nature portfolio

Peer Review File

Ladder of Eckhaus instabilities and parametric conversion in chi(2)~microresonators

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Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors investigated the Eckhaus instability in parametric down-conversion microresonators. More precisely, they show evidence of a discrete sequence of the Eckhaus by varying the signal-idler frequencies. They consider both degenerate and non-degenerate optical conversion systems and they use generic coupled-mode equations to investigate the formation of the ladder of Eckhaus type of instability. Finally, they provide realistic parameters based on the high-Q quadratic whispering gallery microresonators.

This manuscript is almost perfect and fulfills the high standards of Communications Physics. In the following I have some minor concerns that can be easily fixed before the publication of this interesting contribution:

a) The authors have neglected the effect of temporal drift caused by the group velocity mismatch, while it might not be the case in real conditions. The effect of a nonzero group velocity mismatch is to introduce an asymmetry, and thus to create a drifting motion with a constant velocity of the emerging solutions through the Eckhaus instability. Does the Echakaus instability survive to temporal drift for microresonators?

b) The Eckhaus instability has been studied non only in the context of fluid mechanics but also in Kerr resonators in a paper by Périnet et al "Eckhaus instability in the Lugiato-Lefever model, Eur. Phys. J. D 71, 243 (2017)" in Topical Issue "Theory and Applications of the Lugiato-Lefever Equation", edited
by Chembo et al. In this contribution, the authors used continuation methods to reveal
the existence of a set of wave instability branches. The organization of these branches suggests the existence of an Eckhaus scenarioin the weakly nonlinear regime. The continuation in the highly nonlinear regime showed that the furthest branches become unstable through a Hopf bifurcation.

c)The authors consider situations where parametric down frequency conversion occurs with zero loss, while it might not be the case in real conditions. They indicate in the abstract that they consider "law loss microresonators". Could the authors provide realistic values for whispering gallery microresonators of the losses even if they are law?

d)Some contributions on parametric down-conversion versus transverse effects [Phys. Rev. E 56, 6524 (1997) , Phys. Rev. A 59, R2575(R) (1999)] worth to be mentioned together with Ref. [25-28].

Reviewer #2 (Remarks to the Author):

The current report addresses one of the important question in quadradic frequency comb generation through optical parametric down conversion, especially on its frequency range. With the third order nonlinearity, the frequency comb generation is well described by the well-known LLE equation. However, there is lack of such governing equation for the second order nonlinearity. The authors theoretically investigate a slowly varying type coupled equation model. Based on this theory framework, they have demonstrated an interesting type of Eckhaus instabilities during the OPO processes. I personally believe such model will be beneficial for quadradic frequency comb generation which is an emerging field in the recent years. But before I recommend it for publication, there're some concerns and questions that have to be addressed:

1. After reading the text, I'm still confusing on how to generate "continuous" comb verse "stagged comb" as shown in Fig. 1, is it just the power dependence as shown in the results of Eckhaus instabilities in Fig. 3 & 4.
2. It's also hard for me to grasp the key idea behind Fig. 2, since many of its related results are presented later in Fig. 3 & 4, I would suggest the authors re-think about the rearrange the figure's order.
3. From a experiment perspective, I would like to see more discussion/results on dispersion, given the two spectra separated out by two-harmonics, especially with some materials like LNOI.
4. Can the authors also comment on the breather states shown in Fig. 3, especially its difference to the counterpart with kerr nonlinearity.
5. As an experimentalist, I would suggest the authors to reduce some non-essential formulism, and put them into the supplement. The authors may consider focusing on some essential results like Eq. 15. Too much formulism makes the paper hard to follow, not general enough for the audience of the current journal, more suitable for PRA type. I would suggest put more physical insights and elaboration instead.

