nature portfolio

Peer Review File

Continuous time crystal coupled to a mechanical mode as a cavity-optomechanics-like platform

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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 authors report an experiment in which a continuous time crystal is coupled to a mechanical mode of a liquid surface. This represents the first concept and experimental realization of a time crystal playing a role analogous to that of the electromagnetic field in an optical cavity, where radiation is coupled to the mechanical motion of one of the cavity mirrors. This opens the door to constructing complex devices in which a mechanical system both influences and is influenced by a spontaneously periodically evolving system - a time crystal. If the mechanical device were sufficiently "small", the situation would be even more intriguing, as it would enter the regime of quantum behavior of the mechanical element, a direction the authors briefly mention in the Discussion and Outlook.

In addition to demonstrating the coupling of a time crystal with a mechanical subsystem, it is worth emphasizing that the time crystal acts as a detector of the mechanical subsystem behavior. That is, monitoring the modulation of the time crystal oscillation frequency allows for the characterization of the resonance parameters of the surface mechanical mode. The time crystal is also used as a thermometer, enabling measurement of the energy deposited into the mechanical mode that is dissipated, which leads to heating of the superfluid He-3. This application-related aspect of the time crystal constitutes an additional achievement of the authors.

I have the following specific comments regarding the manuscript:

1/ Ref. [8] describes an experiment with a Bose-Einstein condensate in an elongated trap, where an excited transverse mode leads to the spontaneous excitation of a longitudinal mode that forms a time crystal. If the transverse mode is interpreted as a mechanical system coupled to the time crystal (i.e., the longitudinal mode), then one could view this experiment as an instance of a time crystal coupled to a mechanical mode, similar to what is explored in the current manuscript. I would appreciate the authors' comments on the similarities and differences between these two situations.

2/ The description of the experimental mechanical forcing in the article and in the Methods section is not entirely clear for non-experts. Without a schematic, it is difficult to visualize the exact geometry, orientation, and method of mechanical driving applied to the system.

3/ The manuscript discusses gravity waves on the surface of the superfluid, and also mentions gravitational waves toward the end of the article in the context of potential future applications of time crystals coupled to mechanical systems. In the latter case, to be sure which kind of waves are being referred to, one needs to check the cited reference. It would be helpful to ensure that both experts in superfluid physics and more general readers are not left confused.

4/ In Fig. 2, results for the bulk time crystal are shown using blue symbols, and those for the surface time crystal using red symbols. In Fig. 3, however, both this convention and its reverse appear, which may cause confusion. The reader would benefit from a consistent color-coding scheme across all figures.

These comments do not affect my very positive overall assessment of the work. I believe that this first experimental realization of coupling a time crystal to a mechanical mode fully merits publication in Nature Communications.

Reviewer #2

(Remarks to the Author)
The paper titled “Time crystal optomechanics” by J. T. Mäkinen et al. presents a study that bridges the concepts of time crystals and ‘optomechanics’. The authors demonstrate how a magnon Bose–Einstein condensate (BEC) in superfluid helium-3 (3He) can function as a time crystal and be coupled to a mechanical resonator, specifically a gravity wave mode on a nearby liquid surface. This coupling forms an ‘optomechanical’ system, allowing the exploration of time crystal dynamics through mechanical interactions. The key novelty of this work as claimed, compared to the authors’ prior publications (PRL 2018, Nature Materials 2021, Nature Communications 2022), lies in the introduction of ‘optomechanical’ coupling. The authors convincingly show how the mechanical motion of the superfluid surface modulates the trapping potential of the magnon BEC, leading to frequency sidebands in the precession signal—characteristic of parametrically modulated systems and consistent with optomechanical interaction. The manuscript is well written, the experimental work is convincing, and the results are original. However, one key concern is the use of the term optomechanics in the manuscript. As the authors themselves acknowledge in the introduction, the system exhibits an ‘optomechanics-like’ phenomenon rather than conventional cavity optomechanics. Therefore, using optomechanics directly in the title may be misleading. In the Hamiltonian description, the coupling is more accurately described as magnon–ripplon interaction, rather than true optomechanical coupling as seen in photon–phonon systems. I recommend the authors reconsider the terminology in both the title and the main text to ensure it accurately reflects the physical nature of the coupling and avoids potential confusion for readers from the optomechanics community. The other important aspect missing from the current manuscript—particularly if the authors wish to retain the term optomechanics—is a discussion of how the time crystal dynamics influence the mechanical motion of the superfluid surface. Mutual backaction is a defining feature of optomechanical systems, yet the manuscript currently only addresses how the surface motion affects the time crystal. The authors should also examine the reciprocal effect, such as whether the magnon condensate exerts any measurable influence on the surface mode dynamics. The paper could be suitable for publication in Nature Communications after the authors address the points raised and make the appropriate revisions as outlined in the detailed comments below.

