

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The paper considers the problem of entanglement enhanced spectroscopy.

Some background on entanglement enhanced spectroscopy in general is lacking, with an emphasize on what is new about the current manuscript. Here is an incomplete list of previous works: [Infrared spectroscopy with visible light by Kalashnikov et al], [Entanglement-enhanced detection of single-photon scattering events by Hempel et al] and [Temperature-Controlled Entangled-Photon Absorption Spectroscopy by Leon-Monitel et al]...

The results part are written in a way that mixes new results and known results, and makes it hard to tell which part is really novel.

Why is the interaction with the matter described by a four-wave mixing process? It does not seem to be justified.

What matter is being considered? I suppose different matter will provide different results other than the one being plotted in Fig. 5d. What information is obtained from the simulated measurement result? What parameter or properties is being inferred from Fig. 5d?

It is unclear to me what "The signals of Eqs. (6), (7), (S13) correspond to classical four-wave mixing processes." mean, I cannot find those equations.

Loss analyses or discussions are necessary, as the paper deals with single-photon level interferometry.

Minor things:

line 24: extra dot after "are."

line 26 "Lately" should not use capital

Due to the above listed confusions, I cannot recommend publication at this point, but if the authors can well explain my confusion and improve the manuscript, it might worth a reconsideration.

Reviewer #2 (Remarks to the Author):

The main point of the paper is to propose a scheme to observe the alteration of so-called quantum light when interacting nonlinearly with a molecular sample, as a means to learn about the sample. The results are most likely correct mathematically, but there are several issues that make it not suitable for Communications Physics.

The main claim is: We demonstrate how interferometric detection of nonlinear spectroscopic signals may be used to improve the measurement accuracy of matter susceptibilities.

It is not clear to me where in the paper the accuracy is discussed quantitatively and shown to be improved.

The paper is very hard to read and understand:

1. The use of English is not sufficient and this goes beyond merely correcting simple grammar and punctuation errors. The logic and organization of the paper and the explanations of many details are not clear enough to be useful to a wide range of readers. See points below.

2. The work is not put into sufficient prior context. The scheme resembles that in several prior papers, so it is important to explain the differences. Rather than mentioning Refs 16-18 in line 168, it would be better to discuss them in the Introduction. What is known from the prior works, and how does the new result go beyond?

One other paper that might be cited is:

Unified view of the second-order and the fourth-order interferences in a single interferometer Jing Qiu et al Optics Communications 336(2015)9-13

3. I believe Equation 1 simply represents the far field emitted by an oscillating electric dipole, and is well known in classical E&M theory. B Mollow used this quantum form in early papers on resonance fluorescence I believe.

4. The descriptions about short/long pulses and narrowband/broad band confusing.

Line 52-54 says: In the following we consider narrowband pulses,... Then it says: This regime fits experimental setups with ultrafast pulses ($\delta T \leq 1$ ps).

Line 95 says: Below we focus on a narrowband pump. Extension to a broadband pump is outlined in Sec. S2 of SI.

Line 102 says: We focus on the $L^* \ll L_p$ regime, where the spatial extent of the photon wavepacket after the interaction is localized relative to the dimensions of the interferometer.

Are all the above statements inconsistent?

5. Lines 99 and 109 refer to Eq 14 and 15, but its not clear where those are from. Cite Methods at end.

6. There is confusion and I believe errors regarding the so-called two-photon wavefunction and its symmetry.

Line 141 says: Such symmetry is a consequence of the exchange symmetry in the twin-photon wavefunction...

This is not true in general. One might think it is true because photons are bosons. But the state given is not a state of photons, but a state of modes of the field. The modes are distinguishable, (so for example in thermal equilibrium obey Boltzmann statistics not Bose, as is well known.) So, the two-frequency state of the field (joint spectral amplitude) does not have any symmetry in general, but it is well known that only the symmetric part participates in HOM interference.

The same error occurs in the caption of Fig 5. and in Methods Line 199.

The correct treatment when discussing photon states rather field states is well known. See for example the review Smith, B. J., & Raymer, M. G. (2007). Photon wave functions, wave-packet quantization of light, and coherence theory. New Journal of Physics, 9(11), 414.

7. Line 203 says: Here ω_a and ω_b correspond to the projections of the wavevectors. This seems to make no sense at all as written. Frequencies are not wave vectors.

