

Resonance fluorescence and indistinguishable photons from a coherently driven B centre in hBN

Corresponding Author: Dr Aymeric Delteil

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

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The paper by Gerard et al reports on coherent control of the photon emission from a special type of emitters in hexagonal boron nitride (hBN) with high spectral quality. Using resonant optical excitation and cross-polarization suppression of the laser plus spectral filtering they observe resonantly scattered photons. Two signatures prove the coherent nature of the scattering, i.e. observation of the Mollow triplet and two-photon interference in a Hong-Ou-Mandel type experiment.

Point defects in 2D materials have received lots of attention as single photon sources with high emission rates, however often plagued by spectral diffusion or spectral instabilities due to the presence of further defects (charge traps) on the material's surface. The authors here investigate a particular defect, the "B center", which exhibits superior stability among the hBN emitters, but still a large spectral broadening (0.75 GHz inhomogeneous broadening vs. 84 MHz Fourier limit). The experiments presented here only work out because the spectral jumps happen on a timescale that is longer than the temporal distance between two subsequently emitted photons which are made to interfere in the HOM interferometer. The results *pe se* are very interesting and relevant as they very convincingly demonstrate coherent optical control (Rabi oscillations) and coherent emission effects (Mollow triplet and high indistinguishability) – this has been demonstrated for many atomic and atom-like emitters but – as far as I can see – not for emitters in hBN.

The work presented here, to some extent, is the culmination of earlier work on the same or similar systems ranging from resonant excitation of hBN emitters (ACS Photonics, 5(2), 295-300 (2017) – not cited here) and work by some of the current authors on two-photon interference under off-resonant excitation (Ref. 28) and investigations of spectral diffusion effects under resonant excitation (Refs 15, 16, 22). Nevertheless, it contains a sufficient level of novel results justifying publication.

Specific comments:

1. p. 1: cross-polarization rejection – there is literature that explains the technique in great detail, e.g. PRX 11, 021007 (2021) or Rev. Sci. Instrum. 84, 073905 (2013). The authors might consider quoting this.
2. Fig. 1: photon detection seems to happen by TCSPC for all cases (RF spectroscopy, PLE spectroscopy, HOM interference) – does this mean the detection events are always referenced to a trigger signal?
3. The material thickness is 170 nm, certainly much thicker than one or few monolayers of hBN. I am not entirely familiar with the nomenclature in the hBN community, but does this count as 2D material? This goes beyond academic questions of naming conventions as here the emitters might be sufficiently far away from the surface with its adverse effects; furthermore interactions with lattice phonons varies between 2D and 2D materials.
4. p. 3 and Fig. 1e: does the evaluation of emitter purity ($g_2(0)$) take into account the obvious bunching effect in the photon statistics? (e.g. if the zero delay peak is compared just to the neighboring ones) The multi-exponential fitting function described in the Methods section seems to assume constant peak heights. Furthermore, is the area of the entire zero delay peak taken into account?
5. p. 5: the authors might clearly state that "time filtering" of the laser means time gating of the analysis, resulting in HOM visibilities of post-selected events.
6. For HOM measurements under pulsed excitation the correct normalization of the visibility is taking the ratio of the areas under the central peak for orthogonal and parallel polarizations. From the manuscript it looks like that the visibility is calculated from the peak values $g_2(0)$ only?

Reviewer #2

(Remarks to the Author)

The authors report on experimental work related to resonant pumping of a quantum emitter (B centre) in a hexagonal Boron Nitride flake transferred onto of a silver substrate for increased collection efficiency. The main novelty of the work resides in the exploration of the pulsed resonant regime, which the authors implement either by phonon sideband (PSB) collection, or by cross-polarization of the reflected laser. The latter being insufficient to entirely filter out the laser, the authors employ additional noise mitigation and post-selection techniques. The authors investigate a broad set of experiments including pulsed Rabi oscillations, the Mollow triplet, and to demonstrate a high visibility ($92 \pm 26\%$) in the Hong Ou Mandel experiment. The latter is a significant milestone in the field, showing considerable improvement compared to the previously measured 55% by the same group under non-resonant excitation. Although, this claim regarding the HOM visibility is weakened by the high error bar and need for post-selection.

