (2021) and Science 361, 6399 pg 259-262 (2018).

As per reviewer's query, we have added to following to the main text to highlight the prospect of an investigation into the coherent photon-mediated spin exchange interactions in a solid-state system:

"While here we have focused on the dissipative regime by operating with the cavity resonant with the ions, we may consider a detuned cavity, where an additional spin-spin interaction term must be considered in the Hamiltonian. Such a regime may open possibilities for studying coherent spin-spin interactions and for use as quantum simulators \cite{Norcia2018,Swan2021} in a solid-state platform."

2. On-chip narrow frequency reference. The best fabricated on-chip cavities are Fabry-Perot or ring resonator cavities in SiN. These can be used for example for frequency referencing, detection, optical filtering, and lasers. CIT is a ubiquitous phenomenon for producing a resonance feature with properties similar to that of a cavity (including a phase shift around the resonance), but with a linewidth narrower than the original cavity (in our case, three orders of magnitude). We propose that CIT can be used to narrow the cavity linewidth by orders of magnitude by coupling an inhomogeneous ensemble of coherent emitters. To outline this, we have added Supplementary section 2.5.5, which suggests the validity of CIT with higher-Q cavities.

3. This observation of superradiance in this rare-earth ensemble directly contributes to the development of a solid-state superradiant laser (arXiv:2003.14281). Specifically, the prepint theoretically propose a sub-Hz linewidth laser using a cavity-coupled rare-earth ion-doped crystal, and claim that the desirable traits of these rare-earth ions (good coherence, the ability to make a dense ensemble while maintaining MHz-level inhomogeneous linewidths) make it an ideal candidate for such a laser.

A few points of confusion through the text:

— why do the authors say the most cavity QED is done with small number of identical emitters? I would say we have a lot of works and experiments with several constituents in a collective regime

We thank the referee for pointing this out, and we agree that there have been some recent experiments investigating cavity QED with many particles. As per referee's suggestion, we have reworded a sentence in the introduction to (line 61):

"While much of these early works in cQED have focused on one or a few cavity-coupled emitters \cite{...}, there has been progress in the study of many-body cQED with a macroscopic ensemble of emitters, [...] \cite{...}."

— the definition of strong drive should be spelled out clearer and more prominently, I had to navigate through the Appendix to understand what they were talking about. It is also not completely transparent how this better achievable in this platform wrt to pre-existing ones

The strong drive is in contrast to the weak-excitation limit where the ions are all assumed to be approximately in the ground state, that is, $< \sigma_z > \approx -1$ for each ion. While we have explained it in parts throughout the text, we have added a sentence to make this fact more prominent in the introduction (line 80):

"[...] subjected to a strong driving field such that the resonant ions are excited."

This strong drive has certainly been achievable in some prior platforms. However, the strong drive may not have been emphasized or studied in some of these other platforms due to the absence of exotic phenomena, since they either lack the inhomogeneity, high cooperativity, or both. We can for example consider prior works dealing with dipole-induced transparency (Phys. Rev. Lett. 96, 153601 (2006) and all related future works), where there is negligible inhomogeneity. In this case, entering the strong drive simply saturates the ions such that the dipole-induced transparency disappears, and there is no CIT. We show that in our system, the strong drive gives rise to novel transparency and collective/many-body dynamics due to the combination of inhomogeneity, high cooperativity, and strong drive, all of which are necessary.

And some further speculations:

— the model the authors study is regarded as a way to simulate BCS superconductors in and out of equilibrium. See for instance A. M. Rey's group papers (building on almost 15 years of previous literature in the cond mat/ math phys community). What can the authors infer from their results that connect to this physics? Probably there is no direct relation since they work in the strong dissipative limit, but it would be nice to see a few lines of comment on this.

We thank the referee for mentioning these works, and inspiring another very interesting experimental direction. We have responded to this point earlier in this response, under "Concrete prospects (1)".