Reviewer #3 (Remarks to the Author):

This paper is largely a proposal for how one can use HOM measurements to measure properties of molecules that are placed into one of the beams of a parametric down conversion source. The paper develops the theory both for entangled and unentangled sources, and shows that the entangled source leads to better time resolution (by perhaps a factor of 2 based on the application presented in Fig. 5). This is a useful result that will help guide the field, however I have one important comment that needs to be addressed:

It is not clear how Figs. 1 and 2 connect. Fig. 1 describes parametric down conversion leading to the emission of entangled photons that propagate in different directions and ultimately recombine at the beam splitter. One of these photons interacts with matter in Fig. 2 but now there are three photons. I don't understand what this is all about. Typically one photon will scatter from the matter, and this leads to absorption and phase shifting as described later. Of course there is a χ -3 process that would involve nonlinear interaction with matter (such as in an optical Kerr process) but for intensities that are typically available from a down conversion source, this would be a minor effect compared to the linear scattering.

There are some obvious formatting problems in the first couple pages of the manuscript, with periods interspersed randomly here and there. And the first paragraph of the Introduction is very poorly worded. Hopefully someone who is fluent can polish.

RESPONSE TO THE REFEREES

We would like to thank the referee for the thoughtful review of our work. We are pleased that the referee found our work interesting. We have modified our manuscript to address all the reviewer points. The summary of changes made in the manuscript as a part of this revision along with our brief response to the referee concerns is enclosed below. We hope that the paper will now be accepted for publication in Communications Physics.

Best regards,

Konstantin E. Dorfman, Shahaf Asban, Bing Gu, and Shaul Mukamel

SUMMARY OF THE CHANGES

Reviewer #1 (Remarks to the Author):

The paper considers the problem of entanglement enhanced spectroscopy.

Q1. Some background on entanglement enhanced spectroscopy in general is lacking, with an emphasize on what is new about the current manuscript. Here is an incomplete list of previous works: [Infrared spectroscopy with visible light by Kalashnikov et al], [Entanglement-enhanced detection of single-photon scattering events by Hempel et al] and [Temperature-Controlled Entangled-Photon Absorption Spectroscopy by Leon-Monitel et al]...

A1. We thank the referee for pointing out these relevant references. We now cite them in the revised manuscript

Q2. The results part are written in a way that mixes new results and known results, and makes it hard to tell which part is really novel.

A2. In the revised manuscript we now clarify the novelty of the results by adding the following discussion:

On p. 1:

“Previous works attempted either to utilize HOM-like measurements to address properties of the beam splitters or the individual emitters [K.E. Dorfman and S. Mukamel, Sci. Rep. 4, 3996 (2014)], or utilize four-wave mixing in the quantum light generation [Z. Yang et al., APL 116, 244001 (2020)]. In the present work we combined the interferometric detection (HOM) with wave mixing that uses both classical and quantum light beams to address more complex nonlinear optical processes and the corresponding novel components of the nonclassical response function”.

Q3. Why is the interaction with the matter described by a four-wave mixing process? It does not seem to be justified.

A3. We added the following discussion on p. 3:

“The general third order nonlinear optical process generates various signals that are well studied in classical light spectroscopy: optical pump-probe, Raman, fluorescence, transient grating, photon echo and others. Four-wave mixing (FWM) signals plays an important role as it allows to have additional control over the field-matter interactions via spatial phase matching. The typical FWM setup shown in Fig. 2 where three beams interacting with the material sample generate a fourth beam propagating in the direction governed by one of the eight possible phase matching conditions.”

Q4. What matter is being considered? I suppose different matter will provide different results other than the one being plotted in Fig. 5d. What information is obtained from the simulated measurement result? What parameter or properties is being inferred from Fig. 5d?

A4. Indeed, the model of the matter is crucial for interpreting the coincidence measurement. To better explain the model of matter we added the following discussion on p.3:

“The matter is modeled by a collection of N homogeneously distributed point-like molecular dipoles at random positions $\mathbf{r}_\alpha$. Adopting the multipolar coupling Hamiltonian following with rotational averaging we obtain...”

Another discussion on p. 5:

“The signal with phase matching at $\mathbf{k}_s = \mathbf{k}_a + \mathbf{k}_b + \mathbf{k}_c$ can be generated in the four electronic state system shown in Fig. 4a. One can surely select another phase matching direction, out of the eight possible ones. Each direction contains a different type of material information governed by the set of pathways containing in the corresponding $\chi^{(3)}$ nonlinear susceptibility.”