In my view, while the work presents a significant achievement, it also suffers from a major shortcoming. The authors attempt to demonstrate that the nature of the emission stems from resonance fluorescence (RF), as opposed to phonon-assisted excitation and/or other incoherent processes, via the demonstration of pulsed Rabi oscillations and the Mollow triplet (as the authors state in the manuscript: "The observation of SPE signal close to the laser frequency is not a conclusive proof of RF in the solid state, due to the presence of potential incoherent mechanisms associated with photon emission"). My main concern is that the data provided are not conclusive on the two topics. First, the Rabi oscillations (fig. 1d) do not show collapse and revival but only a decrease after π pulse. Second, the Mollow triplet (fig. 3a) does not show peaks, but rather "shoulders" with monotonic slopes. These two datasets do not dismiss the possibility that the laser pulse may lead to incoherent power broadening of the emitter, causing the reduced signal in fig. 1d and the shoulders in fig. 3a. The demonstration of a clear revival in the Rabi oscillations, and/or well-resolved sidebands in the Mollow triplet would provide conclusive evidence and considerably strengthen the manuscript if the authors are able to provide these data.

I recommend major revision. In particular, the authors should provide more conclusive evidence for resonance fluorescence, either by demonstrating revival in the Rabi oscillations or by resolving the sidebands of the Mollow triplet more clearly. Without such evidence, a central claim of the manuscript remains insufficiently supported.

Other questions/remarks:

- How is the error bar obtained in the HOM visibility measurement ? Is this from the fit or by computing the visibility from the histogram ? This should be discussed (especially given the high uncertainties coming from the low count rates) and, ideally, both values of the HOM visibility (computed from the histogram, vs from the fit) with the corresponding error bars should be provided.
- The authors state: "Laser rejection is implemented by setting the excitation arm polarisation to linear, with an axis at 45degrees relative to the emitter dipole direction." How is the angle of excitation controlled ? This is not indicated, and according to the setup drawing there is no half waveplate, which could have fulfilled this function. Is the angle random and happens to be close to 45 degrees, and if so how is it verified ? Please clarify.
- The $g(2)$ measured in fig. 1e is claimed to be "the highest reported for a B centre". This claim should be nuanced as the $g(2)$ is only measured on the PSB emission. The $g(2)$ achieved under the same conditions for the full spectrum of the photon is in fact quite high and the authors must use heavy noise mitigation and post-selection techniques in the rest of the manuscript to distinguish the signal from the laser.
- The terminology "multi-exponential" for the fit in fig. 1e is misleading as it may imply that multiple timescales are involved, which is not the case according to equation (2). Could the authors please rephrase to avoid confusion.

Reviewer #3

(Remarks to the Author)

The paper reports the unambiguous measurement of resonance fluorescence (RF) from a B centre defect in hexagonal boron nitride (hBN). A series of experimental results are presented which evidence this claim and begin to characterise the coherence of the RF photons. The paper is very well-written giving good experimental details, clear and uncomplicated data analysis, and following a logical structure that guides the reader through the evidence in an elegant manner. The topic area is highly relevant to the community interested in the quantum optical properties of materials and their applications. The quality of the science is excellent, and the claims made in the paper are well-supported by the evidence provided. Having said this, the results are not surprising and, while the measurements have been done meticulously, for example recording datasets over 180h to measure the Hong Ou Mandel effect, the experiments are otherwise standard. Nevertheless I would expect the paper to be well-cited as it evidences the potential for these materials to be used in quantum technology applications.

Given this emphasis on technological potential, it is also important to mention some of the limitations and other contextual information. There are several points on which these are glossed over and the paper would be stronger if it addressed them: 1) Some comment could be made regarding the relatively low count rates measured. Does this indicate low quantum efficiency or a prolonged metastable state? Presumably the short emission wavelength means that the SPAD efficiency is quite low, but how much does this affect the measured count rates? In the 180h measurement time for the HOM contrast, only about 100 coincident counts are measured with orthogonal polarisation, suggesting that only 1 in 1 million excitation

pulses produced a measured indistinguishable photon. This appears much lower than the count rates in the rest of the paper would suggest.