1. The authors emphasize that their system realizes a continuous time crystal. To strengthen this point, it would be helpful to elaborate further in the main text on how the symmetry breaking in this system fundamentally differs from that in periodically driven (discrete) time crystals instead of simply citing the author’s previous work. For instance, is there a well-defined order parameter, phase diagram, or detuning response that illustrates the spontaneous breaking of continuous time-translation symmetry? Including such discussion would help clarify the distinction and highlight the novelty of the work.
2. While the manuscript cites key works on discrete time crystals, it would benefit from citing references on continuous time crystals—especially as the magnon BEC is presented as a non-Floquet time crystal. More foundational studies on quantum and classical continuous time crystal should also be cited. This will help clarify the broader context and underscore the novelty of coupling a continuous time-crystalline magnon phase to a mechanical oscillator, reinforcing the cross-platform relevance of the results presented in this manuscript.
3. It would be helpful if the authors could provide a clear time trace indicating when the RF pump is turned on and off, when the acoustic wave modulation begins, and when the time-crystalline state emerges. This would greatly aid readers in understanding the temporal sequence of events and the onset dynamics of the observed time-crystal optomechanics behavior.
4. The authors should clarify the apparent discrepancy between line 35 of the main text, which states that the precession frequency of the time-crystal wavefunction is close to ωnmr = 2π×833 kHz, and Figure 1b, which shows an oscillation period of approximately 0.3 ms (~300 Hz). It would be helpful to explain how these two frequencies relate and whether they refer to different aspects of the system’s dynamics.
5. In the inset of Figure 1c, is the red line a fit to the data? If so, please specify the fitting function or model used, and include the corresponding formula in the caption or main text for clarity.
6. While the authors describe how the superfluid surface motion influences the time crystal oscillation, they should also discuss the reciprocal effect—how the time crystal dynamics or magnon impact the mechanical motion of the superfluid surface, analogous to how optical radiation pressure drives a mechanical mirror in conventional optomechanics. Whether the magnon condensate exerts any measurable influence on the surface mode dynamics. Addressing this mutual interaction would provide a more complete picture of the coupled system.
7. The authors should also address whether this platform can eventually access quantum optomechanical regimes and measure the quantum ripplon, and how.
8. Minor issues:
a) In the caption of Fig. 3d, the phrase “static tilt tilt angle” contains a redundancy. Please revise this repetition for clarity and check the entire manuscript for similar typos.
b) There appears to be a typo error in the temperature difference equation on line 231. The authors should correct this for clarity and accuracy.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my comments in a satisfactory manner. In my opinion, the revised version of the article is ready for publication in Nature Communications.

Reviewer #2

(Remarks to the Author)
The authors have answered all my questions and I'm happy to recommend its publication in Nature Communications.

Dear Editor, July 24, 2025

We are pleased to resubmit our manuscript for publication in Nature Communications. Below we describe our point-by-point responses (black) to the reviewer comments (blue).