Regarding the results shown in Fig. 5d. Indeed, the coincidence measurement provides matter information related to the model shown in Fig. 4. We have added the following discussion on p. 6:

“The coincidence HOM measurement can reveal the energies and lifetimes of the four electronic levels $\{g, e_1, e_2, f\}$ along with the transition dipoles between the states and relevant coherence dephasing rates. For instance, the f-g coherence dephasing is a crucial parameter that determines the temporal resolution of the coincidence measurement and strongly depends on the state of light.”

Q5. It is unclear to me what "The signals of Eqs. (6), (7), (S13) correspond to classical four-wave mixing processes." mean, I cannot find those equations.

A5. We removed the misleading sentence.

Q6. Loss analyses or discussions are necessary, as the paper deals with single-photon level interferometry.

A6. This is very important point. To address it we added the following discussion on p. 5:

“Note, that the response function $\mathcal{C}(\tau)$ is calculated using three classical fields followed by a single photon field in the last interaction. While it is not unusual that quantum enhanced performance is dramatically eroded by the loss of a single photon, this is not the case here. The classical incoming fields have a large number of photons and are thus insensitive to losses compared to other classical technique. When the single photon contribution has losses, the signal vanishes due to violation of the phase-matching that results from momentum conservation. The improved performance is attributed to the non-classical correlations between the photon pair, not from their Fock-state characteristics. Thus, it is robust against losses similar to classical nonlinear methods (shot-noise limited).“

Q7. Minor things:

line 24: extra dot after "are."

line 26 "Lately" should not use capital

A7. We corrected the typos.

Q8. Due to the above listed confusions, I cannot recommend publication at this point, but if the authors can well explain my confusion and improve the manuscript, it might worth a reconsideration.

A8. We thank the referee for helping us improve our manuscript. We hope that the revised version will meet all concerns of the referee and will be accepted for publication.

Reviewer #2 (Remarks to the Author):

Q1. The main point of the paper is to propose a scheme to observe the alteration of so-called

quantum light when interacting nonlinearly with a molecular sample, as a means to learn about the sample. The results are most likely correct mathematically, but there are several issues that make it not suitable for Communications Physics.

The main claim is: We demonstrate how interferometric detection of nonlinear spectroscopic signals may be used to improve the measurement accuracy of matter susceptibilities.

It is not clear to me where in the paper the accuracy is discussed quantitatively and shown to be improved.

A1. The goal of the present work is to identify the novel molecular information obtained by combining interferometric detection and a four-wave mixing process that involves both classical and quantum light fields. While the accuracy of the experiment is very important, we focused on qualitative effects such as new information associated with the nonlinear response of the molecules which is not accessible classically. We have modified the presentation of the main results by adding the following in the introduction:

“The general third order nonlinear optical process generates various signals that are well studied in classical light spectroscopy: optical pump-probe, Raman, fluorescence, transient grating, photon echo and others. Four-wave mixing (FWM) signals plays an important role as it allows to have additional control over the field-matter interactions via spatial phase matching. The typical FWM setup shown in Fig. 2 where three beams interacting with the material sample generate a fourth beam propagating in the direction governed by one of the eight possible phase matching conditions.”

Q2. The paper is very hard to read and understand:

The use of English is not sufficient and this goes beyond merely correcting simple grammar and punctuation errors. The logic and organization of the paper and the explanations of many details are not clear enough to be useful to a wide range of readers. See points below.

A2. We carefully checked and improved our presentation including grammar, punctuation as well as the logic of the presentation. We believe that the reviewer will find our revised manuscript significantly more accessible.

Q3. The work is not put into sufficient prior context. The scheme resembles that in several prior papers, so it is important to explain the differences. Rather than mentioning Refs 16-18 in line 168, it would be better to discuss them in the Introduction. What is known from the prior works, and how does the new result go beyond?

A3. In the revised version we included the discussion of the mentioned references in the introduction putting our work in the context of the prior works including the roadmap article [1]. We added the following discussion in the introduction:

“The unified picture of second-order and fourth-order interferences in a single interferometer have been demonstrated in [6]. Previous works attempted either to utilize HOM-like measurements to address properties of the beam splitters or the individual emitters [K.E. Dorfman and S. Mukamel, Sci. Rep. 4, 3996 (2014)], or utilize four-wave mixing for the quantum light generation [Z. Yang et al., APL 116, 244001 (2020)]. In the present work we combined the interferometric detection (HOM) with wave mixing that involves both classical and quantum light beams to address more complex nonlinear optical processes and the corresponding novel components of the nonclassical response function.”

