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Review of the doctoral dissertation by Aleksander Głódkowski, MSc, entitled "Effective theories for many-body systems with nonuniform symmetries"

"Everything that is not forbidden is compulsory" is a famous phrase adopted by many physicists (such as Murray Gell-Mann), highlighting the power and importance of symmetry-based reasoning in physics. In his PhD thesis, Aleksander Głódkowski takes this principle to heart by relying solely on symmetry assumptions to derive the low-energy effective theory of a generic system and its corresponding finite-temperature hydrodynamic description. In traditional textbooks, one typically emphasises the distinction between so-called internal symmetries that act on fields, e.g., order parameters, and symmetry operations acting on coordinates, such as space and time. Głódkowski takes a different approach and focuses on the distinction between uniform symmetries (such as a spatial translation) and non-uniform symmetries (such as a spatial rotation). With many elucidating examples, he shows that the former determine the number of independent gapless modes in the system, while the latter impose (kinematic) constraints on the effective theory. This distinction is very compelling and important, and I believe this perspective has the potential to influence future textbooks when the counting of Goldstone modes is discussed in the context of spontaneous symmetry breaking.

Before assessing this thesis, I would like to note that effective field theory, which is one of the principal methodologies employed in this work, is not my primary area of expertise. Accordingly, my assessment emphasises the conceptual soundness, physical insights, and broader significance of the work rather than the detailed technical aspects of the calculations. The thesis consists of six chapters, based on five publications by the PhD candidate, with one additional publication from his PhD that was not included in the thesis. It is noteworthy that this publication is a single-author work in a reputable journal (Journal of High Energy Physics). Moreover, Głódkowski stands out for having three of his six articles published without his PhD supervisor, which clearly shows his scientific maturity and ability to form scientific collaborations. Furthermore, the thesis is well-written, with a strong logical narrative in which chapters build upon each other in increasing difficulty. In a sense, it is very didactic; however, one still needs to have some knowledge of effective field theory and its techniques (such as group theory) to fully appreciate the results. I will now proceed with the descriptions of the chapters and, where necessary, dress them with my comments.

Chapter 1 sets the stage for the thesis and briefly discusses the concepts behind effective field theory and the scope of the thesis regarding uniform versus nonuniform symmetries. The latter are defined when the corresponding charges have a non-vanishing Poisson bracket with the generator of spacetime translations. The fact that such uniform symmetries do not lead to additional degrees of freedom in the effective theory serves as the guiding thread throughout the thesis. Generally, this chapter is well written, although it would have been helpful if some notions —though well known within the effective-field theory community —were made explicit for people outside this field. Specifically, the god-given unit system ($\hbar = c = 1$) should have already been introduced here upon its first usage, and the Hodge-star operator could have been defined. Furthermore, I noticed some aesthetic inconsistency in notation, in that field components are in this chapter enumerated by lower case italic letters a, b.... In contrast, later in the thesis, this notation is changed to I, J, ... However, these are minor comments. The thesis is then split into two parts, where Part I (Chapters 2 and 3) focuses on the low-energy structure of the theory at zero temperature. In contrast, part II focuses on transport (hydrodynamics) at finite temperatue (Chapters 4-6).

Chapter 2 addresses the counting of gapless modes in systems with spontaneously broken non-uniform symmetries. The main point is that, when the full set of broken generators is taken into account, the number of Goldstone modes is generally smaller whenever non-uniform symmetries are spontaneously broken. The resulting counting formula highlights an aspect of spontaneous symmetry breaking that is often overlooked. I enjoyed reading the clear presentation of this concept and the elucidating example of a crystal with the emergence of phonons. In my opinion, the presentation of these results is a clear original contribution of this thesis.

However, on certain points, the presentation in this chapter could be improved. A brief sentence introducing the Bargmann algebra would help readers from outside the field. Furthermore, the generators in Eq. (2.85) are not defined, and it would have been helpful if their physical interpretation had been explained. In Sec. 2.2 the setting was a 1+1 dimensional field theory, whereas the narrative is switched in Sec. 2.5 to the 2+1 dimensional case (where, in between, it was kept general). In principle, the reader can guess the number of spatial dimensions by noting that there is a single generator for spatial translations L (which was not explained), so stating this explicitly would have been helpful. Of course, one can also infer this from the number of acoustic phonons. Finally, it would have been helpful if the author had explained that the symbol D is sometimes used as the dipole generator, whereas on other occasions it is used as the covariant derivative. However, I am aware that the context makes this clear, and this might be common in the effective-field theory literature.

Chapter 3 examines rotating superfluids as a more complex example of a system in which non-uniform symmetries play an important role. The author places a strong emphasis on the formation of the so-called vortex crystals, which host a single low-energy excitation, called the transverse Tkachenko mode. Because the system volume undergoes rotation (which is treated in a co-rotating frame), the global symmetries of the theory are more complicated, and its generators satisfy the magnetic Bargmann algebra. The symmetry-breaking pattern is then discussed, and the counting formula of the previous chapter is applied and rigorously discussed to infer the existence of a single Tkachenko mode. Subsequently, the low-energy effective theory is constructed and thoroughly discussed. The author has written this chapter with care, and the findings are interesting. However, I have a few minor comments and some larger comments. These comments concern exposition rather than the validity of the results.

