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Reply to "Comment on 'Renormalization-group theory for the phase-field crystal equation' "

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In a recent Comment [Y. Shiwa, Phys. Rev. E **79**, 013601 (2009)], Shiwa points out that our multiple-scales analysis of the Van der Pol equation [Appendix of Phys. Rev. E **74**, 011601 (2006)] contained an error, and thus there may be no ordering ambiguity for renormalization and differentiation operations, as we had proposed for the phase-field crystal equation and other conservation laws. The Van der Pol analysis was indeed incorrect, and while his Comment casts doubt over our treatment of the multiple-scales analysis of the phase-field crystal (PFC) equation, there is no substantial change to our renormalization (RG) group results or simulations of the PFC model. This example highlights the merits of the RG approach to singular perturbation problems: the RG method is more mechanical and requires less user-insight than traditional methods.

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In an earlier paper [1], we attempted to derive the amplitude equation to the phase-field crystal (PFC) model—a conservation law [2]—using several variants of the renormalization group (RG) method and compared the results obtained thereby with the result derived from a benchmark multiple-scales (MSs) analysis of the same model. We noted a discrepancy between the RG results and the MSs results, viz. the MSs analysis yielded certain extra higher-order terms in the amplitude equation which were not captured by either of the RG methods. We have since verified that these terms have virtually no bearing over our simulations of the PFC equation. In our paper, we attempted to resolve this apparent mismatch by proposing a different ordering of the renormalization and differentiation operations (see [1] for details) in the RG method that led to agreement with the MSs results. We attributed this ordering ambiguity to the presence of the conservation law.

In his Comment Shiwa has, however, argued that there is no need for this reordering, claiming an error in our MSs analysis. The Comment has two parts, and these need to be considered separately.

(1) The Comment points out an error in the Appendix of our original paper in the derivation of the amplitude equation for the Van der Pol oscillator using MSs. The author states that we ignored an extra time scale inherent to this problem, as a result of which our derivation using MSs did not match the derivation using the proto-RG method. Based on this he concludes that there is no need for the modification we suggested for the RG method (renormalization before differentiation) as applied to the Van der Pol oscillator in the Appendix of our paper. We agree completely with this point. We made an error in the MSs calculation, and if we had not done so, the original RG calculation would have easily given the correct result.

(2) The Comment goes on to argue that the MSs scale result for the (one-dimensional) PFC equation that we derived in our paper must also be incorrect—and that had the MSs analysis been correctly done, our result would have agreed with the standard RG calculation derived in the paper. Thus there would not be a need to make any modification of the standard RG calculation. Actually, as much as we would like this to be the case (the senior author on our paper was one of the original developers of the RG method), the Comment does nothing to prove this, although it does point out one area of doubt in our MSs calculation: the neglect of a zero mode, arising from the conservation law to which we had already drawn attention. We agree that this neglect casts doubt on the MSs calculation that we used as a standard of comparison for our RG calculations, and presume that if the RG calculation were compared with the correct MSs calculation, there would be agreement without any need for modifying the order of renormalization and differentiation. However, Shiwa does not show this in his Comment.

While we welcome Shiwa's identification of our error in the multiple-scales analysis, we would like to clarify a couple of points.

(I) We point out that there is a disconnect between the calculation done in the Appendix (the Van der Pol oscillator) and the calculation on the PFC equation: the Van der Pol oscillator does not exhibit the conservation law which we already recognized in our paper was causing a problem in the derivation of amplitude or RG equations. The calculation error made in the Appendix using the MSs method had nothing to do with the main issue addressed in our paper, namely, the coarse graining of the PFC equation. In other words, we made two separate errors in using the MSs method.

(II) More importantly, the Comment does not report a full RG calculation and comparison with the correct MSs calculation on the PFC equation; i.e., this is still an open question requiring a more complete treatment. The author himself says as much, in the closing of his Comment. We look forward to a complete resolution of this question.

We conclude by noting that this episode highlights the superiority of RG methods over existing singular perturbation theory techniques, such as the MSs method, which require delicacy in order to be correctly operated.

[1] B. P. Athreya, N. Goldenfeld, and J. A. Dantzig, Phys. Rev. E **74**, 011601 (2006).

[2] The PFC equation describes the evolution of the time-averaged local atomic density field in the material. An interesting property of the PFC equation is that under appropriate boundary conditions, e.g., periodic boundaries, the density field when spatially averaged over the physical domain remains constant in time, since for equal influx and outflow of atoms, the mass of the system must be conserved.