2) A weakness of the system is the spectral diffusion which would, with the current materials, prevent indistinguishability between photons from different B centres or indeed from the same centre if the time delay between excitation pulses were longer. This should be stated. Also the inhomogeneous spectral distribution of ZPLs amongst different B centres should be mentioned to help the reader to understand the technological potential.

3) The ZPL wavelength of this colour centre is at 436 nm which is not convenient for most photonics applications (It would be helpful to readers to see a plot of the PL spectrum of the emitter).

4) A brief comment on the physical structure of the B centre would be useful.

Finally there are a few grammatical and typographic errors which can readily be fixed.

Overall I would recommend publication in Nature Communications if these points can be addressed.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I have read the authors' rebuttal and the new version of the paper and conclude that all of the referees' comments were taken into account in the revised version. I still have a few minor comments:

- rebuttal, p.3: discussion of bunching – the original figure 1e gave the impression that there was a varying peak height, indicative of bunching, for the first few side-peaks, i.e. with a time-constant of few 10 ns. In Ref. 20 / Supplement I could not find a pulsed resonant g_2 ? If the authors refer to Fig. S2(a) of Ref. 20: this shows some bunching in the 10^{-7} - 10^{-6} s range. Thus, just to be sure: can bunching on a time scale of 10...100 ns be excluded?

- rebuttal, p.4: the multi-peak fitting procedure seems to be better explained now. However, as it effectively subtracts background from the data, the fit parameters A and B need to be given.

- Discussion of Fig. 4b (and old data in supplement): $g_{2_parallel}(0)$ is given as 0.03 ± 0.09 ; this seems unphysical as g_2 has a lower bound of zero. I assume it is a consequence of the effective background suppression by the multi-peak fit. Accordingly, $V_{HOM} = 0.92 \pm 0.26$ is unphysical as well, as V cannot exceed 1.

Reviewer #2

(Remarks to the Author)

All comments have been addressed. In particular, the claims on Rabi oscillations and Mollow triplets are now supported by convincing data. In addition, the HOM data appear less noisy which further strengthens the manuscript. I recommend publication in nature communications.

Reviewer #3

(Remarks to the Author)

The authors have addressed all of my questions and comments satisfactorily, and I am happy to support publication

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Preliminary remarks

We thank the Referees for their constructive feedback. The three Referees acknowledge the importance and potential impact of our work. However, while Referees 1 and 3 have been convinced by our data, Referee 2 would have preferred some of the experimental signatures to be more compelling. Therefore, as a general remark before detailing our point-by-point responses to the Referees questions and remarks, we wish to let you know that we have complied with the Referee's request for additional data exhibiting clearer signatures of the investigated phenomena.

The measurements we presented in the initial version were obtained in a 170 nm hBN flake on silver (as indicated in the initial manuscript), which was available at the time of the initial experiment. We redid the whole series of measurements on an emitter in a similar structure, but hosted in a 220 nm thick flake. While the difference in sample structure seems slim, the second emitter has a lower saturation power and a higher count rate, which make the signatures of coherent control and emission clearer. This difference is explained to a large extent by the more efficient planar cavity effect in the second case, as well as a favourable vertical position of the emitter within the crystal. These effects have been investigated in a prior work (G  rard et al., ACS Photonics 2024 – ref 52). We also improved the throughput of the interferometer.

The new measurements confirm all conclusions presented in the initial version, with clearer signatures of the investigated phenomena, including the Mollow triplet and the Rabi oscillations in pulsed regime. The initial dataset has been kept in its entirety, and moved to a supplementary section, as it provides evidence of the reproducibility of the demonstrated phenomena. Both datasets are also openly available on Zenodo (<https://zenodo.org/records/17634836>).

When addressing the Referees' comments, we refer to the new dataset, except when explicitly stated.