First of all, I think the transition from Eq. (3.2) to (3.3) warrants more explanation, as the author provides only a brief introduction to rotating superfluids. The mass m is not defined, and in the above Figure 2, it would have been helpful if the author had given a characteristic size for what the author means by "microscopic". Under Eq. (3.8), the particle number and angular momentum should have been explicitly defined in terms of their symbols N and L, respectively. A physical interpretation of the symmetry operations in Eq. (3.15) would have been insightful, as well as the associated conserved charges in Eq. (3.17). Below Eq. (3.28), the reference to Eq. (4.143) is incorrect, and I believe that it should be (3.25) instead (probably some duplicate LaTeX label). The exterior derivative d could have been defined because it is sometimes used as a dimensionality in the thesis. In Eq. (3.58), two notations for big-O are used: O and $\mathcal{O}$. Is there a specific reason for this? Furthermore, the angular brackets in Eq. (3.67) are not defined until below Eq. (6.17). Since the thesis should read as a cohesive document, it would be preferable to introduce this notation when it first appears.

Throughout this chapter, the author works in two spatial dimensions, e.g. when writing the conserved charges in Eq. (3.17). However, the scheme in Figure 1 suggests that the author has three spatial dimensions in mind. This implies that there is an implicit assumption of translational invariance along $\mathbf{\Omega}$, although it was not explicitly stated. Perhaps the author can comment on the conditions under which this is an appropriate assumption? It seems to me that the imposed boundary conditions play an important role, as does the shape of the container. What kind of interesting physics occurs when the quasi-2D assumption breaks down, and how are the results and conclusions in Chapter 3 in this case affected? Moreover, is it possible that a vortex line is not aligned with $\mathbf{\Omega}$? On certain occasions, vortex lines can form loops. How can these instances be captured within the author's framework? I understand that some of these comments may fall outside the scope of this work; nevertheless, I hope they spark some interesting discussions.

In Chapter 4, the first chapter of Part II, the author shifts to a non-equilibrium setting within the hydrodynamic paradigm. I come from a more "traditional" hydrodynamic background myself, mostly from creeping flow applied to soft and biological matter. It was therefore interesting to read how an effective field theorist views hydrodynamics, and I would like to praise the author for a job well done in explaining some central concepts from his viewpoint. The reasoning in Part II largely follows the identification of broken symmetries and their associated charge generators. With these charge generators, an associated locally conserved density is defined with a corresponding current density for which the constitutive relation is inferred. Throughout, the local thermodynamic equilibrium assumption is invoked, and transport coefficients are constrained by the second law of thermodynamics (non-negative entropy production). Then the hydrodynamic fluctuations are analysed. The author makes a clear distinction between conserved densities associated with uniform versus non-uniform symmetries. For the latter, the explicit coordinate dependence is factored out, yielding uniform currents that satisfy covariantly defined conservation laws. When investigating the hydrodynamic mode spectrum, non-uniform symmetries can lead to sub-diffusive behaviour. Effects of momentum and (internal) energy are then included in the context of Aristotelian fluids.

As before, I start with minor comments and then move to major comments. On page 32, the length scale l_{mf} is not specified nor explained. After Eq. (4.63), the Lie derivative $\mathcal{L}_\xi$ should have been defined. I would have appreciated a physical intuition behind Eq. (4.70). Furthermore, it might be worthwhile to relate the discussion to established terminology for certain principles. For example, what is described below Eq. (4.101) is called the Curie principle, named after the husband of the scientist quoted at the beginning of the thesis.

Moving on to bigger comments, I was a bit surprised to see Fick's law used as an example for uniform symmetry breaking. The author largely operates within the local thermodynamic equilibrium assumption. When you investigate diffusion in linear irreversibible thermodynamics, it is strictly well-defined only if one considers a multi-component system; see, e.g., the book by de Groot and Mazur. In the first few chapters of this book, one constructs the balances of mass, linear momentum, and energy. Then one flux of a chosen component is eliminated in favour of the so-called barycentric velocity. This quantity determines the transport of linear momentum in the system, with, on top of it, diffusive currents defined relative to this reference velocity. Could the author comment on the relation between this framework and the effective-field-theory notion of diffusive transport?

Another aspect I noticed is that the author claims that, I quote "...our presentation differs from conventional treatments by systematically incorporating the full set of global symmetries,...". However, in this treatment, microscopic time reversibility is not included and is only imposed (see footnote 27). Could the author comment on why microscopic time reversal is imposed only at a later stage rather than incorporated from the outset? Typically, if one starts with a Rayleigh dissipation function that is bilinear in the generalised velocities, one obtains constitutive relations that automatically satisfy Onsager reciprocity. Is there an effective-field theory analogue of this? Finally, the hydrodynamic fluctuations are discussed by expanding around the equilibrium. However, in some systems, this might not be appropriate, for example, if there are non-trivial steady states. Could the author comment on this?