Referee #3 (Remarks to the Author):

The manuscript by Lei et al. reports on experiments on collective nonlinear interactions in an ensemble of solid-state emitters in an optical cavity. The central claim is the observation of a transparency effect in the reflection of the ensemble, which the authors coin “collectively induced transparency.” The experiment is backed by a theoretical model building on an extension of the Tavis-Cummings Hamiltonian. The manuscript is generally well-written although some important details are dealt with in a somewhat cavalier manner, in particular data analysis and theory. However, I remain concerned about both novelty and correctness of the claims as outlined below.

We thank the referee for their feedback on our manuscript. We would like to mention here that our modeling of CIT is based on the Tavis-Cummings Hamiltonian with the well-known mean-field approximation (please see the newly added Supplementary section 2.5.4 for the justification) and the dynamics is using the exact Tavis-Cummings Hamiltonian. We maintain that our experimental discovery and theoretical analysis of CIT is novel and correct, and we have outlined point-by-point responses below.

Novelty and correctness: The authors should comment on the relation to Blaha et al., PHYSICAL REVIEW A 105, 013719 (2022), which is a surprising omission in the reference list. In particular Fig. 4c in Blaha et al., where a similar transparency is predicted and measured but explained by deficiencies of the Tavis-Cummings model rather than inhomogeneous broadening.

After careful reading of this work, we’ve determined that the feature seen by Blaha et al., in Fig. 4c (and throughout the paper) is not a transparency, nor CIT. The lines in Fig. 4c indicate the positions of the resonances. A better comparison to our Fig. 2 is by looking at Blaha et al. Fig. 3b, which directly plots the reflection spectrum. As we can see from their Fig. 3b, the additional resonances predicted in Fig. 4c will lead to a plateau feature in the reflection. As they explain in the caption, they call this an additional resonance produced due to “the fact that light undergoes multiple 2π phase shifts in a single resonator roundtrip”, akin to a resonance produced by a Fabry-Perot resonator. Essentially, in the system(s) investigated in Blaha et al., the optical resonator length is long and the atoms are coupled to this resonator in such a way that the effective interaction length is very long. In this case, before the light can make a round-trip around the resonator, the light’s phase is modulated enough such that additional resonances are created.

Meanwhile, CIT is a transparency that appears in Dipole-induced reflection (DIR). As we outlined in our work, CIT occurs due to the combination of the destructive interference of off-resonance atoms, and the saturation of the on-resonance atoms, which suppresses the DIR at the center of our atomic inhomogeneous distribution. We have shown CIT to be accurately predicted by the standard Tavis-Cummings Hamiltonian, with justified use of the mean-field approximation (please see new Supplementary section 2.5.4).

We would like to summarize the differences between Blaha et al. and our systems in terms of phenomenon, mechanism, and system:

1. Phenomenon: Blaha et al. predicts new “resonances” (vertical lines in the Fig. 3b), which leads to a “plateau in the reflection” as they noted in the caption of Fig. 3b. Instead what we observe with CIT is a singular narrow dip in the reflection within DIR. The resonance in Blaha et al. and CIT dip shown in the reflection spectrum are two totally different phenomena.
2. Mechanism: Blaha et al. considers multiple identical ions interacting with the cavity field sequentially, due to the long cavity and effective interaction length. CIT is a combination of off-resonance ions phase cancellation and saturation of resonance ions.
3. System: Blaha et al. considers systems with multiple identical atoms in the weak excitation regime. That is, there is no atomic inhomogeneity and they are all assumed to be approximately in the ground state. Meanwhile, CIT occurs in systems with an inhomogeneously broadened ensemble, and the ions must exceed the weak excitation regime. Additionally, Blaha et al. considers multiple cavity modes, while in our case there is only a single mode that plays a role.

In the table below, we summarize the critical parameter differences between our work and Blaha et. al.