Reviewer #1 (Remarks to the Author):

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Point defects in 2D materials have received lots of attention as single photon sources with high emission rates, however often plagued by spectral diffusion or spectral instabilities due to the presence of further defects (charge traps) on the material's surface. The authors here investigate a particular defect, the "B center", which exhibits superior stability among the hBN emitters, but still a large spectral broadening (0.75 GHz inhomogeneous broadening vs. 84 MHz Fourier limit). The experiments presented here only work out because the spectral jumps happen on a timescale that is longer than the temporal distance between two subsequently emitted photons which are made to interfere in the HOM interferometer. The results *pe se* are very interesting and relevant as they very convincingly demonstrate coherent optical control (Rabi oscillations) and coherent emission effects (Mollow triplet and high indistinguishability) – this has been demonstrated for many atomic and atom-like emitters but – as far as I can see – not for emitters in hBN.

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We thank the Referee for their positive assessment of our work, and for supporting publication of our manuscript. We have remedied the omission of the important reference they suggest, which we now cite in the introduction (page 1, right column).

Specific comments:

1. p. 1: cross-polarization rejection – there is literature that explains the technique in great detail, e.g. PRX 11, 021007 (2021) or Rev. Sci. Instrum. 84, 073905 (2013). The authors might consider quoting this.

We thank the Referee for suggesting these important references, which we now cite in the introduction (page 2, left column).

2. Fig. 1: photon detection seems to happen by TCSPC for all cases (RF spectroscopy, PLE spectroscopy, HOM interference) – does this mean the detection events are always referenced to a trigger signal?

We use the same detection setup in all experiments. However, a trigger signal is only needed in the HOM experiment. In the other measurements, the TCSPC is operated in count rate mode (for PLE scans, RF spectroscopy) or in start-stop mode (for HBT measurements in cw and pulsed regime). We have added this information in the Methods section.

3. The material thickness is 170 nm, certainly much thicker than one or few monolayers of hBN. I am not entirely familiar with the nomenclature in the hBN community, but does this count as 2D material? This goes beyond academic questions of naming conventions as here the emitters might be sufficiently far away from the surface with its adverse effects; furthermore interactions with lattice phonons varies between 2D and 2D materials.

We agree that the term “2D materials” is ambiguous and could be understood as monolayer crystals. Therefore, we have substituted this term by “van der Waals materials” throughout the text. However, it is worth mentioning that a relatively large thickness is not necessary to obtain B centres, since the SPEs are stable in crystals down to 3 nm thickness (Liu et al., arXiv:2507.02633 2025, now ref. 15). We have added this information in the introduction (page 1, right column).

4. p. 3 and Fig. 1e: does the evaluation of emitter purity ($g_2(0)$) take into account the obvious bunching effect in the photon statistics? (e.g. if the zero delay peak is compared just to the neighboring ones) The multi-exponential fitting function described in the Methods section seems to assume constant peak heights. Furthermore, is the area of the entire zero delay peak taken into account?

If the Referee is referring to the usual bunching artefact at long times when inferring g_2 by start-stop measurements (i.e. by considering only consecutive detection events instead of coincidences between all pairs of events). Considering the count rate we measure at pi pulse (8 kHz), the timescale of the bunching artefact is about 100 μ s. We fit the g_2 by considering 10 periods on each side of the zero delay and therefore the bunching effect is not discernible at these timescales (see figure R1 below, where it can be observed that the lateral peaks have constant heights). We have added this information in the Methods section.

If the Referee is considering the possibility of additional sources of bunching, such as shelving states or blinking, we have excluded such phenomena in prior work by measuring long-time $g(2)$ in resonant pulsed regime (ref. 20, Supplementary Information). We have also added a sentence in the Methods section, which reads “The height of all lateral peaks is fixed to the same value, consistently with the absence of bunching in long-time $g^{(2)}$ under resonant excitation by a broad pulsed laser [20].”

We note that on the figure 1e of the main text we only plot four lateral periods on each side due to space constraints, but the data corresponding to the whole range is available on Zenodo. Regarding the question about the area of the zero-delay peak, since the exponential decay is the same for all peaks and is fixed to the emitter lifetime, the relative height of the centre period (c_0) coincides with the ratio of the centre peak area to the average area of the non-zero periods. We now mention this remark in the revised Methods section.

