nature portfolio

Peer Review File

Multicomponent Kardar-Parisi-Zhang Universality in Degenerate Coupled Condensates

Corresponding Author: Mr Harvey Weinberger

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 Weinberger, Comaron, and Szymanska discusses the phase diagram of two-component one-dimensional exciton-polariton systems with two independent phase rotation symmetries, and an exchange symmetry between the components. A combination of numerical analysis and effective field theory methods is used. The phase diagram is found to be rich, involving fragmented and homogeneous regions. A focus is on the long-wavelength behavior in the homogenous phases. Key effects gorverning the physics in these regimes are KPZ scaling behavior as well as the appearance of space-time vortices. The competition of these effects is resolved for realistic parameter regimes. Special attention is then paid to the discussion of smooth long-wavelength fluctuations, where effects like emergent equilibrium behavior and other emergent symmetries (such as present on the Cole-Hopf line) are established based on a one-loop renormalization group calculation.

The work represents a comprehensive study of realistic exciton-polariton systems and certainly has a place in the landscape of modern literature on the topic. It is scholarly written and all results are well justified. While it does not provide fundamentally new physical insight (the scaling and space-time vortex phenomenology has been established previously, coupled U(1) x U(1) exciton-polariton condensates have been investigated, and the RG flows of the coupled KPZ equations have been analyzed previously), it is a timely and thorough contribution with a clear perspective to guide experimental progress. I would therefore recommend publication of this work, after a few questions and concerns below have been addressed.

1. In the introduction, it might be worth re-emphasizing that thorough known solutions of KPZ are restricted to one dimension.
2. p. 1 right: Why emphasize \mathbb{R}^{d+1}? I don’t think anything is said about d > 1.
3. The upshot of the paragraph including Eq. (2) is unclear. It seems that this discussion has already largely been lead in Ref. [51]. In addition, Eq. (3) including the parameters as function of microscopic couplings has been derived in that reference, this should be clearly marked. The latter also demonstrated the emergence of KPZ scaling in the stable regimes, which should be acknowledged. Of course the new element here is to impose the Z_2 symmetry, which is a special case of the previous results, and possibly most relevant for experiments.
4. p. 2 right: unclear what is meant by ‘ground state topology’.
5. p. 3 right, and Fig. 2: The discussion of z exponents should be made more clear, including how it is read off the figure, in particular, z = 1 for the vortex turbulent regime. Is there any explanation for this finding?
6. In Fig. 2, it was not clear to me what the difference between (i) and (ii) is in terms of parameters.

Reviewer #2

(Remarks to the Author)
The authors study 2 coupled non-linear equations that are known to be studied by Ertas-Kardar first, (the author are missing the second important reference Phys. Rev. E 48, 1228 – Published 1 August, 1993 DOI: https://doi.org/10.1103/PhysRevE.48.1228) and which recently it was also proposed to describe KPZ fluctuations in integrable chains. They study it in the context of coupled dissipative polariton condensate, so the main point is that eq 1 for the condensate wave function can be mapped to eq 5 for the coupled hydrodynamic modes. This mapping is correct when one can neglect phase fluctuations and phases vorteces, in a completely analogous way as in the case of the single condensate, a problem which is has been already quite extensively studied and which received experimental confirmation recently.

The topic and the general implications of the study are clearly interesting, but I sincerely struggle how this paper can be published in a broad journal as nat. comm. phys. There are basically no new results: the RG calculations were done, the the Cole-Hopf line was determined, in both cases by Ertas-Kardar, the mapping from coupled driven condensate to KPZ equation is known and there is nothing new to it in this paper. The paper merely establishes a mapping between the condensate parameters and the parameters in the KPZ condensate, which is an exercise for a master student. The only addition of the paper is good numerical simulations of eq 1, but these give results could already be guessed, once the mapping between 1 and 5 is known, which is, I repeat, nothing more than the usual mapping between driven condensate dynamics and KPZ equation.

Moreover, the paper is badly written, instead of reporting all changes of variables that are at some point very confusing and off the point, it should instead explain what physics leads to eq 1 and how to actually explore experimentally the parametric phase space. Also, figures are too compact and hard to read: the inset of Fig 2 iii doesn't have labels for the axis and numbers so that it is impossible to know the scale of the plot; Fig 3 iii, the lines indicating TW-GOE seems noisy for no reason, the TW-GOE distribution has an analytical prediction that can be evaluated to machine precision.

Once these things are implemented, then the paper can probably suit for publication in a more technical journal like Phys Rev A or E.

Reviewer #3

(Remarks to the Author)
The manuscript of Weinberger, Comaron, and Szymanska is devoted to a study of the multi-component Kardar-Parisi-Zhang (KPZ) equation arising from the renormalization-group (RG) analysis of the coupled Ginzburg-Landau equations for two pseudospin components of an exciton-polariton condensate. The equations include phenomenological description of the polariton nonequilibrium nature, nonlinearities, spin-anisotropic interactions, and noise. Motivated by the recent results by some of the Authors with respect to the Berezinskii-Kosterlintz-Thouless transition in two-dimensional polariton condensates [Refs 44,45], in their current work the Authors focus on a one-dimensional system and offer a comprehensive analysis of RG flows in various sets of parameters, discuss the dynamics of excitations, stability of emergent phases, and distribution of fluctuations.

After reading the manuscript several times, I conclude that the Authors have done a tremendous mathematical job and arrived at solid and new results, which are however quite badly presented. The paper is composed from the short, letter-format main text (4 pages without the Intro and References) and a great volume of Supplemental Materials (18 pages). At that, the main text is barely understandable: the number of notations and various parameters introduced throughout the text is taken to an extreme, some notations and abbreviations are never explained (see below) and some figure panels not discussed at all, which altogether looks very entangled and makes the reader completely lost by the middle of the manuscript. Frankly speaking, without studying the SM in a very careful way, the main text on its own is simply unreadable. Due to this reason, even though I would like to praise the results and all the effort spent on this work, and pose some physical questions below, I kindly ask the Authors to consider that the text is very confusing and I could misunderstand some of its parts.

1. My first concern is Eq. (1): in the derivation given in Section A of the SM, I believe, the sign in front of \mu_c in Eq. (S.1) should be minus and below (S.4) \mu = – \mu_c + i\mu_d. Generally, I am trying to say that the signs in front of the chemical potential and in front of the interaction terms should be opposite. In the main text, the Authors refer to Ref. [51] where the equation that they study was first introduced. Looking at the same equation in [51], I note that there the equation (1) is divided by imaginary unit compared to Eq. (1) of the current manuscript; so in [51] there would be a minus in front of the term corresponding to \mu_c if one restores the i. Could the Authors double check in their derivations, is it just a misprint and in their calculation they have the correct sign, or if it is a mistake, how does the correction of it influence the results? Does e.g. the expression for mean field given above Eq. (3) [or similarly Eqs. like (S.15)] change and what are the implications?

2. Why are such parameters as chemical potential, gain saturation, etc are equal for the two different components?

3. When discussing the miscible-immiscible transition, the Authors vary the rescaled intercomponent interaction v_c/u_c, as I can judge from Fig. 1(i), within approximately from -1.75 to 1.75. At the same time, they write explicitly that for the considered exciton-polariton systems v_c = –0.1u_c so their rescaled interaction is quite defined and lies in the vicinity of –0.1. In this sense, it's not totally clear and it's not discussed in the paper how really relevant is their phase diagram to polaritons. It would be useful to comment, to which other systems it can refer that could potentially exhibit all the quadrants I – IV, if they exist?