	Atomic ensemble linewidth	Total coupling rate g_N	FSR	Cavity length	Cavity linewidth	Number of cavity modes
Our work (CIT)	150 MHz	2π x 4.5 GHz	25 THz	3 microns	44 GHz	1
Blaha et al.	2.61 MHz	2π x 8.74 MHz	7.1 MHz	30 meters	0.5 MHz	>5

To be concrete, Blaha et al. explains the criteria for the validity of the Tavis-Cummings model of which the most important is (Eq. (9) in Blaha et al.): FSR >> g_N^2/gamma_l. Now, taking an average decay rate of 1/(590μs) from the author's own work (Ref. 41), assuming 50 participating ions (line 249), and g = 2*pi*35e6 Hz (line 655), estimating the FSR from the authors own work on the same device (Ref. 38, Fig. 3d), we arrive at

g_N^2/gamma_l = (sqrt(50)*2*pi*35e6Hz)^2/(1/(590μs)) ~ 1.4e15 Hz

and

FSR = (c/(1527e-9m)-c/(1561e-9m)) ~ 4.3e12 Hz.

Unless something is wrong in the numbers used here, the TC-model is therefore not valid in the regime of the present experiment.

First, we must consider the fact that Eq. 9 in Blaha et al. is based on the system with N identical atoms without any frequency detuning. If we extend Eq. 9 in Blaha et al. to an inhomogeneous broadening ensemble, the right hand side should be replaced by the ensemble absorption rate W (Diniz et al. PRA 84, 063810 (2011)). One straightforward way to see this is to refer back to Blaha et al., Eq. A5-A8 in the Appendix, where they derive the cavity-induced emitter excitation probability, which is the right-hand side of Eq. 9. Looking at Eq. A8, they neglect the term Δ_a (corresponding to Δ_j in our notation), which is the emitter-probe detuning, as they only consider identical atoms. However, due to the non-negligible ion inhomogeneity in our system, we must consider this term, and in fact dominates over the homogeneous linewidth γ. The physical meaning of Eq.9 is that the cavity free-spectral range (FSR) is much larger than the atomic absorption rate: $FSR >> \left|\sum_{j=1}^{N} \frac{g_j^2}{\gamma + i\Delta_j}\right|$ where γ is the homogeneous linewidth and Δ_j and g_j is the ion detuning and coupling rate, respectively.

Second, the numbers used by the reviewer are incorrect. The calculation of the FSR here erroneously uses noise captured in the spectrometer data from Ref. 38, Fig. 3d. Our device is a confined photonic crystal cavity, and in our small mode volume design where the cavity dimensions are on the order of a wavelength, with finite bandwidth mirrors, we only have one cavity mode at each polarization, TE and TM (and in some designs, only one of these polarizations are allowed). Fig. 3d does indeed show two resonances, but these are the TE and TM modes, hence cannot be used in the calculation for FSR. Additionally, the referee is referring to a device created for Erbium ions at 1530 nm, rather than for the Yb ions used in this current work at 980 nm. These two devices differ in design, polarization, and material.

Instead, to correctly calculate the FSR of our system, we use the formula for a Fabry-Perot cavity:
FSR = c/(2*n*L) where n=2 is the refractive index of our material, and L=3um is the length of our cavity, extracted from simulations. This comes out to:

$$FSR = 2\pi * c/(2 * 2 * 3\, um) = 2\pi * 25\, THz$$

For the right hand side of the inequality, $|W| = \left|\sum_{j=1}^{N} \frac{g_j^2}{\gamma + i\Delta_j}\right| = \frac{2\sum_{j=1}^{N} g_j^2}{\Delta_{inh}} = \frac{2g_N^2}{\Delta_{inh}}$, where g_N is the total coupling rate of all the ions. We have three transitions A, E, I, and I is the most well-coupled one since it has double the population. $g_{N,I} = 2\pi * 6.3\ GHz$, which gives us