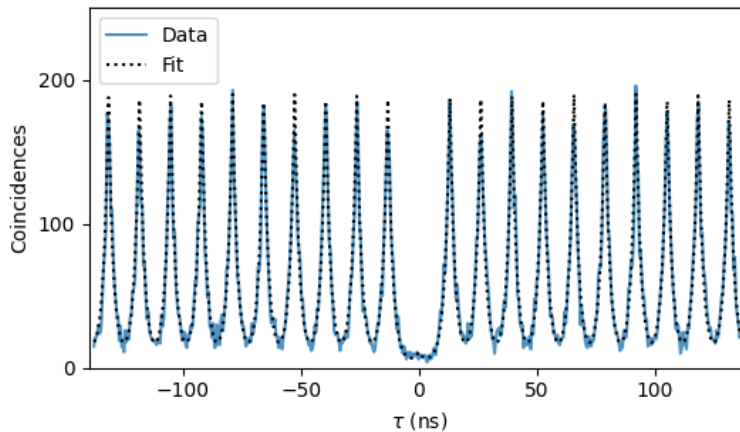


Figure R1: $g^{(2)}$ function measured in start-stop mode.

5. p. 5: the authors might clearly state that “time filtering” of the laser means time gating of the analysis, resulting in HOM visibilities of post-selected events.

We have modified the main text to make this point clearer in the revised manuscript. The new relevant sentences read:

p.5, left column: “Additionally, we use the timescale mismatch between the laser pulse length and the emitter lifetime to perform time gating of the remaining laser light based on post-selection of the detection events.”

p.5, left column: “We then characterize the indistinguishability of the post-selected photons in the blue shaded region on fig. 4a, i.e. excluding the pulsed laser background.”

p.5, right column: “We deduce an estimation of $V_{\text{HOM}} = 0.95 \pm 0.21$ of the post-selected events.”

6. For HOM measurements under pulsed excitation the correct normalization of the visibility is taking the ratio of the areas under the central peak for orthogonal and parallel polarizations. From the manuscript it looks like that the visibility is calculated from the peak values $g(0)$ only?

We refer the Referee to our response to their comment number 4. The ratio of the $g(0)$ for orthogonal and parallel polarisations as extracted from the fit coincides with the ratio of the central peak areas. We have added this mention to the Methods section. We note that this approach based on multipeak fitting is robust to the presence of background uncorrelated signal and to peak overlap, and is widely used in prior work on indistinguishable photon sources (e.g. Gazzano Nature Communications 2013, Somaschi Nature Photonics 2016, Kirsanske PRB 2017 – we now cite these references). We have added the following sentence in the revised text (p. 5, right column): “This fitting approach makes it possible to reliably extract the values of $g^{(2)}(0)$ even in the presence of sizeable uncorrelated background or partial peak overlap [48-50] (see Methods)”.

Reviewer #2 (Remarks to the Author):

The authors report on experimental work related to resonant pumping of a quantum emitter (B centre) in a hexagonal Boron Nitride flake transferred onto of a silver substrate for increased collection efficiency. The main novelty of the work resides in the exploration of the pulsed resonant regime, which the authors implement either by phonon sideband (PSB) collection, or by cross-polarization of the reflected laser. The latter being insufficient to entirely filter out the laser, the authors employ additional noise mitigation and post-selection techniques. The authors investigate a broad set of experiments including pulsed Rabi oscillations, the Mollow triplet, and to demonstrate a high visibility ($92 \pm 26\%$) in the Hong Ou Mandel experiment. The latter is a significant milestone in the field, showing considerable improvement compared to the previously measured 55% by the same group under non-resonant excitation. Although, this claim regarding the HOM visibility is weakened by the high error bar and need for post-selection.

In my view, while the work presents a significant achievement, it also suffers from a major shortcoming. The authors attempt to demonstrate that the nature of the emission stems from resonance fluorescence (RF), as opposed to phonon-assisted excitation and/or other incoherent processes, via the demonstration of pulsed Rabi oscillations and the Mollow triplet (as the authors state in the manuscript: "The observation of SPE signal close to the laser frequency is not a conclusive proof of RF in the solid state, due to the presence of potential incoherent mechanisms associated with photon emission"). My main concern is that the data provided are not conclusive on the two topics. First, the Rabi oscillations (fig. 1d) do not show collapse and revival but only a decrease after π pulse. Second, the Mollow triplet (fig. 3a) does not show peaks, but rather "shoulders" with monotonic slopes. These two datasets do not dismiss the possibility that the laser pulse may lead to incoherent power broadening of the emitter, causing the reduced signal in fig. 1d and the shoulders in fig. 3a. The demonstration of a clear revival in the Rabi oscillations, and/or well-resolved sidebands in the Mollow triplet would provide conclusive evidence and considerably strengthen the manuscript if the authors are able to provide these data.