Reviewer #3 (Remarks to the Author):

The manuscript results are new, and the presented theory is based on the recent and well-cited experimental results on frequency conversion in microresonator. Association of the OPO tuning to the hydrodynamic instabilities gives to work the cross-disciplinary flavour and appeal. Therefore, the work appears timely and important. Overall, the manuscript is well written and will be interesting for broad community of researchers working in the field of optical microresonators and optical frequency comb generation. I recommend publication of this manuscript after the following minor revisions.

1. The linewidth parameters in the visible and infrared parts of the spectrum can differ significantly. However, the values used are 1 and 2 MHz. It'll be beneficial to see a comment on how results and conclusions could change if both or one of the kappas go up by one or two orders of magnitude.

2. Equations 16 for a_nu and a_-nu could be missing an arbitrary phase. Adding this phase is unlikely to change anything but will make the equation more rigorous.

3. How is it possible that q_0=1 and q_\ne 0 is 2? I would expect the structure of the governing equations to assume some symmetry, and a comment on this is needed.

4. The letter (c) in Fig. 3c should be put in the top corner to match the other panels.

5. A sentence about breathers after Eq 22, says that the breathers are 'exempt'. However, Fig. 3b seems to show a small breather cascade similar, but inverse, to the usual OPO. Clarifying this could be helpful.

6. nu_min in Fig. 5 should be shown with the line style different from nu_max. I was not sure which one was where.

Dear Editor,

We thank the referees for carefully considering our results and recommendations to publish after revisions. Below we include the reviews in full and provide the point by point responses highlighted with yellow.

Authors

Reviewer #1 (Remarks to the Author):
The authors investigated the Eckhaus instability in parametric down-conversion microresonators. More precisely, they show evidence of a discrete sequence of the Eckhaus by varying the signal-idler frequencies. They consider both degenerate and non-degenerate optical conversion systems and they use generic coupled-mode equations to investigate the formation of the ladder of Eckhaus type of instability. Finally, they provide realistic parameters based on the high-Q quadratic whispering gallery microresonators.

This manuscript is almost perfect and fulfills the high standards of Communications Physics. In the following I have some minor concerns that can be easily fixed before the publication of this interesting contribution:

a) The authors have neglected the effect of temporal drift caused by the group velocity mismatch, while it might not be the case in real conditions. The effect of a nonzero group velocity mismatch is to introduce an asymmetry, and thus to create a drifting motion with a constant velocity of the emerging solutions through the Eckhaus instability. Does the Echakaus instability survive to temporal drift for microresonators?

Reply: *We do not neglect the group velocity mismatch, see the D_1a-D_1b term in Eq. 6. This mismatch is large and it is the dominant term, see, e.g., the line 14 in the abstract. This condition is typical for the whispering gallery microresonators. All our numerical modeling results fully account for it. Our analytical theory is approximate and uses the fact it is a large parameter. Thus, we demonstrate the Eckhaus instability in the case of large D_1a-D_1b. However, the growth rate of the Eckhaus instability, i.e., the real part of the respective eigenvalue, does not depend on D_1a-D_1b in the leading order, see Eqs 22 and 23 and the excellent match with numerics in Fig. 4a and also in Fig. 3. The large D_1a-D_1b is responsible for the fast oscillations, see Eq. 10. We have now explicitly addressed this point in the Conclusions section, see lines 438-447.*

The drift mentioned by the referee happens in the coordinate space, while we work in the Fourier space which is most appropriate to study frequency conversion. On the transformation to the coordinate space one would see the drift, which is of course relative to the choice of the reference frame.

b) The Eckhaus instability has been studied non only in the context of fluid mechanics but also in Kerr resonators in a paper by Périnet et al "Eckhaus instability in the Lugiato-Lefever model, Eur. Phys. J. D 71, 243 (2017)" in Topical Issue "Theory and Applications of the Lugiato-Lefever Equation", edited by Chembo et al. In this contribution, the authors used continuation methods to reveal the existence of a set of wave instability branches. The organization of these branches suggests the existence of an Eckhaus scenario in the weakly nonlinear regime. The continuation in the highly nonlinear regime showed that the furthest branches become unstable through a Hopf bifurcation.