$$|W| = \frac{2g_{N,I}^2}{\Delta_{inh}} = 2\pi * 0.54\, THz$$

Here, for the total coupling strength g_N^2, we have used $2\pi * 6.3\ GHz$ for the I transition (for E and A, it should be $2\pi * 4.5\ GHz$, so even smaller) which takes into account the inhomogeneity of the cavity field and the total number of ions, rather than just 50 (50 is our estimate for the ions participating in superradiance, nothing to do with CIT). We see that from the above calculations, the inequality is satisfied by about two orders of magnitude: $25\, THz \gg 0.54\, THz$, hence the Tavis-Cummings model is valid for our system.

It could be that the ad-hoc amendments to the TC-model made by the authors eventually can mimic the more rigorous approach by Blaha et al., but in any case, the novelty of the claims by the authors is elusive because Blaha et al. already report a collectively induced transparency (without naming it so).

While the experimental data reported by the authors is clearer than the data in Blaha et al. and this should certainly merit publication in a reputable journal such as PRL or maybe even Nature Communications (in both cases after substantial revisions), the effect is not novel and the model appears to be wrong.

The approximations made to the TC-model in the derivation of CIT is the very standard mean-field approximation, and we have added the justification of its use in Supplementary section 2.5.4. Furthermore, the assumptions made in Eq. 13a-c is purely to derive an approximate analytical expression for the CIT width and depth that allow us to gain some physical intuition as to what's happening. The numerical modeling (and its results in Fig. 2c) do not make any of the assumptions.

We also want to reemphasize that Blaha et al., does not see a transparency. We would describe their findings more as a deviation from the TC model due to the violation of the above inequality Eq. 8-9. Our approach and Blaha et al. are dealing with a different platform in a very different regime. As we have already summarized the difference between our work and Blaha et al., we don't agree that Blaha et al. influences the accuracy nor the novelty of our work.

Nevertheless, we find that the work of Blaha et al. is related to novel effects found in collective atomic systems, and as such have added that as a reference in the introduction in line 66. Additionally, we have summarized our calculations above in the Supplementary as proof of the validity of our use of the Tavis-Cummings model for our system.

An important aspect of the analysis is the peak counts vs. power (Fig. 3c). A conventional decay curve (Fig. 3b) has the advantage that it can accurately probe the dynamics of an emitter regardless of the radiation pattern, but when only the peak is extracted and plotted, its significance is not clear. For example, the three regimes might be expected for an ensemble of emitters for different reasons than suggested by the authors: I: Linear increase with power; II: Saturation of the emitters (which typically causes the peak value to drop); III: another linear increase with power as many-particle and/or phonon-dressed states are populated. Can the authors rule out such an interpretation?

Indeed we agree that just having Fig. 3c would not be a convincing argument for our analysis. However, as the reviewer has pointed out, we pair this with decay curves that show decays that are 1. Faster than Purcell-enhanced decay and 2. Slower than Purcell-enhanced decay, and modeling using the master equation. Hence the presence of enhanced/suppressed decay rates, in conjunction with the modeling and additional evidence presented in Fig. 4, gives us confidence in our interpretation.

More specifically, in regards to the alternative interpretation the referee has provided, in regime II if the emitters are saturated, we should see the peak value saturate, instead of drop, as we are plotting the pure power on the x-axis. In regime III, we believe there should be no phonon-dressed states at our dilution temperatures. We believe regime III to be the excitation of off-resonant ions.