I recommend major revision. In particular, the authors should provide more conclusive evidence for resonance fluorescence, either by demonstrating revival in the Rabi oscillations or by resolving the sidebands of the Mollow triplet more clearly. Without such evidence, a central claim of the manuscript remains insufficiently supported.

As we mentioned in introduction of our Response letter, we have repeated the whole series of measurements using a SPE with a sizably lower saturation power and a higher count rate. This has been made possible by using a favourable emitter in a sample with a different thickness (220 nm), which has a better collection enhancement, as studied in a prior study in photoluminescence (ref. 52). As a consequence, the Rabi oscillations in the pulsed regime (Fig. 1d) can be performed over more than one period and a half, exhibiting a clear revival of the count rate as the laser power increases. In addition, the Mollow sidebands on Fig. 3a are clearer and are seen as separate peaks at the highest powers. The initial dataset was kept as a supplementary material to provide evidence of reproducibility. We hope that this additional data constitutes a conclusive demonstration of the claimed phenomena.

Other questions/remarks:

- How is the error bar obtained in the HOM visibility measurement ? Is this from the fit or by computing the visibility from the histogram ? This should be discussed (especially given the high uncertainties coming from the low count rates) and, ideally, both values of the HOM visibility (computed from the histogram, vs from the fit) with the corresponding error bars should be provided.

The visibility and its error bar are deduced from the multi-peak fit to the data. We refer the Referee to our response to questions 4 and 6 from Referee 1 concerning the additional information about the fitting method. The main reason for this fitting method, which is used extensively in prior work on photon indistinguishability (see e.g. refs 48-50), is that it is robust to the presence of uncorrelated background signal as well as peak overlap. In turn, the coincidence histogram of fig 4b does not directly provide the indistinguishability, due to relatively large background counts and a slight peak overlap. We have added the following sentence in the revised text (p. 5, right column): "This fitting approach makes it possible to reliably extract the values of $g^{(2)}(0)$ even in the presence of sizeable uncorrelated background or partial peak overlap [48-50] (see Methods)". In addition, we discuss the method further in the Methods section.

- The authors state: "Laser rejection is implemented by setting the excitation arm polarisation to linear, with an axis at 45degrees relative to the emitter dipole direction." How is the angle of excitation controlled ? This is not indicated, and according to the setup drawing there is no half waveplate, which could have fulfilled this function. Is the angle random and happens to be close to 45 degrees, and if so how is it verified ? Please clarify.

There is indeed no half waveplate in the setup. The emitter dipole directions are randomly distributed amongst three directions correlated with the crystal axes. The sample is oriented such that one of these three dipole axes is at about 45° relatively to the polarized beamsplitter axes. The two studied emitters are selected amongst SPEs on our samples to possess this 45° dipole orientation. This is controlled in the absence of the polarized beamsplitter by manually rotating a polarizer and verifying that the count rate is minimal when the polarizer is turned by about 45°. We have added this information in the Methods section.

- The $g(2)$ measured in fig. 1e is claimed to be "the highest reported for a B centre". This claim should be nuanced as the $g(2)$ is only measured on the PSB emission. The $g(2)$ achieved under the same conditions for the full spectrum of the photon is in fact quite high and the authors must use heavy noise mitigation and post-selection techniques in the rest of the manuscript to distinguish the signal from the laser.

We have removed the claim mentioned by the Referee and now make it clearer that the $g(2)$ is measured for the PSB photons. The modified sentence reads "The multi-peak fit (dashed black curve on fig. 1e) provides $g^{(2)}(0) = 0.03 \pm 0.02$, corresponding to a purity of 0.97 ± 0.02 for the PSB photons."

- The terminology "multi-exponential" for the fit in fig. 1e is misleading as it may imply that multiple timescales are involved, which is not the case according to equation (2). Could the authors please rephrase to avoid confusion.