Speaking of polaritons, could the Authors place a marker within the diagram Fig. 1(i) which would indicate where do the polariton parameters belong (compared to the immiscible boundaries given in Eq. (2))? What would be the expected values for the rescaled gain saturation v_d/u_d for polaritons?

4. Is \theta with the subscript i/j and \theta with superscript alpha/beta the same thing? Or i/j and alpha/beta have a different meaning? Both notations appear in the main text and in the SM and neither is explained. Also, is it a must that \theta_i are the same for the classical and quantum parts of psi (as given in Eq. (S.12))?
How does Eq. (S.6) relate to the quantum part of psi in (S.12)? Or (S.6) only described the classical part? Are \theta_i in (S.12) the same as \theta_i in (S.6)? This is a bit confusing and should be clarified.

5. Starting from Eq. (3) and till the end of the main text, there is a continuous flow of new notations and parameters which then acquire tildas and the asterisks, and their various combinations that are all given as in-line math without any discussion of their meaning. I understand that some re-notation is required to transform the SCGLE from its initial shape to KPZ-like equations, coupled and decoupled, but the way it's currently presented is very entangled. What is the covariance matrix Delta (without the tilde) appearing in the noise description below Eq. (3) and within the first column of page 3? I did not find it even in the SM which only lists Delta with the tilde. This other Delta with the tilde isn't defined in the main text, either (only in the SM). The discussion of Fig. 1(iii) begins before the parameters X and Y plotted there are even introduced. I would like to ask the Authors to thoroughly revise the text from Eq. (3) and below, to avoid all confusion and give a proper introduction to the interaction tensor, diffusion matrix, etc. and all their consequent modifications. I believe it would help if some of the information presented currently in the SM is moved to the main text. In particular, currently the appearance of the four-parameter space {T,X,Y,Z} is not really understandable from the phrase that "the adimensionalising of coordinates in Eq. (5) leads to it", while in the SM it is naturally derived (in the main text there is not even a reference for the reader to check the SM on this matter).

6. Along the same line, I do believe that the Authors should consider moving the discussion of Flow Equations that is presented in pages 14-15 of the SM to the main text. Only after reading this far into the Supplemental file I started to understand what is going on in the main text.

7. Not a single panel of Figure 2 is discussed in the main text (neither it is referred to in the SM). It it isn't worth discussing, why is it added to the text? What should the reader learn from it? Please add a discussion. Looking at its caption, I would like to ask: do the parameters that are taken to plot it bear a connection to polariton system? Could they anyhow be related to any polariton studies or could this STV phase be observed experimentally? I did see the remark in conclusions that it can be accessed by tuning intercomponent interactions, but it is really possible, e.g. in the context of exciton-polariton systems?

8. Similarly, the panel (i) in Fig. 3 and some figures in the SM (S1, S3) are not discussed anyhow in the text (neither main nor supplemental).

In conclusion, I do believe that the results presented here are strong, interesting, and suitable for publication in Communication Physics, but the manuscript requires a major revision, possibly redistributing the materials between the main text and the SM, with the ultimate aim to make the main text understandable for a general physicist and making the SM a truly supplementing material and not the main reading like it is now. I believe that the length limits of the journal allow to have more materials and in a more structured way in the main text than it now contains. Also, I expect that should results be presented in a more comprehensible way, some other interesting physics-related (not presentation-related) question may arise. Therefore, I am happy to reread the text if the Authors decide to follow my recommendation.

Please see below also some minor (but meaningful) remarks:
- the abbreviations MSRJD, SCGLE, EW, GOE are not introduced anywhere.
- on the contrary, the abbreviation NG is introduced but not used in the main text.
- since the SM contains a lot of information regarding every part of the main text, I strongly recommend that the Authors add references to SM in every place where it is relevant; such as giving a hint that Eq. (1) is derived in SM, the excitation modes are obtained in SM, and so on, where applicable. Currently the only reference to SM [61] in the main text appears just twice with respect to covariance matrix and the STV phase analysis.
- the parameter \mu_c appearing in Eq. (1) is only introduced as chemical potential at the very end of the 3rd page of the SM; in the main text or within the derivation of Eq. (1) presented at the beginning of SM, it remains undefined.
- please accompany Eq. (1) with a comment that the index i runs over the values 1,2 (or up/down, for spins) and that \bar{i} denotes the other component.
- in the top of second column of page 2, it is written that "for repulsive interactions v_c > 0, … … …these parameters map to Quadrants II and III in Fig. 1". I believe it is a misprint since the values v_c > 0 correspond to Quadrants I and II.
- in panel (iii) of Fig. 1 there are no scales on the axes. From reading the text I concluded that the central dividing lines correspond to X=0 and Y=0, while the red line is at X=1. However, since the X=Y line isn't on the diagonal of the square, I am guessing that the scales in X and Y are different. Please put the scales on the graph.
- in Eq. (S1) of the SM, there is an extra closing round bracket in the first term, while the closing square bracket at the end of the line is missing; additionally, since the Authors are considering a strictly 1D case, I would avoid using the vectorial nabla operator.
- in Fig. S2, the labels on the panels (i), (ii) are missing, while they are mentioned in the caption.
- in the panels and caption of Fig. S3, as well as in Table 1, what does "CH" stand for?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I am satisfied with the changes made by the authors and thus recommend publication

Reviewer #2

(Remarks to the Author)
I appreciate the new version of the manuscript and the replies of the authors. I agree that now the paper explains clearly the main points, and I agree with the authors that the relevance of the X and Y variables was not addressed by Ertas Kardar. I can recommend now publication in nat comm phys.

Reviewer #3

(Remarks to the Author)
I have carefully read the new, substally reworked version of the work by Weinberger, Comaron, and Szymanska on the two-component KPZ universality, that studies the whole parameters space leading to different stability regimes and phases of the coupled stochastic GL equations (and now also in comparison with the generalised coupled GPEs) in context of exciton-polaritons and multi-component atomic Bose gases. Also I have read with an interest the discussion of the Authors with the Reviewer #2 about the relevance of their paper and their argumentation towards the novelty of the reported results with respect to previous studies.

I must admit that the revised version of the main text (which has become twice as long compared to the original version, even though the EW abbreviation is still not introduced...) is indeed a much better story which now provides the reader with some insight and discussion, where required. The results are presented in a more accessible way, and the Supplemental Material is even more extended. As said in my first report, I believe that this is a tremendous piece of job which has now become faceted very well. I would like to praise the effort of the Authors in carefully addressing all of my multiple questions and concerns, and carefully eliminating all the inconsistensies in their manuscript. The added subsection about the Experimental Visibility is much appreciated, while the detailed discussion in response to Reviewer #2 is quite structured and enlightening, so that I would suggest to use some of it to enrich the conclusions of the paper.

In conclusion, repeating the evaluation from my first report, I believe that the results presented here are new, strong and interesting. I gladly recommend the publication of this revised version (with optional changes such as finally defining EW and extending Conclusions with a discussion) in Communication Physics.

Multicomponent Kardar-Parisi-Zhang Universality in Degenerate Coupled Condensates - Response to Reviewers

H. Weinberger[1], P. Comaron[1,2], and M.H. Szymańska[1]
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Dear Editors,

We are grateful to the editors for forwarding the Referees feedback, and to the referees for their evaluations of our manuscript entitled "Kardar-Parisi-Zhang Universality in Multicomponent Degenerate Condensates".

Reviewer #1 was positive about the paper and recommended its publication in Communication Physics. They believe that *it is a timely and thorough contribution with a clear perspective to guide experimental progress*. We have since updated the manuscript for readability and have addressed further concerns.

Reviewer #2 recognises that the *topic and the general implications of the study are clearly interesting*, but suggests that it might be better suited for publication in a technical journal. We have improved the readability of the paper, and added in new results and additional experimental discussions to add to the relevance.