Another question perhaps related to this comment may be: what is the significance of the peak counts and why do we pay attention to this particular quantity? We have outlined this in detail in the Methods section “Master equation simulation of dynamics”. To summarize, the peak counts give an excellent quantity that can help determine what state the system has been prepared by the coherent drive. And further by breaking the measured quantity down as done in Eq. 17 and Extended Data Fig. 7, we can get valuable physical intuition as to why the peak counts increase then decrease with power. This is because the second “correlation” term increases in the presence of superradiance, then decreases as we diffuse out to the subradiance subspace. This “correlation” term is always competing with the first term that represents the individual ion contributions, which is monotonically increasing then saturates (blue curve in Extended Data Fig. 7a). For most systems, this individual term dominates over the correlation term, thus usually just saturates at larger powers. However, due to the large cooperativity of our system, we are able to build up a large amount of superradiance, hence the S-shape curve of the peak counts.

On a related note about Fig. 3b, the radiation pattern of the ensemble should be expected to depend on power in the superradiant regime, and in that case the experimental geometry/numerical aperture would play a central and change the collected peak counts as a function of power. Can the authors rule this out as well?

We agree that what the referee described is the case for free-space superradiance. However, we use a photonic crystal cavity, in which the superradiant emission is all coupled into the cavity mode due to spontaneous emission rate enhancement. As such, the radiation pattern of superradiance should remain constant, and only through our single mode cavity. As such, we rule out this possibility.

The fitting with stretched exponentials appears purely phenomenological, and its use is not convincing. This fit has seven free parameters, and the resulting fit parameters (Fig. 3d) are correlated, which indicates overfitting, i.e., the fitting parameters may not have any simple physical significance. The meaning of fit parameters can only be established using a microscopic model, which is absent.

The stretched exponential fit is a standard model used for decays with multiple different time scales (see Chem. Phys. Lett. 120 4-5, 1985 pg 455-459; Physica A 197 4, 1993 pg 569-582). A similar treatment of the fitting function is done in New J. Phys. 24 053039 (2022), where they have a fast and a slow decay, and we have implemented the stretched exponential factor due to the exploration of the full Dicke space leading to varying decay channels and rates.

We’d like to refer to Fig. S8 and section 4 of the Supplementary, where we list a more detailed explanation of the rationale behind the fit function used. In particular, we start with a very simple model of simple exponentials, which we find does not fit the data well, and gradually increase the complexity of our fit function, finally landing on the stretched exponential fit function that fits our data the best.

More crucially, we maintain that the parameters are physically significant. While there are 7 free parameters, all of them are strictly bounded. For example, the background b is very tightly bounded by the calibrated background dark counts in our experiment, which essentially eliminates that parameter. A_1 is simply obtained from the max counts of the decay, so it’s not a fit parameter, as we can see it follows the peak counts plot. A_2 is also strictly bounded around the peak of the slow decay, which is clear in the regime II curve in Fig. 3b. The real information extracted is then limited to the decay rates and the stretch exponentials, which are unbounded in order to remove any bias from our expectations.

Reproducibility: The authors do not comment on reproducibility but at least one additional data set for the reflection measured on another sample should be presented in the supplementary.

The observed effects are not an anomaly, and should be reproducible. As the referee suggested, to provide information on the reproducibility of our findings, we have added a sentence to the revised manuscript (line 344): "We confirmed that all of the experimental findings for CIT and sub- and super-radiance are reproducible and consistent with our theoretical prediction, independent of the choice of the optical emission lines between the A, E, and I transitions (Ext. Data. Fig 3)."

Minor points:

The illustrations in Fig. 2a, 3f, and 4a are lacking labels on the axes in some cases. While this can be understood in some cases, the “pictogram”-style representation of the Dicke ladder and other states offers very little explanation for a reader beyond a narrow expert community.

Fig. 2a shows the axes and labels on the top plot; we believe that putting it on the other plots would be repetitive. We agree that it would be more clear to include axes labels for the Dicke spacing on Fig. 3f, we have included axes labels in Fig. 3f. We also agree that the axes labels are missing on Fig. 4a, we have added axes labels to Fig. 4a.

The inset in Fig. 4c lacks tick labels.
We thank the reviewer for pointing this out, the tick labels have been added in the revision.

For Extended data Fig. 9c, the power should be stated.