We further added the following references to highlight previous work in the field: [Infrared spectroscopy with visible light by Kalashnikov et al], [Entanglement-enhanced detection of single-photon scattering events by Hempel et al] and [Temperature-Controlled Entangled-Photon Absorption Spectroscopy by Leon-Monitel et al].

Q4. One other paper that might be cited is:

Unified view of the second-order and the fourth-order interferences in a single interferometer
Jing Qiu et al Optics Communications 336(2015)9–13

A4. We have added this highly relevant reference in the revised version.

Q5. I believe Equation 1 simply represents the far field emitted by an oscillating electric dipole, and is well known in classical E&M theory. B Mollow used this quantum form in early papers on resonance fluorescence I believe.

A5. Indeed Eq. (1) originates from a simple dipole radiation/matter coupling Hamiltonian which is used to obtain a variety of the optical signals including resonance fluorescence. We now cite the original Mollow paper to put Eq. (1) in this perspective.

Q6. The descriptions about short/long pulses and narrowband/broad band confusing.

Line 52-54 says: In the following we consider narrowband pulses,... Then it says: This regime fits experimental setups with ultrafast pulses ($\delta T \leq 1$ ps).

Line 95 says: Below we focus on a narrowband pump. Extension to a broadband pump is outlined in Sec. S2 of SI.

Line 102 says: We focus on the $L^* \ll L_p$ regime, where the spatial extent of the photon wavepacket after the interaction is localized relative to the dimensions of the interferometer.

Are all the above statements inconsistent?

A6. We thank the referee for the insightful remarks. We acknowledge the cumbersome presentation of the length scales in the previous version of our manuscript and corrected it. On p. 2, we clarify the role of each scale.

“The interferometric setup naturally gives rise to two characteristic timescales and respective length scales. First, the response interval τ_R in which light-matter interaction

occurs. It is determined by the pulse envelope, the spatial dimension of the sample and the response time. Second is the pulse relative delay interval $\delta\tau$ determined by the interference region governed by the interferometer dimensions. Here, we consider the field-matter interaction region to be localized compared to the spatial dimensions of the interferometer. We further consider a sequence of ultrafast coherent excitation pulses – which are classical for all practical purposes – followed by an interaction with the quantum state of light.

The excitation and deexcitation period is dominated by the duration of the narrowband envelope of the quantum field given by $c\tau_R$ combined with the pulse delay interval $c\delta\tau$. The maximal duration of this interval is defined by L^* which is smaller than a few hundred micrometers even for a picosecond pulse which is well within the narrowband region. For example, for a transform limited Gaussian pulse with central wavelength $\lambda_c = 1064\text{ nm}$ and pulse duration $\Delta t = 2\text{ ps}$ which occupies $c\tau_R \approx 600\text{ }\mu\text{m}$. Finally, the interferometer length scale denoted L_p specifies the free propagation distance between the incident beams, the beam-splitter and the detector. Typical interferometer length is in the order of few centimeters which justifies the separation of timescales – considering the interaction interval to be localized around the sample compared with L_p . Moreover, the Rayleigh distance is typically a few meters in this setup, thus one can consider the propagation as unidirectional for all practical purposes. “

Q7. Lines99 and 109 refer to Eq14 and 15, but its not clear where those are from. Cite Methods at end.

A7. We added Method in the citation of these equations.

Q8. There is confusion and I believe errors regarding the so-called two-photon wavefunction and its symmetry.

Line141 says: Such symmetry is a consequence of the exchange symmetry in the twin-photon wavefunction...

This is not true in general. One might think it is true because photons are bosons. But the state given is not a state of photons, but a state of modes of the field. The modes are distinguishable, (so for example in thermal equilibrium obey Boltzmann statistics not Bose, as is well known.) So, the two-frequency state of the field (joint spectral amplitude) does not have any symmetry in general, but it is well known that only the symmetric part participates in HOM interference.

The same error occurs in the caption of Fig 5. and in Methods Line 199.

The correct treatment when discussing photon states rather field states is well known. See for example the review Smith, B. J., & Raymer, M. G. (2007). Photon wave functions, wave-packet quantization of light, and coherence theory. *New Journal of Physics*, 9(11), 414.