Reviewer #3 was positive about the results of the work and believes *that the results presented are strong, interesting, and suitable for publication in Communication Physics*, but admits that the *manuscript requires a major revision, possibly redistributing the materials between the main text and the SM* to improve clarity of the work. We have since updated the main paper with significant changes and added additional discussion, new results, and information to address the concerns of the reviewers. The supplemental materials has not been cut since we believe that it is important to have the full discussion and derivations presented for further reading.

We have made substantial changes to the manuscript, improving its coherence and readability, and believe that these additions and modifications improve the clarity of our work, and that the new version of the manuscript is suitable for publication in Nature Communication Physics.

Yours sincerely,

Harvey Weinberger, Paolo Comaron, Marzena Szymańska

COMMENTS AND MAJOR AMENDEMENTS

We thank all the referees for taking the time to review our paper on the emergence of multicomponent KPZ in driven-dissipative Bosonic systems. We found the criticisms constructive and the suggestions have allowed us to improve the content of our paper. We reaffirm that we strongly believe that if we want to understand the physics of the SCGLE, then the correct space to interpret the low-energy theory is in the parametric space given by the two adimensionalised coupling parameters (X, Y) and feel that this interpretation helps to fully characterise the phase space of the SCGLE. Going between the physical SCGLE description and the effective theory gives a lot of insight into the intricate physics which these multicomponent systems support. We have made significant changes to the manuscript mainly focused on improving readability by moving sections and discussion from the supplemental material into the main paper. The changes in the main paper are shown in blue. The structure of the paper has changed significantly reallocating sections from the supplemental materials to the main text and therefore note that the page ordering has now changed. In the following we refer to the page ordering of the updated manuscript.

In the main manuscript we have:

(A) Pg. 2 first and second column. Added additional physical insight into the derivation of the SCGLE equation. We specifically emphasise that the coupled SCGLE equation describes Bosonic modes coupled to a Markovian environment with drive and dissipation, not simply exciton-polariton systems. Provided the symmetry preserves the individual phase rotations (via incoherent pump in the case of excitons), there is a degeneracy in the multicomponent, and there is cross-coupling in components, then the multicomponent KPZ equations should still be valid. This means that a lot of the discussion should generalise to other non-equilibrium condensate platforms such as photonic condensates.

(B) Pg. 3 top. Updated FIG. 1 with an additional subfigure (c), taking the place of the flow diagram which now sits as a standalone figure. This subfigure showcases the groundstate manifolds by looking at the distribution of the real and imaginary parts of the lower polariton field, demonstrating smearing in unstable regions. This can be used to determine whether density fluctuations can be appropriately eliminated which is required to map onto KPZ. This is discussed in the main text on pg. 3 in the first paragraph on the left hand column.

(C) pg. 4 right hand column. Moved the projected renormalisation group flows to its own standalone figure (now FIG. 2) and introduced the RG terms directly after Eq. (5) which is in adimensionalised form. This should clear up discussion around the renormalisation group. Updated the figure to have scales on the X and Y axis.

(D) pg. 4 left hand column to bottom of right column. In Eq. (5), we introduce the adimensionalised form the KPZ equations for the Ertaş and Kardar equations as previously contained in the supplementary materials. This helps introduce the RG flows and gives the reader a better appreciation of the parameters which determine the universality classes. We introduce the parameters at this stage and explain the expected form of the scaling function in Eq. (6) and potential limitations.

(E) pg. 5 left column and top right column. Added additional discussion of the instability in Quadrant IV and the subsequent emergence of a spacetime vortex phase when considering compactness. We have included an additional figure in FIG. 3 showing the distribution of fluctuations in Quadrant IV.

(F) pg. 6 top figure Updated FIG. 4 which interprets the instability at the level of a compact phase. This has been updated with a larger inset for readability and colour matching showing how the STV scaling emerges as we tune $\tilde{v}_c$. We feel that interpreting this instability at both the compact and non-compact level is insightful and helpful for understanding.

(G) pg. 5 bottom right column final paragraph to pg. 6 top left column top figure. Placed the full renormalisation group equations into the main text showcasing that there are four relevant parameters however the key universal features are determined by the $\{X, Y, T\}$ subspace since Z is simply a multiplicative constant and can be absorbed into the definition of the RG time. This highlights one key result of our paper where we show that the equations close in the 4-dimensional space allowing us to understand the universal behaviour.

(H) pg. 7 left column. Reran the simulation of the Cole-Hopf line with larger effective KPZ constant $Z = 25$ showcasing that the long time skewness and kurtosis showcase expected convergence. Additionally all linear sums of TW distributions have been updated with higher numerical precision using convolution techniques.

(I) pg. 8 left column and right column. New section titled *Experimental Visiibility.* Added further discussion to the relevance for experiment. One of the key criticisms of the SCGLE is that it does not account for the dynamics of the reservoirs. Importantly, the effective interactions for u_c, v_c, u_d, v_d are not only related to direct scattering between polaritons but also account for interactions with a two-component reservoir (in an effective description after integrating out the reservoir). This shows that the even in cases where the direct bosonic-bosonic scattering is small, there are many other mechanisms to achieve coupling between the two components. We include the mapping of the KPZ parameters in the case of the multicomponent generalised Gross-Pitaevskii Equation and include these parameters in the supplemental materials though do not specifically refer to them. This leads us to discuss the limit $k_d/k_c \gg u_d/u_c$ which is appropriate for recent experiments where we map onto the fluctuation-dissipation line.

In the supplemental material we have made the following changes:

(A) pg. 1 - pg. 2 and Eq. (S.8). In Section A in the derivation from the Lindblad master equation to the KPZ parameters, we have updated the Lindblad operators to consider a momentum-dependent, but Markovian loss mechanism. This single particle loss contributes an additional term in the $q-q$ channel in the Keldysh field theory but does not change the low-energy theory since it only generates terms which couple two quantum fields and phase fields which is higher order, and thus ignored. We have also updated the introductory text in this section and explicitly write out the Lindblad Master Equation in Eq. (S.3).

(B) pg. 3 midway through the page to bottom of section on pg. 4. Updated the section on deriving the stability conditions for the miscible-immiscible transition. In this instance, we show that the results can more generally be found by solving the Routh-Hurwitz stability problem of a quartic determinental equation. This generalises the derivation previously presented which was for small k in Eq. (S.20).

(C) pg. 5 top We have included a new figure FIG. S1 which shows the Bogoliubov spectra for the same example parameters across Quadrants I-IV. This clearly shows what generates the instability from analysing the modes.

(D) pg. 6 top Updated the mapping for clarity by explicitly writing down the first and second order terms in the saddle point expansion. These are contained in Eq. (S.26) which is the first order term and Eq. (S.27). We feel that explicitly seeing the expansion gives the reader better understanding of the direction of the derivation.

(E) pg. 8 shown in blue It was pointed out that there was a misprint in the adimensionalised equations and the white noise term on the $\tilde{\theta}_2$ field should additionally have a prefactor which is $\sqrt{T}$ which has since been changed.

(F) pg. 16 bottom. We write the full flow RG equations in the original bare parameters and the adimensionalised flows. These are in agreement with (Phys. Rev. E 48, 1228 Published 1 August, 1993 as pointed out by Reviewer 2). We note that our derivation was entirely in the $\mathbb{Z}_2$ coordinates for historical reasons and this required us to automate the Wilsonian RG process in a mathematica script which are happy to release on request.