Referee comments:

Reviewer #1 (Remarks to the Author):

The authors report an experiment in which a continuous time crystal is coupled to a mechanical mode of a liquid surface. This represents the first concept and experimental realization of a time crystal playing a role analogous to that of the electromagnetic field in an optical cavity, where radiation is coupled to the mechanical motion of one of the cavity mirrors. This opens the door to constructing complex devices in which a mechanical system both influences and is influenced by a spontaneously periodically evolving system - a time crystal. If the mechanical device were sufficiently "small", the situation would be even more intriguing, as it would enter the regime of quantum behavior of the mechanical element, a direction the authors briefly mention in the Discussion and Outlook.

In addition to demonstrating the coupling of a time crystal with a mechanical subsystem, it is worth emphasizing that the time crystal acts as a detector of the mechanical subsystem behavior. That is, monitoring the modulation of the time crystal oscillation frequency allows for the characterization of the resonance parameters of the surface mechanical mode. The time crystal is also used as a thermometer, enabling measurement of the energy deposited into the mechanical mode that is dissipated, which leads to heating of the superfluid He-3. This application-related aspect of the time crystal constitutes an additional achievement of the authors.

We thank the referee for the accurate summary of our manuscript.

I have the following specific comments regarding the manuscript:

1/ Ref. [8] describes an experiment with a Bose-Einstein condensate in an elongated trap, where an excited transverse mode leads to the spontaneous excitation of a longitudinal mode that forms a time crystal. If the transverse mode is interpreted as a mechanical system coupled to the time crystal (i.e., the longitudinal mode), then one could view this experiment as an instance of a time crystal coupled to a mechanical mode, similar to what is explored in the current manuscript. I would appreciate the authors' comments on the similarities and differences between these two situations.

In both cases the time crystal couples to a mechanical mode. In the case of Ref. [8] the time crystal itself is a phononic excitation that closely resembles Faraday waves (as pointed out in the manuscript) but can be interpreted as a discrete space-time crystal, while in our case the continuous time crystal is magnonic, and exists also in the absence of mechanical forcing.

The focus of the present manuscript is to demonstrate how a continuous time crystal can be coupled to mechanical degree of freedom. We have no immediate suggestions on how to apply similar optomechanical considerations to period-doubling discrete time crystals presented in Ref [8]. We do acknowledge that this is an interesting research direction that ought to be explored in the future, and have added discussion and references to the 'Conclusions and outlook' section to appreciate this point.

2/ The description of the experimental mechanical forcing in the article and in the Methods section is not entirely clear for non-experts. Without a schematic, it is difficult to visualize the exact geometry, orientation, and method of mechanical driving applied to the system.

We have added a simplified schematic of the mechanical support structure and drive as a new Extended Data Figure 1 to aid in visualisation. We refer to this figure in appropriate places in the main text, as well as in the Methods section. We also specify the location of the sample relative to the air springs more accurately, which is reflected in the new figure as well as in the Methods section.

3/ The manuscript discusses gravity waves on the surface of the superfluid, and also mentions gravitational waves toward the end of the article in the context of potential future applications of time crystals coupled to mechanical systems. In the latter case, to be sure which kind of waves are being referred to, one needs to check the cited reference. It would be helpful to ensure that both experts in superfluid physics and more general readers are not left confused.

We have replaced each word 'gravitational' in the manuscript by 'gravity' and removed mentioning of gravitational waves to avoid confusion.

4/ In Fig. 2, results for the bulk time crystal are shown using blue symbols, and those for the surface time crystal using red symbols. In Fig. 3, however, both this convention and its reverse appear, which may cause confusion. The reader would benefit from a consistent color-coding scheme across all figures.

The color-coding scheme is consistent across all figures. Unfortunately, the labels in the caption of Fig. 3a were reversed by mistake. This has now been fixed.

These comments do not affect my very positive overall assessment of the work. I believe that this first experimental realization of coupling a time crystal to a mechanical mode fully merits publication in Nature Communications.

We thank the referee for the positive assessment of our work.