Reply: *We list a variety of optical resonators with chi(3) (Kerr) nonlinearity where this class of instabilities has been discussed in the lines 80-83 of the introduction. The Perinet paper is now cited, see Ref. 41.*

c)The authors consider situations where parametric down frequency conversion occurs with zero loss, while it might not be the case in real conditions. They indicate in the abstract that they consider "law loss microresonators”. Could the authors provide realistic values for whispering gallery microresonators of the losses even if they are law?

Reply: *Losses (parameters kappa_a and kappa_b) are fully accounted for and are not simplified in any way. Their values fit a range of the bulk-cut microresonators and are made explicit in the caption of Fig. 1. To simplify notations we have included kappa’s into the complex valued detuning parameters, see Eq. 13. The revised text around Eq. 13 makes this point unambiguous for the readers. The Eckhaus instability growth rate, see Eq. 22, includes losses explicitly.*

d)Some contributions on parametric down-conversion versus transverse effects [Phys. Rev. E 56, 6524 (1997) , Phys. Rev. A 59, R2575(R) (1999)] worth to be mentioned together with Ref. [25-28].

Reply: *Both of these papers are now cited, see Refs. 31,32.*

Reviewer #2 (Remarks to the Author):

The current report addresses one of the important question in quadradic frequency comb generation through optical parametric down conversion, especially on its frequency range. With the third order nonlinearity, the frequency comb generation is well described by the well-known LLE equation. However, there is lack of such governing equation for the second order nonlinearity. The authors theoretically investigate a slowly varying type coupled equation model. Based on this theory framework, they have demonstrated an interesting type of Eckhaus instabilities during the OPO processes. I personally believe such model will be beneficial for quadradic frequency comb generation which is an emerging field in the recent years. But before I recommend it for publication, there’re some concerns and questions that have to be addressed:

1. After reading the text, I’m still confusing on how to generate “continuous” comb verse “stagged comb” as shown in Fig. 1, is it just the power dependence as shown in the results of Eckhaus instabilities in Fig. 3 & 4.

Reply: *The first chapter of the Conclusions section addresses this question, see lines 414-432. The lines 427-433 have been added during this revision and shed more light on this. We concluded that the low loss and significant mismatch of the group velocities (repetition rates) considered in this work create a strong preference for the staggered combs and non-degenerate OPO regimes. Generally, the microresonator parameter space is at least 6-dimensional (pump, detuning, phase mismatch, dispersion, rep rate difference and losses) and, therefore, understanding of their frequency conversion properties requires careful application of the framework developed here on the case by case basis. The relevant references have also been updated, see line 433.*

2. It’s also hard for me to grasp the key idea behind Fig. 2, since many of its related results are presented later in Fig. 3 & 4, I would suggest the authors re-think about the rearrange the figure’s order.

Reply: *Figure 2 is the only figure that shows the boundaries of stability of the no-OPO (left) and degenerate OPO (right) states in the detuning-power parameter space. These two parameters are the ones that are controlled by tuning the laser and without altering the resonator. Figures 3 and 4 select specific values of the laser power and phase-mismatch parameters and demonstrate how the OPO spectrum evolves with detuning. Hence, there is no repetitive data presentation. We have improved the readability of figures 2,3,4 by explicitly stating the pump power levels within the figure panels and providing the worded descriptions for the axes.*

3. From a experiment perspective, I would like to see more discussion/results on dispersion, given the two

spectra separated out by two-harmonics, especially with some materials like LNOI.

Reply: *We have used the most typical normal dispersion. Changing dispersion signs to anomalous or the mixed one requires preparing a separate manuscript. The loss and nonlinearity are also changing together with geometry, pump wavelength, and dispersion. The variety of regimes and options there is large.*

4. Can the authors also comment on the breather states shown in Fig. 3, especially its difference to the counterpart with kerr nonlinearity.