In Extended data Fig. 9c, shown are the local maximum values of peak counts as varying the power, extracted at each detuning. The power for each point can be seen from Extended Data Fig. 9a, from the sideways “V” shape. Essentially, we plot the trend of the peak count value at the transition between regime I and II, which is the red dashed line in Extended Data. Fig. 9b, as a function of laser detuning.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #2 (Remarks to the Author):

The authors have done an excellent job at replying to all my issues and concerns. I reconsider my position and would recommend the work for publication provided the authors do some extra work at including most of my perplexities. They reply very carefully but edit the paper very little, in terms of motivation, addressing broad and unexperienced readers, etc

I admit that some of my questions were coming from lack of knowledge on state-of-art quantum optics integrated with solid state, but I wonder how many readers the authors think will be so updated to appreciate them immediately. The paper is clearly still very specialized and I believe that for a Nature publication more effort should be done to welcome a broad audience. I'd like therefore to have a look at the manuscript again, and once I feel this issue is addressed, I would be happy to finalize the work for publication.

Referee #3 (Remarks to the Author):

The authors present a detailed response including a clarification of the differences between their work and Blaha et el., which I find clear and convincing, and the explanations resolve my main concerns about novelty. The associated revisions of the supplementary information are appropriate.

Regarding correctness, I am not convinced by all details of the arguments regarding the analysis of the power dependence (Fig. 3). For example, saturation of quantum emitters typically implies a drop in intensity (see, e.g., Fischer et al., Applied Physics B 117, 797–801 (2014); similar effects are found for other quantum emitters), and the authors are not right that this can be ruled out. Also, while I agree that various fitting functions such as stretched exponentials are used throughout the literature, I do not agree that they are always useful. It is not surprising that the fit improves when more parameters are added, and the question is what the parameters mean. Having said that, saturation effects would not explain the beyond-Purcell enhancement and I find the author’s clarification of the interpretation of the peak counts convincing, which is the most important.

In summary, the response and revisions convince me that the authors have observed the signature of a new phenomenon, and I recommend publication. The authors may want to ensure that the minor points mentioned above are discussed in sufficient detail in manuscript as I find that some of the points in their response on the questions about the data analysis and fitting function would be valuable additions to the supplementary information.

Author Rebuttals to First Revision:

Referee report for “Many-body cavity quantum electrodynamics with driven inhomogeneous emitters”,

Manuscript # 2022-08-12465B.

Referee #2 (Remarks to the Author):

The authors have done an excellent job at replying to all my issues and concerns. I reconsider my position and would recommend the work for publication provided the authors do some extra work at including most of my perplexities. They reply very carefully but edit the paper very little, in terms of motivation, addressing broad and unexperienced readers, etc

I admit that some of my questions were coming from lack of knowledge on state-of-art quantum optics integrated with solid state, but I wonder how many readers the authors think will be so updated to appreciate them immediately. The paper is clearly still very specialized and I believe that for a Nature publication more effort should be done to welcome a broad audience. I'd like therefore to have a look at the manuscript again, and once I feel this issue is addressed, I would be happy to finalize the work for publication.

We thank Referee #2 for their reconsideration, and the positive assessment of our work. In particular, we appreciate the referee’s suggestion to better motivate our work to appeal to the broader audience of Nature. To address this, we have added the following to the introductory section on page 1:

In contrast to the previously more well-studied atomic gas systems (25,27), the solid-state implementation offers the added benefit of on-chip integration in quantum applications, such as high bandwidth quantum memories and transducers (33,34). Here, the high bandwidth is necessary for frequency multiplexing in memories and high speed conversion in transducers, and is achieved as a result of the natural spectral inhomogeneity of the solid-state emitters. In order for such devices to operate efficiently, one must engineer a system with high cooperativity, which is a dimensionless figure of merit that describes the ratio between the collective coupling strength of the cavity-emitter system to the dissipation and disorder. As improvements to material and device parameters are made towards increasing this cooperativity, it becomes critical to fully understand any associated cQED phenomena that may emerge.