(G) pg. 18 top. We discuss subtleties around the flows of the two point correlation functions away from the FDR line. This prompts the new updated figure FIG. S2 with subfigures (a)-(c) where we show how the ratio of the same time correlations flow in RG time. FIG. S2(a) still contains the fixed points of T^* in this instance and we include the explicit form in Eq. (S.87) on pg. 17.

(H) pg. 21 top Updated FIG. S4 to include smooth linear sums of the TW distributions as done in main paper.

(I) pg. 21 -23. Included Sec. F where we map the parameters from a $\mathbb{Z}_2$ gGPE and discuss the location of current experiments in the $X-Y$ plane. We highlight the form in the XY plane and their limiting values. We discuss the portion of parameter space that the 1D experimental observation of KPZ maps onto, highlighting the potential importance of interactions when considering the distribution of fluctuations. In this section, FIG. S5 contains parametric plots of where current experimental values lie in the X-Y plane.

We have also included the following new citations in the paper:

[62] D. Ertaş and M. Kardar, *Phys. Rev. E* **48**, 1228 (1993).

Reviewer 2 pointed out that I was missing this reference that contains important discussion around the RG equations.

[63] J. Quastel and H. Spohn, *Journal of Statistical Physics* **160**, 965–984 (2015).
This is a *review* article around the universality class of the 1D KPZ equation but has some notes around dedimensionalisation.

[64] D. Roy, A. Dhar, M. Kulkarni, and H. Spohn, “Fixed points and universality classes in coupled Kardar-Parisi-Zhang equations,” (2025), arXiv:2504.04162 [cond-mat.stat-mech].
New article published on the fixed point structures of the coupled KPZ equations.

[66] F. Bornemann, *Mathematics of Computation* **79**, 871–915 (2009).
Used this article to compute the TW-GOE distributions numerically.

[67] S. Olver, *Numerische Mathematik* **122**, 305 (2012).
Used this article to compute the TW-GOE distributions.

[68] Q. Yao, E. Sedov, S. Mukherjee, J. Beaumariage, B. Ozden, K. West, L. Pfeiffer, A. Kavokin, and D. W. Snoke, *Phys. Rev. B* **106**, 245309 (2022).
This article discusses the use of a multicomponent exciton rate equation coupled to complex fields.

[71] F. Baboux, L. Ge, T. Jacqmin, M. Biondi, E. Galopin, A. Lematre, L. Le Gratiet, I. Sagnes, S. Schmidt, H. E. Treci, A. Amo, and J. Bloch, *Phys. Rev. Lett.* **116**, 066402 (2016).
This article discusses the use of flatband geometries and micropillars in exciton polariton setups.

We hope that the reviewers and editors find these changes useful and look forward to hearing further questions. We now address each of the individual concerns of the Reviewers.

RESPONSE TO REVIEWER #1

1. **Reviewer:** In the introduction, it might be worth re-emphasizing that thorough known solutions of KPZ are restricted to one dimension.

   Response: We have added a sentence in the introduction clarifying that *exact solutions* to the KPZ equation are only fully known in one spatial dimension. In particular, we emphasize that the Cole–Hopf transformation, which maps the KPZ equation to a linear diffusion equation with multiplicative noise, is only valid in $d = 1$. This is typically what physicists refer to when speaking of "solutions" to the KPZ equation. For this reason, our focus is restricted to one dimension. While the KPZ equation is often formally written for higher dimensions, it is mathematically ill-posed beyond $d = 1$, and loses key features—most notably, the *fluctuation–dissipation symmetries* that constrain its dynamics. These symmetries play a critical role in governing the renormalization group (RG) flows. Although the perturbative RG framework can be formally extended to arbitrary dimensions—as detailed in the Supplemental Materials—we do not expect such generalizations to yield quantitatively accurate predictions for physical observables beyond one dimension.

2. **Reviewer:** 2.1 right: Why emphasize $\mathbb{R}^{d+1}$? I dont think anything is said about $d > 1$.

   Response: This point was originally included to acknowledge that, although the KPZ equation is only exactly solvable in one dimension, it can still be formally written and studied in higher dimensions, and is often used by physicists as a heuristic tool beyond $d = 1$. From a theoretical standpoint, there is nothing inherently pathological about formulating the stochastic complex GinzburgLandau equation (SCGLE) in higher dimensions and carrying out the mapping to an effective KPZ-type equation. However, we agree that since our work focuses exclusively on the one-dimensional caseand for the reasons outlined in Point 1, we will revise the text to emphasize that our analysis is strictly confined to $d = 1$.

3. **Reviewer:** The upshot of the paragraph including Eq. (2) is unclear. It seems that this discussion has already largely been lead in Ref. [51]. In addition, Eq. (3) including the parameters as function of microscopic couplings has been derived in that reference, this should be clearly marked. The latter also demonstrated the emergence of KPZ scaling in the stable regimes, which should be acknowledged. Of course the new element here is to impose the $\mathbb{Z}_2$ symmetry, which is a special case of the previous results, and possibly most relevant for experiments.

   Response: We were originally not aware of Ref.[51] and then upon reading through the paper we noted that there were some large gaps in the understanding presented there. The paper suggests that, provided the condensate is stable against linear fluctuations, we should expect to see KPZ scaling. We think this is a large leap and although it is supported by a couple of numerical simulations in their paper, we do not think that it is a priori true that generalising to multicomponent systems keeps you within the universality class (especially since we lose the stationary measure, we lose the Cole-Hopf solution, we lose Galilean Symmetry which enforces the identity $\chi + z = 2$ etc.). We really stress that one way to interpret the scaling is by appealing to an RG argument (and sadly we needed to impose a $\mathbb{Z}_2$ symmetry to reduce the number parameters for the system enough to actually run the calculation and make sense of the flow equations otherwise we would have happily attempted it for a higher dimensional parameter space). In fact, it is entirely by chance that the $\mathbb{Z}_2$ coordinate system (which is relevant for linearly pumped polariton system and thus physically motivated) corresponded to a similar set of equations considered by Ertas and Kardar. We think that the discussion here is important for a few reasons : (i) since when interpreting the instability in the Quadrant picture we see that it maps to Quadrants II and III and this is quite nice as a complete narrative since the RG diagram essentially contains all the information for the low-energy theory (ii) the inequalities listed in the previous example are not exhaustive since it only bounds $\tilde{v}_c = v_c/u_c$ from above and gives no lower bound corresponding to a limit as to how attractive the interaction can be and thus does not contain the full picture. This point is stressed in the simulations for Quadrant II and III in FIG. 1(b) where we see an attractive contact interaction but instead of seeing a miscible-immiscible transition, we see that the system does not become fragmented but hosts large density fluctuations. For this reason, we think that even this section builds upon and helps generalise the results in Ref. [51] and thus choose to keep the section largely intact for completeness. We have explicitly shown that the mapping from the SCGLE has been considered previously.

4. **Reviewer:** p. 2 right: unclear what is meant by ground state topology.

   Response: This specific phrasing was used to highlight that the fluctuations are soft modes associated with the $U(1)$ symmetry breaking and there is a topological interpretation to these hydrodynamic modes. We opted for this route since, symmetry breaking from the coset construction which generalises to any continuous symmetry group. The general idea is as follows: if a system has a continuous symmetry group G that is spontaneously broken down to a subgroup H in the ground state, then the NambuGoldstone (NG) modes are parametrized by the coset space G/H. In the present case, the symmetry breaking pattern is $U(1)\times U(1) \to U(1)/\{e\}\times U(1)/\{e\}$, and so the NG manifold is simply $U(1)\times U(1) \cong S^1 \times S^1$, where S^1 denotes the unit circle. This group-theoretic framing is also useful because S^1 is compact, and its fundamental group is $\pi_1(S^1) \cong \mathbb{Z}$, corresponding directly to vortex winding numbers. We have expanded this section in the manuscript to clarify this construction and make the connection to topological defects more transparent. In light of this we also directly show what the "ground state topology" looks like for different areas of parameter space in FIG.1(c) showing that smearing occurs when density fluctuations become important. Geometrically this captures how density fluctuations are in the radial direction to the ground state manifold whereas the phase fluctuations are tangential to the manifold.