Reply: *Here we are considering the near-phase-matching between the mu=0 modes in the pump and signal fields, i.e., epsilon_0 is order of the linewidth. It leads to the Rabi-like oscillations between the green and red mu=0 modes, i.e., to the breather states, see new text added in lines 262-268. The chi(2) Rabi oscillations for mu>>1 were reported by us in Refs. 46,47. Therefore, the physics behind the breathers in our work could be substantially different from the Kerr (chi-3) case.*

5. As an experimentalist, I would suggest the authors to reduce some non-essential formulism, and put them into the supplement. The authors may consider focusing on some essential results like Eq. 15. Too much formulism makes the paper hard to follow, not general enough for the audience of the current journal, more suitable for PRA type. I would suggest put more physical insights and elaboration instead.

Reply: *The formalism introduced and applied by us is new and its description is reduced to the minimum allowing understanding of the physics discussed and results derived, so that it can be extended to the cases with other dispersion signs, losses, etc in future investigations.*

Reviewer #3 (Remarks to the Author):
The manuscript results are new, and the presented theory is based on the recent and well-cited experimental results on frequency conversion in microresonator. Association of the OPO tuning to the hydrodynamic instabilities gives to work the cross-disciplinary flavour and appeal. Therefore, the work appears timely and important. Overall, the manuscript is well written and will be interesting for broad community of researchers working in the field of optical microresonators and optical frequency comb generation. I recommend publication of this manuscript after the following minor revisions.

1. The linewidth parameters in the visible and infrared parts of the spectrum can differ significantly. However, the values used are 1 and 2 MHz. It'll be beneficial to see a comment on how results and conclusions could change if both or one of the kappas go up by one or two orders of magnitude.

Reply: *Changing the dissipation values from the currently used ones only leads to the asymmetry of the two threshold minima seen in Fig. 2.*

2. Equations 16 for a_nu and a_-nu could be missing an arbitrary phase. Adding this phase is unlikely to change anything but will make the equation more rigorous.

Reply: *We agree with this, but since it does not change anything, we choose to keep our formalism in the concise form, which is also proposed by the previous reviewer.*

3. How is it possible that q_0=1 and q_\ne 0 is 2? I would expect the structure of the governing equations

to assume some symmetry, and a comment on this is needed.

Reply: *This relates to the structure of the last equation in Eq. (15), since the mu>0 and mu<0 terms are the same, and mu=0 is not repeated. Explaining this would probably distract a reader from the main line of our research.*

4. The letter (c) in Fig. 3c should be put in the top corner to match the other panels.

Reply*: This has been fixed.*

5. A sentence about breathers after Eq 22, says that the breathers are 'exempt'. However, Fig. 3b seems to show a small breather cascade similar, but inverse, to the usual OPO. Clarifying this could be helpful.

Reply*: This correlates topically with the comment by the previous referee and the relevant text is now included, see lines 262-268.*

6. nu_min in Fig. 5 should be shown with the line style different from nu_max. I was not sure which one was where.

Reply*: We have applied the respective changes to Fig. 5.*

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript has been revised suitably in accordance with the above mentioned critics/ Comments , and therefore I support its publication as it is.

Reviewer #2 (Remarks to the Author):

The authors have well addressed all my previous concerns & comments, now the current manuscript is well written and clearly demonstrates the important comb generation in a x^2 micro-resonator, which is a emerging field recently.

there's one minor point: Fig. 2 & Fig. 5 uses the W as the unit for the laser power, but with dB scale. usually dBm is used for that purpose. it's a bit awkward here.

Reviewer #3 (Remarks to the Author):

The authors have addressed the reviewers' concerns and the manuscript can be published in the present form.

Dear Editor,

We thank the referees for recommendations to publish.

Authors

Reviewer #2 (Remarks to the Author):

there's one minor point: Fig. 2 & Fig. 5 uses the W as the unit for the laser power, but with dB scale. usually dBm is used for that purpose. it's a bit awkward here.

***Reply:** We have now replaced dB with dBm*