Chapter 5 applies the concepts introduced in Chapter 4 to fractons, which are quasi-particle excitations with restricted mobility. Considering recent experimental results, such as Ref. 14 and Scherg *et al.*, Nat. Commun. **12**, 4490 (2021), I view the study of fractons as a timely research subject. Here, non-ergodic behaviour of excitations can be observed, which is attributed to kinetic constraints. In the context of Głódkowski's work, these phenomena can be understood as arising from the spontaneous breaking of a non-uniform dipole symmetry. In the above-mentioned Nature Communications experiment, this symmetry is effectively realized through a non-vanishing tilt induced by an external magnetic field. Chapter 5 thoroughly discusses fracton physics from an effective-field theory perspective. I find the example of a system in which total charge and total dipole moment are conserved especially instructive. However, it should be noted that they should be treated as abstract charges and dipoles, since electrical charges in Coulomb systems are constrained by global charge neutrality. In this case, I would say that microscopic details sometimes matter.

Fracton physics is then subsequently discussed as a culmination of the methods introduced in previous chapters. In particular, the low-energy effective theory at zero temperature is constructed, as well as finite-temperature hydrodynamics. Furthermore, some interesting dualities of the theory are discussed, and a connection to a possible experimental setup is presented: a tilted Bose-Hubbard chain. I was wondering in this case how fermions and bosons would behave differently in the presence of non-uniform symmetries. Perhaps the author could comment on this? To what extent do the conclusions depend on bosonic statistics? As in previous cases, I noticed in this chapter that square and angular brackets are not defined; their definitions appear only in Chapter 6.

Chapter 6 is the most extensive part of the thesis and discusses fracton physics further in the context of homogeneous and isotropic systems. It is an extension of the previous chapter, since it also includes energy and momentum degrees of freedom. The author chooses again for a construction of increasing difficulty, where first only the dipole symmetry is spontaneously broken, giving rise to dipole-conserving fluids, and later also the monopole symmetries for realising fracton superfluids. For both types of fluids, the conserved charges are first identified. Second, the thermodynamic relations are established, followed by a construction of the hydrodynamic theory. In the case of fracton superfluids, even third-order hydrodynamics is discussed.

I have a few minor comments on Chapter 6. There is some unfortunate notation where n denotes the conserved density associated with the monopole charge, but at the same time it is also used as a measure for the truncation order of the hydrodynamic theory. More than in other chapters, some calculations are simply stated as results, without any comments or brief interpretation (e.g., after Eqs. (6.58) and (6.84)). Including a small discussion would have made the reader appreciate the findings more. The term "Josephson relation" is used, but could have been explained in one of the introductory chapters for non-experts. Table 2 is referred to before Table 1 on page 82, but the table appears only on page 87. Therefore, I think that the placement of tables could have been improved for readability. The term "ideal" may be interpreted differently by readers from other areas of physics, where it is often associated with non-interacting systems rather than non-dissipative transport. Using the term "mechanical" here might therefore appeal to a broader audience. Finally, a section on the status of experimental realizability of fracton (super)fluids would have been meaningful. Perhaps the author can comment on the current state of the experimental side? If there are no experiments, what are the reasons that they have not yet been realised, and what challenges are still left? This comment is especially relevant because on page 74 twelve different transport coefficients were found. I think it would be appropriate for the author to comment on how such a large number of transport coefficients could, in principle, be measured independently in experiments.

The thesis then follows with clearly written conclusions, some appendices, and references. The latter could have been formatted better for article titles: some letters should have been capitalised (e.g., bose-einstein versus Bose-Einstein). Furthermore, given the emphasis on hydrodynamics and constitutive relations, it might also have been worthwhile to mention the work of e.g. Pleiner and Brand. Their continuum-theoretical approach shares several conceptual themes with the present treatment, even though it is formulated from a different perspective. Finally, across this thesis, especially in the context of hydrodynamics, I think the role of boundary conditions is not sufficiently highlighted. As far as I can tell, it was only briefly discussed in the context of the rotating bucket experiment, when superfluidity is introduced. I believe it would be interesting to discuss possible boundary conditions that can emerge from symmetry considerations, which could also lead to practical calculations for experimental setups. Furthermore, non-trivial boundaries can also lead to non-trivial topological behaviour, which is well known to be important in fracton physics. Perhaps the author can give his perspective on this matter.

To finalise this review, I would like to note that my comments and questions are not meant to undermine the high quality of the work presented here. I hope that I can offer a new perspective on this work from someone outside the field, and that it can serve as a proxy for identifying where things can be improved. Generally, I rate the quality of the PhD thesis as high, and the manuscript was a pleasure to read. One of the strengths of the thesis is that apparently diverse systems are shown to follow from a common symmetry-based framework, rather than being treated as unrelated examples. Overall, the thesis presents a coherent and original contribution to the theory of spontaneous symmetry breaking and hydrodynamics. The scientific value is high, the exposition is generally clear, and the results significantly advance the understanding of non-uniform symmetries. Furthermore, the author has demonstrated that he can perform independent scientific work and exhibits all the qualities of a mature scientist. In view of the above, I request that Aleksander Głódkowski, MSc, be admitted to the public defence of his PhD thesis.

Kind regards,

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