5. **Reviewer:** p. 3 right, and FIG. 2: The discussion of z exponents should be made more clear, including how it is read off the figure, in particular, z = 1 for the vortex turbulent regime. Is there any explanation for this finding?

   Response: We have added additional discussion as to how the exponent arises in the same time correlation graphs by introducing the scaling more explicitly in updated Eq. (6). This provides a natural entry point into the concept of universal scaling. In light of this, we have revised the figures and restructured this section to better highlight the significance of the vortex-dominated phase, both at the compact and non-compact levels. At the condensate level, the dynamical exponent $z = 1$ implies a growth exponent $\beta = 1/2$, given that the surface remains locally Brownian and thus the roughness exponent is fixed at $\chi = 1/2$. In this regime, the STV phase is characterized by an exponentially decaying autocorrelation function, indicative of strong disorder. The supplementary of Ref. [59] has a small discussion on this where they assume a random walk along a path with random and uniformly distributed vortex charges. They show that these phase jumps "disrupt" the KPZ scaling resulting in a $\beta = 1/2$ exponent in the autocorrelation functions.

6. **Reviewer:** 6. In FIG. 2, it was not clear to me what the difference between (i) and (ii) is in terms of parameters.

   Response: FIG. 2(b) and (c), now updated to FIG. 4(b)(c), show the autocorrelation of the *unwound* phase fields (as appropriate for simulations of the SCGLE) for the same set of parameters. We have updated the figures to use color to indicate different values of $\tilde{v}_c$, and we now make explicit that these plots correspond to the dynamical fields $\tilde{\theta}_1$ and $\tilde{\theta}_2$, respectively. Both components are included to demonstrate that vortex activity emerges in *both* fields once transformed into the rotated basis.

   The overall picture is as follows. The KPZ-like surface, predicted by RG flow, linear stability analysis, and corroborating simulations, initially exhibits large downward jumps–discontinuity events characteristic of shock-like behavior in the non-compact representation. At a later stage, due to the inter-component coupling terms in Eq. (5), the second dynamical field $\tilde{\theta}_2$ also becomes unstable, albeit with a delayed onset relative to $\tilde{\theta}_1$. In the SCGLE framework, the presence of dynamical density modes stabilizes the simulations, preventing numerical divergence. However, the emergence of spacetime vortices in both components remain clearly visible.

   Importantly, these large downward phase slips observed in the non-compact case correspond, in the compact theory, to vortex winding events. This correspondence allows a direct identification between phase discontinuities and topological excitations, providing a consistent interpretation across both formulations.

RESPONSE TO REVIEWER #2

1. **Reviewer:** Missing citation Phys. Rev. E 48, 1228

   Response : We agree that this is an important reference, particularly because it presents a renormalisation group (RG) derivation using the same perturbative framework that we have employed and discussed in our work. We have now cited this reference in the manuscript and thank the reviewer for drawing our attention to it. In fact, the paper has helped to clarify several open questions in our own analysis; for example, we had been exploring a generalization of the Cole–Hopf transformation to the case of non-commuting matrices.

2. **Reviewer:** The RG calculations were done in Ref. [Phys. Rev. E 48, 1228] by Ertas and Kardar.

   Response : The renormalisation group analysis of Eq. (5) was first analysed in the Ertas and Kardar paper, however we strongly believe that our work helps to fill in gaps of understanding in the renormalisation group flow and how the theory actually comprises of a four dimensional space. Below, we address our concerns with the Ertas and Kardar's paper and how our paper builds upon the RG specifically.

   (a) The [Phys. Rev. E 48, 1228] does not motivate why X and Y are the correct coordinates to look at. This can be properly motivated in a few different ways (i) by dedimensionalising the equations and showing that this emerges naturally as done in the Supplemental Materials. We have made this more apparent by replacing Eq. (5) for the Ertas and Kardar rotated equations to the equations in adimensional coordinates since these are the correct coordinates to consider the scaling behaviour (ii) we can also consider the normalised interaction vertices $\hat{\Gamma} = [\hat{C}^{-1}]_{\alpha\beta}\Gamma^{\beta}$ and then requiring that the vertices satisfy the trilinear (colinear) condition described in Funaki et al. Ref. [Journal of Functional Analysis Volume 273, Issue 3, 2017].

   (b) Only upon writing down the flow equations explicitly and noticing that we are actually working in a $\mathbb{R}^4$-dimensional parameter space can we start to unpick some of the subtleties of the flows themselves. Importantly, if you want there to be a decoupling transformation via Cole-Hopf you need all three conditions $T = 1$, $X = 1$ exactly. What our paper shows is that along the Cole-Hopf line, irrespective of the starting value of T, the fixed point that you flow to in the upper Quadrant I corresponds to $T = 1$. This insight is important since the RG flows only constrain X, Y and do not constrain the flow of T itself. In a similar vein to Funaki we mention the explicit decoupling transformation and derive the form for the distribution of fluctuations as the sum and differences of GOE distributions (for flat initial condition). This explains why we see extended tails in the $\tilde{\theta}_2$ distribution which has not been adequately addressed, other than by simply saying it should be somewhat KPZ. We additionally numerically verify this as in FIG. [3](c-d).

   (c) We also identify that away from this line the distributions of fluctuations does not seem to be described by linear sums of TW distributions but exhibits a smooth crossover between different phases. This is important especially for polariton systems in Lieb lattices and flat band lattices which should map to a point in parameter space not at the decoupled point $(1, 1)$. Experimentally, although the distribution of fluctuations is not yet observable, it lays important ground work as to why they would not see this distribution but more likely some other set of non-Gaussian and likely non-universal distributions.

   (d) In Spohn's recent paper (2024), there is some discussion as to why the $\tilde{\theta}_1$ and $\tilde{\theta}_2$ fields have different heights for the components of the spacetime two-point correlation matrix $\hat{C}_{11}(x,t)$ and $\hat{C}_{22}(x,t)$ even though the initialise the system with normalised parameters with $\hat{C}_{11} = \hat{C}_{22} = 1$. This is briefly addressed where we suggest that the renormalisation of the two-point correlators to flow to a state with different heights will be true for all points away from the FDR line. This is briefly addressed in the PRE however we further this discussion.

   (e) We explain using a simple linear stability argument for the non-linear dominated theory (i.e. strongly coupled theory) why Quadrants I and IV are unstable. This again is supporting evidence that these two Quadrants which posed issue in simulations in the PRL and PRE are unstable.

However, the discussion and relevance of our paper lies in connecting these ideas to spinor condensates i.e the exciton-polariton and cold atoms communities, where polaritons are inherently multicomponent, and presenting a comprehensive account of the physics, which arises in these systems. We strongly believe that the strength of our work lies in interpreting the parametric phase space for the coupled SCGLEs with the effective theory of $\mathbb{Z}_2$-coupled KPZ equations which are equivalent to the Ertas-Kardar equations. This interpretation reveals three main phases (i) a KPZ/KPZ phase in Quadrant I with specific Cole-Hopf/FDR submanifolds with lots of nice emergent structure (ii) a phase which presents unstable gapless fluctuations leading to large density fluctuations or fragmentation (which lie in Quadrants II and III), and (iii) a phase which supports an instability in the phase modes as revealed by RG, numerical simulations and linear stability for a non-linear dominated surface but then this is reinterpreted at the level of the SCGLE where the phase is compact.