We have replaced the ambiguous term "multi-exponential" by "multi-peak" throughout the text.

Reviewer #3 (Remarks to the Author):

The paper reports the unambiguous measurement of resonance fluorescence (RF) from a B centre defect in hexagonal boron nitride (hBN). A series of experimental results are presented which evidence this claim and begin to characterise the coherence of the RF photons. The paper is very well-written giving good experimental details, clear and uncomplicated data analysis, and following a logical structure that guides the reader through the evidence in an elegant manner. The topic area is highly relevant to the community interested in the quantum optical properties of materials and their applications.

The quality of the science is excellent, and the claims made in the paper are well-supported by the evidence provided. Having said this, the results are not surprising and, while the measurements have been done meticulously, for example recording datasets over 180h to measure the Hong Ou Mandel effect, the experiments are otherwise standard. Nevertheless I would expect the paper to be well-cited as it evidences the potential for these materials to be used in quantum technology applications.

Given this emphasis on technological potential, it is also important to mention some of the limitations and other contextual information. There are several points on which these are glossed over and the paper would be stronger if it addressed them:

We thank the Referee for their positive assessment of our work. In the following, we address their comments.

1) Some comment could be made regarding the relatively low count rates measured. Does this indicate low quantum efficiency or a prolonged metastable state? Presumably the short emission wavelength means that the SPAD efficiency is quite low, but how much does this affect the measured count rates? In the 180h measurement time for the HOM contrast, only about 100 coincident counts are measured with orthogonal polarisation, suggesting that only 1 in 1 million excitation pulses produced a measured indistinguishable photon. This appears much lower than the count rates in the rest of the paper would suggest.

In the original dataset, we indeed obtained about 100 coincidences per bin in the HOM experiment, which corresponds to about 250 coincidences when integrated over a full 13 ns period. In the new dataset, we obtain a similar number of coincidences, in a reduced time of about 60 h thanks to the improved count rate of the new sample. This corresponds to a count rate of about 600 counts/s in the APDs after the HOM interferometer. Starting from an excitation rate of 76 MHz, we obtain a probability of 8×10^{-6} that a laser pulse yields detection of an indistinguishable photon.

In brief, there are multiple reasons for such low count rate, including the low throughput of the HOM interferometer (which is only used in the last part of the current work), the limited transmission efficiency of the rest of the setup, the filtering of ZPL photons from the whole spectrum, the laser suppression using cross-polarisation suppression, and the efficiency of the light collection by the first objective. Regarding the emitter quantum efficiency and the presence of shelving states, prior work by our team using the same samples confirms that the emitter has a high quantum efficiency and no efficient shelving to metastable states.

We have added a section in the Supplementary Information (section S3) to estimate the various sources of photon losses, which we found consistent with the count rates measured in the multiple experiments we present in the manuscript. This new section includes a mention of the collection efficiency, the emitter quantum efficiency and the shelving states. We explicitly refer to this supplementary section in the main text (page 3, left column) and in the Methods section.

2) A weakness of the system is the spectral diffusion which would, with the current materials, prevent indistinguishability between photons from different B centres or indeed from the same centre if the time delay between excitation pulses were longer. This should be stated. Also the inhomogeneous spectral distribution of ZPLs amongst different B centres should be mentioned to help the reader to understand the technological potential.

Following the Referee's suggestion, we have added a sentence at the end of the paragraph about HOM interference to acknowledge for this limitation (page 6, left column). The sentence reads "The visibility of HOM interference between photons from the same source is expected to remain high as long as there is no spectral jump between photon emission events, i.e. at delays shorter than a few milliseconds. Interference between photons separated by longer times, as well as between photons from distinct sources, will require to mitigate spectral diffusion using improved material, electrical gating or Purcell enhancement."

In addition, we quote the inhomogeneous ensemble distribution width in the revised introduction (page 1, right column).

3) The ZPL wavelength of this colour centre is at 436 nm which is not convenient for most photonics applications (It would be helpful to readers to see a plot of the PL spectrum of the emitter).