A8. We agree, that in general twin state wavefunction does not have exchange symmetry as follows from Eq. (14) while the interference in HOM occurs from the symmetric part. In the narrowband regime analytical simplifications give rise to the twin photon wavefunction given by Eq. (15), which is symmetric. In the revised manuscript we discuss more extensively the regimes of validity of the demonstrated results, which nonetheless do not limit our formalism. We then mention the relevant references provided by the referee.

“Note, that the wavefunction in Eq. (14) does not generally possess exchange symmetry, since the twin state represents the state of the field modes (amplitudes), which can be distinguished by e.g. group velocity dispersion, polarization etc. In the same time Eq. (15) yields a simplified expression originating from the narrowband pump pulse, where overlapping modes result in the exchange symmetry of the wavefunction typical for the HOM experiment. A more general treatment of the photon wave functions is discussed in [New Journal of Physics, 9(11), 414].”

Q9. Line203 says: Here ω_a and ω_b correspond to the projections of the wavevectors. This seems to make no sense at all as written. Frequencies are not wave vectors.

A9. We corrected the error highlighting the relation between the frequency variables and transverse components of the wavevectors.

Reviewer #3 (Remarks to the Author):

This paper is largely a proposal for how one can use HOM measurements to measure properties of molecules that are placed into one of the beams of a parametric down conversion source. The paper develops the theory both for entangled and unentangled sources, and shows that the entangled source leads to better time resolution (by perhaps a factor of 2 based on the application presented in Fig. 5). This is a useful result that will help guide the field, however I have one important comment that needs to be addressed:

We thank referee for the positive assessment of our work.

Q1. It is not clear how Figs. 1 and 2 connect. Fig. 1 describes parametric down conversion leading to the emission of entangled photons that propagate in different directions and ultimately recombine at the beam splitter. One of these photons interacts with matter in Fig.2 but now there are three photons. I don't understand what this is all about. Typically one photon will scatter from the matter, and this leads to absorption and phase shifting as described later. Of course there is a χ^3 process that would involve nonlinear interaction with matter (such as in an optical Kerr process) but for intensities that are typically available from a down conversion source, this would be a minor effect compared to the linear scattering.

A1. This is a very important question that needs to be addressed. Fig. 1 and Fig. 2 represent two conceptually different setups that aim to illustrate conventional HOM and four-wave mixing setups, respectively. The two setups provide two pieces of the puzzle while Fig. 4 represents the final measurement where both setups are combined. In the revised manuscript we connected the different parts of the presentation in a more transparent and logical way. We thus added the following discussion on p. 2 before the discussion of individual steps.

“The proposed experiment combines several conventionally used optical techniques such as four-wave mixing, beam splitting, and Hong-Ou-Mandel (HOM) interferometer, and three-photon absorption spectroscopy. In the following sections we present each technique independently highlighting the main principle and the underlying theoretical model that will be used to describe each part of the setup. We finally combine both techniques in the setup shown in Fig. 4a and discuss the resulting HOM spectroscopic measurements. The main goal of the proposed measurement is to use interferometric (HOM-like) detection to investigate the $\chi^{(3)}$ nonlinear susceptibility that combines three classical and one quantum fields. Unlike the Kerr process which requires high intensity laser pulses to produce third-order nonlinear response, here we deal with resonant absorption of each of the light beams participating in the four-wave mixing. The corresponding level -scheme is shown in Fig. 4a, where three classical light beams generated are combined with the quantum light pulse produced by the parametric down conversion (independently from the classical three pulses). This nonlinear process is the main focus of our study.”

In addition we added insets to Fig. 4 to clarify the combined setup.

Q2. There are some obvious formatting problems in the first couple pages of the manuscript, with periods interspersed randomly here and there. And the first paragraph of the Introduction is very poorly worded. Hopefully someone who is fluent can polish.

A2. In the revised version we made a great effort to correct the formatting and language of our presentation.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

It is still unclear to me what is being measured, the authors claim one can reveal energy and lifetime of the four electron levels, but in their model of matter, it is just point-like dipole. No clear connection has been established. If you hope to estimate energy and lifetime, why we do not see a dependence on these parameters in the matter model?

The authors claim that the advantage is robust against loss, but without a clear analyses or proof. The fact is, when the noise is high enough, non-classical correlations will be destroyed. So unless the authors can write a proof like what happened in quantum illumination, the claim is simply false. It is likely that the authors want to rely on post-selection, but to me right now they are not clear about it yet.