Additionally, we have edited our wording throughout the entire paper to be in plainer language, such that the main text is easier to understand for a non-expert, and leaving details in the Supplementary for more expert readers. Please see the attached difference file for all the changes to the text.

We would like to once again thank Referee 2 for providing a number of critical comments. We sincerely hope that our work will stimulate further theoretical and experimental studies that will lead to the robust implementation of high-efficiency, high-bandwidth on-chip quantum devices exploiting cQED phenomena.

Referee #3 (Remarks to the Author):

The authors present a detailed response including a clarification of the differences between their work and Blaha et el., which I find clear and convincing, and the explanations resolve my main concerns about novelty. The associated revisions of the supplementary information are appropriate.

Regarding correctness, I am not convinced by all details of the arguments regarding the analysis of the power dependence (Fig. 3). For example, saturation of quantum emitters typically implies a drop in intensity (see, e.g., Fischer et al., Applied Physics B 117, 797–801 (2014); similar effects are found for other quantum emitters), and the authors are not right that this can be ruled out. Also, while I agree that various fitting functions such as stretched exponentials are used throughout the literature, I do not agree that they are always useful. It is not surprising that the fit improves when more parameters are added, and the question is what the parameters mean. Having said that, saturation effects would not explain the beyond-Purcell enhancement and I find the author's clarification of the interpretation of the peak counts convincing, which is the most important.

In summary, the response and revisions convince me that the authors have observed the signature of a new phenomenon, and I recommend publication. The authors may want to ensure that the minor points mentioned above are discussed in sufficient detail in manuscript as I find that some of the points in their response on the questions about the data analysis and fitting function would be valuable additions to the supplementary information.

We thank Referee #3 for raising several important points, and in particular, for their suggestion to provide more details about the data analysis in the manuscript. Below, we provide our responses to the referee's comments point-by-point.

First, as for the saturation of emitters, we agree with the referee that, on first glance, the nonlinear S-curve behavior in Fig. 3c looks similar to the known saturation phenomena from single emitter cases. However, we believe that the underlying mechanism for our observation with dense inhomogeneous emitters is fundamentally different from that for a single emitter for the following two reasons:

1. As the referee pointed out, the single-emitter saturation effect does not predict and cannot explain the subsequent emission dynamics exhibiting sub- and super-radiant decay rates beyond the single-emitter Purcell factor. The observed correlation between the nonlinear S-curve and resultant collective emission dynamics is more consistent with our model based on the many-body Dicke states.

2. We also would like to point out that the S-curve we see with the peak emission intensity is related to the excited state population of an atom, $(\langle \sigma^z \rangle + 1)/2$, while the single-emitter saturation effect is rather characterized by the coherence of an atom, $\langle \sigma^- \rangle$. As shown below, we note that these two observables show qualitatively different behaviors as a function of excitation power, which further lends support to our many-body model concerning the interplay between cavity photons and an ensemble of many emitters.