We agree with the Referee that applications to quantum communications are jeopardized by the short emission wavelength. However, we believe that this family of emitters is well suited for applications based on integrated photonics, where the controlled position and wavelength is an asset for the realization of top-down integrated devices. We have added the following sentence to the introduction section:

“Their emission wavelength has a narrow (0.5 nm full width at half maximum) ensemble distribution around 436 nm [11]. Although this wavelength is not ideal for quantum communications, the controlled positioning of the emitters enables the development of integrated on-chip quantum computing devices.”

In addition, we have added a PL spectrum in the Supplementary Material section S1, and we refer to it in the main text (page 3, left column).

4) A brief comment on the physical structure of the B centre would be useful.

We have added a short sentence to mention the current understanding of their structure and cite recent references on that matter. The new sentence reads (p. 1, right column):

“Their microscopic structure is currently understood as an out-of-plane carbon dimer in the split interstitial configuration, fabricated by electron-beam-induced conversion of ubiquitous UV-emitting in-plane dimers [16-18].”

Finally there are a few grammatical and typographic errors which can readily be fixed.

We have carefully proofread the manuscript and have corrected any grammatical and typographic errors we have detected.

Overall I would **recommend publication** in Nature Communications if these points can be addressed.

We thank the Referee for recommending publication and hope that our responses are satisfactory.

We thank the Referees for supporting publication of our work. In the following, we address the remaining comments from Reviewer #1.

Reviewer #1 (Remarks to the Author):

I have read the authors' rebuttal and the new version of the paper and conclude that all of the referees' comments were taken into account in the revised version. I still have a few minor comments:

- rebuttal, p.3: discussion of bunching – the original figure 1e gave the impression that there was a varying peak height, indicative of bunching, for the first few side-peaks, i.e. with a time-constant of few 10 ns. In Ref. 20 / Supplement I could not find a pulsed resonant g^2 ? If the authors refer to Fig. S2(a) of Ref. 20: this shows some bunching in the 10^{-7} - 10^{-6} s range. Thus, just to be sure: can bunching on a time scale of 10...100 ns be excluded?

The apparent bunching effect in the original figure 1e (now S2d) is due to an aliasing effect originating from the sampling period (i.e. time bin) being incommensurate with the laser repetition period. To ensure the absence of bunching, we calculate the g^2 integrated over the laser repetition period and plot over 200 periods (corresponding to $\sim 2.6 \mu\text{s}$). The results are shown in Supplementary Information section S4 and testify of the complete absence of bunching at short timescales. We refer to this figure in the Methods section. The new sentence reads: *"The height of all lateral peaks is fixed to the same value. This implies that there is no additional correlations at similar timescales due to blinking, spectral diffusion or metastable states, which could yield additional bunching. This is verified in the Supplementary Information section S4, where we plot the integrated g^2 of SPE_1 and SPE_2 over 200 laser periods."*

- rebuttal, p.4: the multi-peak fitting procedure seems to be better explained now. However, as it effectively subtracts background from the data, the fit parameters A and B need to be given.

We now provide the values of A and B in the Methods section. The new sentence reads: *"The extracted signal and background contributions in the HOM experiments are $A = 95$, $B = 33$ for the parallel configuration, and $A = 101$, $B = 32$ for the orthogonal configuration."*

- Discussion of Fig. 4b (and old data in supplement): $g^2_{\text{parallel}}(0)$ is given as 0.03 ± 0.09 ; this seems unphysical as g^2 has a lower bound of zero. I assume it is a consequence of the effective background suppression by the multi-peak fit. Accordingly, $V_{\text{HOM}} = 0.92 \pm 0.26$ is unphysical as well, as V cannot exceed 1.

The error bar is extracted from the fit, which we did not restrict to physical values between 0 and 1. By restricting it to physically acceptable values, we obtain asymmetric error bars, i.e. $g_{\parallel}^{(2)}(0) = 0.03^{+0.09}_{-0.03}$ and $V_{HOM} = 0.93^{+0.07}_{-0.21}$ for SPE₁, and $g_{\parallel}^{(2)}(0) = 0.04^{+0.12}_{-0.04}$ and $V_{HOM} = 0.92^{+0.08}_{-0.26}$ for SPE₁. We have modified all values of the main text and supplementary to account for the Referee's suggestion