3. **Reviewer** : Moreover, the paper is badly written, instead of reporting all changes of variables that are at some point very confusing and off the point, it should instead explain what physics leads to eq 1

   Response : We have changed the key main text and added significantly more experimental connection and motivation for the equations. Importantly, we no longer focus on the intermediate set of equations presented by Ertaş and Kardar but directly present the adimensionalised equations which removes a lot of the confusing mappings.

4. **Reviewer** : Also, figures are too compact and hard to read: the inset of FIG. 2 iii doesn't have labels for the axis and numbers so that it is impossible to know the scale of the plot

   Response : Updated this figure (now FIG. 4) to additionally include labels for the inset and colour coding to allow for easy distinguishing of the corresponding autocorrelation in (b) and (c)

5. **Reviewer** : FIG. 3 iii, the lines indicating TW-GOE seems noisy for no reason, the TW-GOE distribution has an analytical prediction that can be evaluated to machine precision

   Response : We have updated this figure with updated GOE distribution. The Fredholm determinant of the Airy kernel can be easily calculated using a Gauss-Legendre procedure which is detailed in Folkmar Borneman's paper "On the numerical evaluation of Fredholm determinants" and the Hastings-Mcleod solution to the Painleve equation can be found using Sheehan Olvers Github repository (sadly shooting methods and treating the integral as an IVP numerically fail and so you are forced to consider the solution as a Riemann-Hilbert boundary problem). We opted to compute the distributions numerically.

RESPONSE TO REVIEWER #3

1. **Reviewer:** 1. My first concern is Eq. (1): in the derivation given in Section A of the SM, I believe, the sign in front of μ_c in Eq. (S.1) should be minus and below (S.4) $\mu = \mu_c + i\mu_d$. Generally, I am trying to say that the signs in front of the chemical potential and in front of the interaction terms should be opposite. In the main text, the Authors refer to Ref. [51] where the equation that they study was first introduced. Looking at the same equation in [51], I note that there the equation (1) is divided by imaginary unit compared to Eq. (1) of the current manuscript; so in [51] there would be a minus in front of the term corresponding to μ_c if one restores the i. Could the Authors double check in their derivations, is it just a misprint and in their calculation they have the correct sign, or if it is a mistake, how does the correction of it influence the results? Does e.g. the expression for mean field given above Eq. (3) [or similarly Eqs. like (S.15)] change and what are the implications?

   Response : We have checked the derivation and are happy that everything is self-consistent, though do note that this differs with some of the previous literature. We reiterate that μ_d is important in defining the mean field solution $|\psi_i|^2 = \mu_d/(u_d + v_d)$ and if $\mu_d \to -\mu_d$ then the condition for symmetry breaking then becomes $\mu_d < 0$ as opposed to $\mu_d > 0$ which is used in the paper. The origin for the discrepancy can be found by looking at Eq. (73) in the Sieberer review article for the single component case:

$$S = \int_{t,x} (\psi_q^*(i\partial_t + k_c\partial_x^2 - \mu_c + i\mu_d)\psi_c + c.c. - (u_c - iu_d)\left[\psi_q^*\psi_c^*\psi_c^2 + \psi_q^*\psi_c^*\psi_q^2\right] + \ldots \tag{S.1}$$

   where $\mu_d = (\gamma_l - \gamma_p)/2$ which is the opposite convention to the one we are using where $\mu_d = (\gamma_p - \gamma_l)/2$. This is exactly Eq. (S.4) but in single component form. Taking a functional derivative wrt ψ_q^* and setting the saddle point to zero we should recover the SCGLE which then reads

$$(i\partial_t + k_c\partial_x^2 - (\mu_c - i\mu_d) - (u_c - iu_d)|\psi_c|^2)\psi + \ldots \tag{S.2}$$

   and rearranging gives the form of the SCGLE used in the paper. The minus sign arises from the different convention in defining parameters.

2. **Reviewer** : 2. Why are such parameters as chemical potential, gain saturation, etc are equal for the two different components?

   We think this is best revealed by discussing the full generalised Gross-Pitaevskii Equations with independent polariton reservoirs for the two-components corresponding to (n_1, n_2) for the two optically active exciton components (and subsequently the polaritons inherit the spinor structure). Note that typically $1, 2$ indices are replaced by $+, -$ indices to indicate that these correspond to spin projections of $+1$ and -1 respectively. For cw non-resonant pumping with no polarisation (as is used in Ref.[Phys. Rev. B 106, 245309]), both reservoirs are pumped equally with a rate equation of the form:

$$\partial_t n_{r,1} = P - (\gamma_R + R_1(|\psi_1|^2 + R_2|\psi_2|^2)n_{r,1}$$
$$\partial_t n_{r,2} = P - (\gamma_R + R_1|\psi_2|^2 + R_2|\psi_1|^2)n_{r,2}.$$

   with $R_1 = R_2$ in their particular use case. This reflects that the coupling between the reservoirs and the pumping should be the same. In the derivation from the Lindbladian, we are essentially assuming that the aggregate set of environment modes act as a Markovian bath with equal effect on each polariton component. The $\mathbb{Z}_2$ symmetry arises naturally if you assume that the behavior of both reservoirs is the same (equal decay rates) and that the system is symmetric $1 \leftrightarrow 2$. There is no reason to believe that the reservoir decay rates, or the scattering rates between the reservoirs and the condensate would be spin dependent in the case of linearly polarized condensate with equal occupations of both components. The only spin dependent parameters are intra and inter component interactions, which are considered in this work.

3. **Reviewer** : When discussing the miscible-immiscible transition, the Authors vary the rescaled intercomponent interaction v_c/u_c, as I can judge from FIG. 1(i), within approximately from -1.75 to 1.75. At the same time, they write explicitly that for the considered exciton-polariton systems $v_c = 0.1u_c$ so their rescaled interaction is quite defined and lies in the vicinity of 0.1. In this sense, its not totally clear and its not discussed in the paper how really relevant is their phase diagram to polaritons. It would be useful to comment, to which other systems it can refer that could potentially exhibit all the quadrants I IV, if they exist? Speaking of polaritons, could the Authors place a marker within the diagram FIG. 1(i) which would indicate where do the polariton

parameters belong (compared to the immiscible boundaries given in Eq. (2))? What would be the expected values for the rescaled gain saturation v_d/u_d for polaritons?

Response : In response, we have included in an additional section discussing in more detail the experimental relevance of the findings. While the core theoretical framework is intended to remain broadly applicable across any model which maps to multicomponent KPZ, we now explicitly comment on how these results relate to polariton experiment. Specifically, for polaritons without momentum dependent losses $\gamma(k) = \gamma_0 + \gamma_2 k^2$ has $\gamma_2 = 0$, then the bare parameters lie on the Cole-Hopf line exhibiting an emergent decoupling in the RG. In this regard, tuning $\tilde{v}_c$ only takes you away from $X = 1$ and $Y = 1$, but along the Cole-Hopf submanifold (shown as a line in FIG. 1(c)) and so you correctly identify that the entire diagram is not that relevant to standard polaritons but instead reaffirms that generalising to multicomponent systems does not pose any huge pathologies. In the RG limit, both modes should exhibit KPZ scaling and universality subclasses. On the other hand, even in the recent 1D Experiments in the Bloch group using micropolars, the linewidth gives effective momentum dissipative to coherent momentum dependence parameters with $k_d/k_c \sim 1$. This means that we are able to map away from the Cole-Hopf point as we tune $\tilde{v}_c$. So that even for small effective $\tilde{v}_c$, we should be able to capture some of the non-universal features of being away from the Cole-Hopf line such as the distribution of fluctuations not being directly described by TW distributions. This is even more apparent with studies on KPZ in flat band lattice models where feasibly you should be able to access the spatiotemporal vortex phase in Quadrant IV.