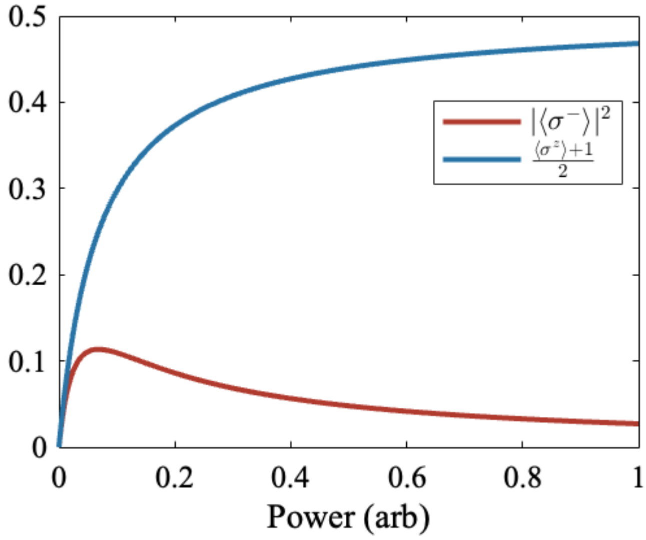


Figure: comparison between excited state population, $(\langle \sigma^z \rangle + 1)/2$, and the intensity of coherence, $|\langle \sigma^- \rangle|^2$, of a single atom as a function of excitation power.

As the referee advised, to elucidate the difference between the single-atom saturation phenomenon and our collective many-body dynamics, we have added the following to the Methods section under *Master Equation Simulation of Dynamics*:

We note that an increase then decrease of emission can be associated with the saturation of the coherence, also seen with just a single emitter. However, we find that the underlying mechanism for our observation with dense inhomogeneous emitters is fundamentally different from the above phenomenon (see Supplementary Information for details).

along with Supplementary 3.4 *Comparison to the Saturation Effect* for a detailed discussion.

Second, as for the fit function, we believe that the physical interpretation of Dicke superradiance and subradiance, each with multiple decay rates, provides the physical picture for the stretched bi-exponential fit. In particular, we want to clarify that we are not just naively adding an arbitrary number of parameters to get a good fit; rather, we also initially found it surprising and interesting to observe that the fit is poor with the simple exponential functions. However, we note that the stretched exponentials with one additional stretch factor () are commonly employed as a 'minimal' fit function when a decay profile is determined by the sum of multiple simple-exponential decays:

$$e^{-t^{\beta}} = \int_0^{\infty} du\rho(u)e^{-t/u}$$

where (u) is the probability distribution of simple exponential time constant u.

Hence, as we detailed in Supplementary 4 *Justification for photon fit function*, the decay dynamics inside the many-body Dicke manifold, where different state-dependent decay rates are present, justifies the use of the stretched exponential fit leading to the better agreement with the experimental data (see Ext. Data. Fig. 6).

To clarify this point, we have added a comment on our fit function based on a stretched exponential fit in the revised manuscript (Page 4):

To effectively capture the existence of multiple decay rates among the various Dicke states, as well as a clear separation between fast (superradiant) and slow (subradiant) decays, we employ a phenomenological stretched bi-exponential fit and extract the relevant fit parameters, which also clearly reveals the presence of the distinct three regimes discussed earlier (Fig.~3d, Methods).

In addition, as the referee requested, to help the readers better understand what different fit parameters mean and how experimental data are fitted, we have provided a more detailed fitting procedure in Supplementary 4 *Justification for photon fit function,* as attached below*:*

To capture both the multi-exponential fast and slow decay, we now attempt a fast stretched exponential + slow stretched exponential (Fig. S8c). In particular, the fit function is A1 exp[−(t/τ1)^x1] + A2 exp[−(t/τ2)^x2] + b, where there is a fast decay with subscript 1 and slower decay with subscript 2. We find that this fits the data well in all power regimes, and most importantly does so with fit parameters that match our qualitative expectations from the picture provided in Fig. 3f in the main text. Additionally, while there are 7 fit parameters, we take several steps to minimize any overfitting of the data. First, the background b is set to be the count level at a time much longer than the longest observed lifetime, essentially the dark counts and any leakage through our setup. Second, the existence of two decays is only relevant in regime II where there is a clear distinction between a fast and slow decay. In both regime I (III), the fast (slow) decay component is dominant over the other, effectively resulting in just a single stretched exponential.

We thank Referee 3 again for their careful evaluation and for the positive assessment of our work. We sincerely hope that the clarifications made in the revised manuscript and supplementary information provide sufficient physical context and explanations regarding both the distinction from the conventional saturation effect and our fit procedures involving stretched exponentials.