To conclude, I don't think this manuscript has met the standard of communications physics, the physics behind what they are doing is not clear.

Reviewer #2 (Remarks to the Author):

The revised manuscript addresses all of my concerns. The results are of interest, and I recommend publication in the current form.

Reviewer #3 (Remarks to the Author):

The revisions are satisfactory in my opinion.

RESPONSE TO THE REFEREES

We would like to thank the referee for the thoughtful review of our work. Their comments and remark truly improved our presentation and make our manuscript more accessible for broad readership. We have modified our manuscript to address all the remaining points. The summary of changes made in the manuscript as a part of this revision along with our brief response to the referee concerns is enclosed below. We hope that the paper will now be accepted for publication in Communications Physics.

Best regards,

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A1. We thank the referee for this important point. We mentioned that matter is modeled by a collection of N homogeneously distributed point-like molecular dipole. We also stated that each molecule is modelled by a four level system $\{g, e_1, e_2, f\}$. We agree with the referee that a link between these two representations was missing. In the revised version of the manuscript, we added the following discussion on p. 7:

“The third order nonlinear susceptibility is calculated using perturbative field-matter interactions arranged in the diagram in Fig. 4c. Following the general approach outlined in chapter six of Ref. [12] we obtain:

$$\chi^{(3)}(-\omega; \omega_c, \omega_b, \omega_a) = \frac{-\mu_{fe_2}\mu_{e_2e_1}\mu_{e_1g}}{(\omega_a + \omega_b + \omega_c - \omega_{fg} + i\gamma_{fg})(\omega_a + \omega_b - \omega_{e_2g} + i\gamma_{e_2g})(\omega_a - \omega_{e_1g} + i\gamma_{e_1g})},$$

where $\omega = \omega_a + \omega_b + \omega_c$ implies energy conservation and μ_{ij} are transition dipole moments. The nonlinear susceptibility contains one-, two-, and three-photon resonances determined by the transition frequencies ω_{ij} and dephasing rates (linewidth) γ_{ij} , $i, j = g, e_1, e_2, f$.”

Q2. The authors claim that the advantage is robust against loss, but without a clear analyses or proof. The fact is, when the noise is high enough, non-classical correlations will be destroyed. So unless the authors can write a proof like what happened in quantum illumination, the claim is simply false.

A2. It is indeed important to provide evidence for the robustness. To that end we modified the following discussion on p. 6:

“Several noise sources can be considered such as losses associated with single photon sources, non phase matched contributions, and imperfect transmission and detection efficiency. First, the proposed setup in the single photon regime allows overcoming the noise because of the photon correlation measurement. The classical incoming fields contain a large number of photons and are thus insensitive to losses compared to other classical technique. When the single photon contribution has losses, the signal vanishes due to violation of the phase-matching due to momentum conservation. Second, the improved performance is attributed to the non-classical correlations between the photon pair, not from their Fock-state characteristics. Contributions to such losses can originate from non-phase matched signals, like spontaneous emission adding vacuum fluctuations to the transmitted beams. Third, losses occurring after the mixing can be modeled by a beam splitter with an empty port [H. A. Bachor and T. C. Ralph, A Guide to Experiments in Quantum Optics (Wiley-VCH, Weinheim, 2004)]. Recent interferometry. Experiments in the four-wave mixing setup performed in a multiphoton regime [L. Wang et al., PHYSICAL REVIEW A 95, 013811 (2017)] indicate the reduction of quantum correlations is proportional to the square of the transmission ratios of the light beam intensities that characterize such losses. Single photon experiments have substantially lower transmission ratios and are therefore robust against such losses. Finally, it has been shown that similar biphoton spectroscopy measurements are robust against the external noise at the detection stage such as background thermal radiation [Kalachev et al., Las. Phys. 4, 10, 722 (2006)], even under the signal-to-noise ratio reaches 1/30.”

Q3. It is likely that the authors want to rely on post-selection, but to me right now they are not clear about it yet.

A3. We would like to highlight that our method does not rely on post selection. Post selected photons require the arrival of combinations of modes from which the desired one (or more) is chosen to satisfy some initial constraint, e.g., minimization of uncertainty, etc. While providing an insightful proposition, it is strongly dependent on the ability to sort between modes – introducing additional source of error. Here, the coincidence simply vanishes upon losses of the last interaction eliminating the sorting problem as well as the background.