We opt to not include a marker for polaritons because the boundaries themselves are strongly dependent on the system parameters. However we add significant discussion around how we can move around the parametric space. To our knowledge, is unclear exactly what the ratio of v_d/u_d would be in experiment however we can gain significant insight by starting with the gGPE and showing which parameters contribute to v_d. This is done in the final section of the supplemental materials. This paper lays the groundwork to theoretically consider the effect of a non-linear dissipative cross-coupling.

Answering the question more broadly, this work is relevant to any dissipative spinor condensate, such as for example photon BEC, where we know from private communication that the experimental search for KPZ phase is on-going in several groups, as well as of cold atoms, where by increasing three-body losses one could in principle access the KPZ regime.

4. **Reviewer** : 4. Is θ with the subscript i/j and θ with superscript alpha/beta the same thing? Or i/j and alpha/beta have a different meaning? Both notations appear in the main text and in the SM and neither is explained.

   Response : The convention used at the moment (and we have added a sentence explaining this) is that when we refer to complex fields we use Latin indices and then when we refer to real fields we use Greek indices. This is purely convention.

5. **Reviewer** : 4 (i) Also, is it a must that θ_i are the same for the classical and quantum parts of psi (as given in Eq. (S.12))?

   Response : We added further discussion on this in the supplemental materials and show that this is the correct parameterisation fo the gapless Nambu-Goldstone modes. In the following we will discuss the single component case as it generalises to multicomponent easily. For Schwinger-Keldysh field theories, there is a doubling of the number of fields in the theory. If we assume that there are no Lindblad operators (and thus no coupling between fields on the forward and backward contours), then we have two independent phase rotation symmetries i.e. the full symmetry group is $U_f(1) \times U_b(1)$ with $\psi_\pm \mapsto e^{i\theta_\pm}\psi_\pm$. Note that we also assume that the initial state $\rho(-\infty)$ does not explicitly break the diagonal symmetry structure. This means that in equilibrium theories we should have two gapless modes associated with both the $U_f(1)$ and $U_b(1)$ symmetries which is simply just the expected two gapless modes we see in the Bogoliubov spectra in ϕ_4 theories. Upon introducing couplings between fields on the forward and backward contour e.g. $\bar{\psi}_+\psi_-$ then we clearly break the $U_f(1) \times U_b(1)$ symmetry to the diagonal subgroup $U(1)$ associated with joint phase rotations of the forward and backward contour. Since a Keldysh rotation is simply a unitary rotation (linear sum and difference of the forward and backward fields), the topological structure of the ground state should remain in the classical-quantum coordinates. This means that the diagonal subgroup symmetry in classical-quantum Keldysh coordinates corresponds to both $\psi_c \mapsto e^{i\theta}\psi_c$ and $\psi_q \mapsto e^{i\theta}\psi_q$. When we expand around the saddle point in gapped fluctuations $\psi_c = \sqrt{\rho + \chi}e^{i\theta(x,t)}$ and $\psi_q = (\xi_R + i\xi_I)e^{i\theta}$, we allow for ψ_c and ψ_q to differ in their phase since this is captured by explicitly writing the fluctuations in terms of real and imaginary contributions $\xi_{R/I}$. However, we know that any fluctuation in $\xi_{I/R}$

must be gapped and thus can be integrated out. This means that the phase difference between the classical and quantum fields is a gapped variable and can be integrated out.

6. **Reviewer** : How does Eq. (S.6) relate to the quantum part of psi in (S.12)? Or (S.6) only described the classical part? Are θ_i in (S.12) the same as θ_i in (S.6)? This is a bit confusing and should be clarified.

   Eq. (S.6) only parametrises the classical field since we have effectively Hubbard-Stratonoviched the path integral and *replaced* the quantum field with Gaussian correlated white noise, appropriate in a semiclassical approximation to map to stochastic equations for the classical fields $\psi_i(x,t)$. You could potentially interpret this as a transformation on the complex white noise however I do not think this is particularly enlightening.

   θ_i in both cases have the same parameterisation and the only difference is that in Eq. (S.6) we are working directly with the SCGLE whereas in Eq. (S.12) we are working within a path integral formalism but both are equivalent up to a Hubbard-Stratonovich. The benefits of working with the path integral formalism is that we can motivate the SPDE from a Linbladian in suitable limits and it is easier to run Wilsonian RG. Equivalently, when looking at the stability conditions for the distribution of fluctuations we could have looked at the pole conditions from the retarded Green's functions $\det G^R(\omega,k)$ but we decided not to do this.

7. **Reviewer** : 5(i). Starting from Eq. (3) and till the end of the main text, there is a continuous flow of new notations and parameters which then acquire tildas and the asterisks, and their various combinations that are all given as in-line math without any discussion of their meaning. I understand that some re-notation is required to transform the SCGLE from its initial shape to KPZ-like equations, coupled and decoupled, but the way its currently presented is very entangled.

   Response : 5(i) We have added a lot of material which originally appeared in the supplemental text into the main text. We hope that this makes the text clearer. We have replaced Eq. (5) with the adimensionalised version in the main text since this allows us to introduce the exactly what is flowing in the renormalisation group equations and hopefully avoids some of the confusing coordinate transformations.

8. **Reviewer** : 5(ii). What is the covariance matrix Delta (without the tilde) appearing in the noise description below Eq. (3) and within the first column of page 3? I did not find it even in the SM which only lists Delta with the tilde. This other Delta with the tilde isnt defined in the main text, either (only in the SM). The discussion of FIG. 1(iii) begins before the parameters X and Y plotted there are even introduced.

   Response : 5(ii) We have shown how we can find the noise covariance in the $\mathbb{Z}_2$ coordinates by means of a rotation however we do not write them out explicitly since we find their form lengthy and not that enlightening.

9. **Reviewer** : 5(iii). I would like to ask the Authors to thoroughly revise the text from Eq. (3) and below, to avoid all confusion and give a proper introduction to the interaction tensor, diffusion matrix, etc. and all their consequent modifications. I believe it would help if some of the information presented currently in the SM is moved to the main text. In particular, currently the appearance of the four-parameter space T,X,Y,Z is not really understandable from the phrase that the adimensionalising of coordinates in Eq. (5) leads to it, while in the SM it is naturally derived (in the main text there is not even a reference for the reader to check the SM on this matter).

   Response : 5(iii) As addressed in 5(i) we have moved significant sections of the SM into the main text.

10. **Reviewer** : 6. Along the same line, I do believe that the Authors should consider moving the discussion of Flow Equations that is presented in pages 14-15 of the SM to the main text. Only after reading this far into the Supplemental file I started to understand what is going on in the main text.

    Response : We have opted to move only two of the equations for X and Y into the main text since this captures the fixed point structure and does not clutter the main text too much. We do not see that much value in adding in the flows of T and Z for the ratio of the diffusion constants and the effective KPZ constant for $\tilde{\theta}_1$. The full flows are contained in the supplemental materials.

11. **Reviewer** : 7. Not a single panel of Figure 2 is discussed in the main text (neither it is referred to in the SM). It it isnt worth discussing, why is it added to the text? What should the reader learn from it? Please add a discussion. Looking at its caption, I would like to ask: do the parameters that are taken to plot it bear a connection to polariton system? Could they anyhow be related to any polariton studies or could this STV phase be observed experimentally? I did see the remark in conclusions that it can be accessed by tuning intercomponent interactions, but it is really possible, e.g. in the context of exciton-polariton systems?