Reviewer #2 (Remarks to the Author):

The paper titled "Time crystal optomechanics" by J. T. Mäkinen et al. presents a study that bridges the concepts of time crystals and 'optomechanics'. The authors demonstrate how a magnon Bose–Einstein condensate (BEC) in superfluid helium-3 (3He) can function as a time crystal and be coupled to a mechanical resonator, specifically a gravity wave mode on a nearby liquid surface. This coupling forms an 'optomechanical' system, allowing the exploration of time crystal dynamics through mechanical interactions. The key novelty of this work as claimed, compared to the authors' prior publications (PRL 2018, Nature Materials 2021, Nature Communications 2022), lies in the introduction of 'optomechanical' coupling. The authors convincingly show how the mechanical motion of the superfluid surface modulates the trapping potential of the magnon BEC, leading to frequency sidebands in the precession signal—characteristic of parametrically modulated systems and consistent with optomechanical interaction. The manuscript is well written, the experimental work is convincing, and the results are original.

We thank the referee for the positive evaluation of our manuscript.

However, one key concern is the use of the term optomechanics in the manuscript. As the authors themselves acknowledge in the introduction, the system exhibits an 'optomechanics-like' phenomenon rather than conventional cavity optomechanics. Therefore, using optomechanics directly in the title may be misleading. In the Hamiltonian description, the coupling is more accurately described as magnon–ripplon interaction, rather than true optomechanical coupling as seen in photon–phonon systems. I recommend the authors reconsider the terminology in both the title and the main text to ensure it accurately reflects the physical nature of the coupling and avoids potential confusion for readers from the optomechanics community.

We have taken the referee concerns into account. We reformulated the title and the essential part of the text using "optomechanical-like" wording. Nevertheless, since we are introducing a new platform with potentially strong impact, we prefer to also suggest a concise name for it. In the Discussion and Outlook section, we propose the name "time crystal optomechanics". Hopefully by that point in the paper, the meaning will not be confusing. We have considered other alternatives. The term "magnomechanics" does exist, but we believe our approach will work for different continuous time crystals, not necessarily magnon-based. And the construction "timecrystallomechanics" we have found too complicated.

The other important aspect missing from the current manuscript—particularly if the authors wish to retain the term optomechanics—is a discussion of how the time crystal dynamics influence the mechanical motion of the superfluid surface. Mutual backaction is a defining feature of optomechanical systems, yet the manuscript currently only addresses how the surface motion affects the time crystal. The authors should also examine the reciprocal effect, such as whether the magnon condensate exerts any measurable influence on the surface mode dynamics. The paper could be suitable for publication in Nature Communications after the authors address the points raised and make the appropriate revisions as outlined in the detailed comments below.

We thank the referee for the insightful comments. To address this concern, we have added a whole new paragraph (next to the last in the manuscript). The basic message is that the present system is not optimized for the optomechanical protocols dependent on the backreaction and control of the mechanical mode via cavity due to large mass and relatively low Q value of the mechanical mode. This limitation is, however, not fundamental. It can be overcome using a nanomechanical oscillator as a mechanical mode, as outlined in the manuscript. Such improvement will hopefully allow to demonstrate side-band cooling, formation of non-classical states of motion and quantum-enhanced position sensing. This is a task for future research.

1. The authors emphasize that their system realizes a continuous time crystal. To strengthen this point, it would be helpful to elaborate further in the main text on how the symmetry breaking in this system fundamentally differs from that in periodically driven (discrete) time crystals instead of simply citing the author's previous work. For instance, is there a well-defined order parameter, phase diagram, or detuning response that illustrates the spontaneous breaking of continuous time-translation symmetry? Including such discussion would help clarify the distinction and highlight the novelty of the work.

This aspect is not in the focus of the current manuscript – it has been discussed in detail in our previous work (PRL 2018, Ref. 9 of the manuscript) and more recently in our review (APL 2024, Ref. 15). We have, however, extended the discussion of the CTC in the manuscript and separated it to its own section. We have put special emphasis on the nature of the time translation symmetry breaking in our system and its manifestation in the experiments.

2. While the manuscript cites key works on discrete time crystals, it would benefit from citing references on continuous time crystals—especially as the magnon BEC is presented as a non-Floquet time crystal. More foundational studies on quantum and classical continuous time crystal should also be cited. This will help clarify the broader context and underscore the novelty of coupling a continuous time-crystalline magnon phase to a mechanical oscillator, reinforcing the cross-platform relevance of the results presented in this manuscript.

We have added more references to experimental realizations of continuous time crystals in the manuscript (covering all known to us works, Refs. 10-14,16).

3. It would be helpful if the authors could provide a clear time trace indicating when the RF pump is turned on and off, when the acoustic wave modulation begins, and when the time-crystalline state emerges. This would greatly aid readers in understanding the temporal sequence of events and the onset dynamics of the observed time-crystal optomechanics behavior.

We have clarified the sequence of events and their duration in the new "Continuous time crystal" section. We have decided not to include a figure which is very similar to published in Refs. 9 and 15.

4. The authors should clarify the apparent discrepancy between line 35 of the main text, which states that the precession frequency of the time-crystal wavefunction is close to $\omega_{\rm nmr}$ = 2p×833 kHz, and Figure 1b, which shows an oscillation period of approximately 0.3 ms (300 Hz). It would be helpful to explain how these two frequencies relate and whether they refer to different aspects of the system's dynamics.

In our notation $\omega_{\rm NMR}$ referred to the resonance frequency of our NMR tank circuit, $\sim$833 kHz, which is by design close to the Larmor frequency in our experiments. To avoid confusion, we have removed this notation from the revised version, and speak only about the CTC frequency (which is a couple hundred Hz higher than the Larmor frequency). The signal shown in the figure is recorded after downcoversion of the frequency from 833kHz to about 3kHz. This is now clearly explained in the Methods section and referenced in the figure caption.

5. In the inset of Figure 1c, is the red line a fit to the data? If so, please specify the fitting function or model used, and include the corresponding formula in the caption or main text for clarity.

The red line is indeed a fit to the data. The model follows from Eq. (1) in the main text, but the detailed description of the fitting procedure is given in the 'Signal analysis' section in Methods. Clarifications and reference to Methods added to the figure caption.

6. While the authors describe how the superfluid surface motion influences the time crystal oscillation, they should also discuss the reciprocal effect—how the time crystal dynamics or magnon impact the mechanical motion of the superfluid surface, analogous to how optical radiation pressure drives a mechanical mirror in conventional optomechanics. Whether the magnon condensate exerts any measurable influence on the surface mode dynamics. Addressing this mutual interaction would provide a more complete picture of the coupled system.

As explained above, we have added a paragraph addressing this question.

7. The authors should also address whether this platform can eventually access quantum optomechanical regimes and measure the quantum ripplon, and how.

We believe that the most feasible way to reach the quantum regime is to replace ripplons with phonons in a nanomechanical oscillator. This also moves the setup closer to conventional optomechanics. We describe this path forward in the last paragraph of the main text and actually plan to pursue it in future work. To address ripplons in the quantum regime, the geometry should be significantly changed as short-wavelength higher-frequency ripplons are required. This will require also new excitation methods and coupling schemes, as interactions within superfluid ^{3}He change significantly at length scales below few micrometers. Additionally, there is no knowledge about damping of such short-wave ripplons. On the other hand, we have successfully operated nanomechanical devices in superfluid ^{3}He at the lowest temperatures with three orders of magnitude higher frequencies and two orders of magnitude higher Q values, than the surface mode in this work. And those parameters can still be improved.

8. Minor issues: a) In the caption of Fig. 3d, the phrase "static tilt tilt angle" contains a redundancy. Please revise this repetition

for clarity and check the entire manuscript for similar typos. b) There appears to be a typo error in the temperature difference equation on line 231. The authors should correct this for clarity and accuracy.

We have corrected the typos pointed out by the referee, and double-checked our manuscript for similar mistakes.

Yours sincerely,

Jere T. Mäkinen
(on behalf of all authors)