Response : In response to this and several reviewer comments, we have expanded the discussion of the spatiotemporal vortex phase in Quadrant IV. The clearest route to understanding this phase begins from the non-compact equations which support an instability in the linear analysis around the non-linear dominated equations (see supplemental). The instability renders Quadrants II and IV unstable but additionally we see other signs of pathologies in the RG, such as $T \to 0$ signaling a breakdown in scaling and the failure of a description in terms of two KPZ-scaling modes.

Instead, the distribution of fluctuations—particularly for $\tilde{\theta}_1$ shown in the new FIG. [3]—reveals an instability in the random interface due to large *shocks* in the surface downwards. FIG. [3] deals with the distribution of fluctuations for both components. This also gives rise to an additional scaling exponent briefly before the simulations numerically breakdown.

The question then becomes: how can we interpret this at the level of a compact phase? This is detailed in FIG. [4] where we show direct SCGLE simulations as we cross from the decoupled points to the STV dominated phase in Quadrant IV showcasing a proliferation of spacetime vortices. This figure has been updated and the aspect ratio has changed to allow for better reading of the EW, STV and KPZ phases. Additionally we have added colours to the inset in FIG.3(a) to better capture the connection between (a) (where we tune v_c and (b)-(c). The parameters are not specific to a polariton system, however they are not completely unreasonable $k_d/k_c = 1/3$ (this is achievable in lattice band models as discussed already), $u_d/u_c = 2/3$ is probably one or two orders of magnitude too large since most experimental values lie closer to 10^{-2} to 10^{-3}. Tuning $\tilde{v}_c$ from 0 to -1.20 is also unlikely to be seen in polariton systems directly. However, it is possible to think of values which are more reasonable and which do lie in Quadrant IV and so what this captures is the theoretical difference between Quadrants I and IV in the appearance of STVs. Feasibly, we are not able to directly tune the scattering of polaritons but if one is able to tune R_2 in the rate equations of the gGPE then one could have greater control over the effective $v_{c/d}$. We think the key reading point for exciton-polaritons is: if one is in flat band models (experiments on polariton micropilar lattices), even at small intercomponent couplings one should move away from the decoupled point giving rise to non-TW GOE statistics in the fluctuations and discrepancies in the 2-point correlation function.

12. **Reviewer** : 7. Not a single panel of Figure 2 is discussed in the main text (neither it is referred to in the SM). It it isnt worth discussing, why is it added to the text? What should the reader learn from it? Please add a discussion. Looking at its caption, I would like to ask: do the parameters that are taken to plot it bear a connection to polariton system? Could they anyhow be related to any polariton studies or could this STV phase be observed experimentally? I did see the remark in conclusions that it can be accessed by tuning intercomponent interactions, but it is really possible, e.g. in the context of exciton-polariton systems?

    Response : We originally mentioned panel (i) where we discussed that this shows that a noise coupling between independent KPZ equations is irrelevant. We have added an additional sentence explaining this further. The idea here is that we want to build up the intuition and structure of the RG flows in this $\mathbb{R}^4$-dimensional by looking at simple cases of the projected flow. First, introducing simply correlated noise (some non-diagonal covariance structure) between the $\mathbb{Z}_2$ equations for θ_i, we show that the noise covariance flows to zero i.e. the fixed point is simply two independent KPZ equations and noise correlations do not affect the scaling. Secondly, we need to show that in general $T \to 1$ in the RG as this is a necessary condition for the existence of a Cole-Hopf transformation. We show that this is the case in Fig 4(b).

13. **Reviewer** : - the abbreviations MSRJD, SCGLE, EW, GOE are not introduced anywhere.

    Response : All initialisms have now been introduced properly.

14. **Reviewer** :- on the contrary, the abbreviation NG is introduced but not used in the main text.

    Response : This has been replaced by Nambu-Goldstone and NG not introduced in the main text.

15. **Reviewer** : - since the SM contains a lot of information regarding every part of the main text, I strongly recommend that the Authors add references to SM in every place where it is relevant; such as giving a hint that Eq. (1) is derived in SM, the excitation modes are obtained in SM, and so on, where applicable. Currently the only reference to SM [61] in the main text appears just twice with respect to covariance matrix and the STV phase analysis.

    Response : This has been addressed and the reader is referred to SM [61] on each relevant occasion.

16. **Reviewer** :- the parameterμ_c appearing in Eq. (1) is only introduced as chemical potential at the very end of the 3rd page of the SM; in the main text or within the derivation of Eq. (1) presented at the beginning of SM, it remains undefined.

    Response : We have introduced the chemical potential and all parameters after Eq. (1).

17. **Reviewer** :- please accompany Eq. (1) with a comment that the index i runs over the values 1,2 (or up/down, for spins) and that $\bar{i}$ denotes the other component.

    Response : This has been addressed by adding "where i runs over the spinor components and $\bar{i}$ denotes the other component."

18. **Reviewer** : - in the top of second column of page 2, it is written that for repulsive interactions $v_c > 0$, these parameters map to Quadrants II and III in FIG. 1. I believe it is a misprint since the values $v_c > 0$ correspond to Quadrants I and II.

    Response : This needs some more clarification - we have moved this paragraph earlier since this is general with "All bare parameters for polaritons which violate this condition conveniently map into Quadrants II and III in FIG.1(a-c)". Both Quadrants II and III are unstable and can be mapped to irrespective of the sign of v_c since the mapping is a function of lots of tuneable parameters. However the two behaviours (fragmentation or not) is dependent on whether we have attractive or repulsive interactions. We then pick two simple examples to show this to be the case, one which maps to Quadrant II and has $\mathbb{Z}_2$ symmetry breaking for the system state (fragmentation) and one without fragmentation which in this case maps to Quadrant III. This is not general and could easily swap these around.

19. **Reviewer** : in panel (iii) of FIG. 1 there are no scales on the axes. From reading the text I concluded that the central dividing lines correspond to X=0 and Y=0, while the red line is at X=1. However, since the X=Y line isnt on the diagonal of the square, I am guessing that the scales in X and Y are different. Please put the scales on the graph.

    Response : We have added scales to this graph and updated FIG. 1.(c) (which is now FIG. 2.)

20. **Reviewer** : in Eq. (S1) of the SM, there is an extra closing round bracket in the first term, while the closing square bracket at the end of the line is missing; additionally, since the Authors are considering a strictly 1D case, I would avoid using the vectorial nabla operator.

    Response : We have removed all vectorial nablas in the supplemental materials for clarity, however we do note that the derivation presented in (A) from the quantum master equation is dimension agnostic and thus generalises to higher dimensions.

21. **Reviewer** : in FIG. S2, the labels on the panels (i), (ii) are missing, while they are mentioned in the caption.

    Response : The old figure has been updated and now includes the labels.

22. **Reviewer** : in the panels and caption of FIG. S3, as well as in Table 1, what does CH stand for?

    Response : "CH" stands for Cole-Hopf. I have replaced this simply to Cole-Hopf. The Cole-Hopf manifold allows for a mapping from the KPZ equation to a linear coupled stochastic heat equation for which we can then understand the solutions of the KPZ equation.

RESPONSE TO REVIEWER #3

1. **Reviewer:** the EW abbreviation is still not introduced
2. **Authors:** We have finally updated the main manuscript with the initialism.
3. **Reviewer:** while the detailed discussion in response to Reviewer #2 is quite structured and enlightening, so that I would suggest to use some of it to enrich the conclusions of the paper.
4. **Authors:** We have added in the conclusion some further discussion around how the results clarify some of the subtleties of the flows in the conclusion.