The article « On the intrinsic pinning and shape of charge-density waves in 1D Peierls systems » deals with extensions of Brazovskii-Dzyaloshinskii-Krichever (BDK) model, searching for phases that have some characteristic property similar to integrable ones, where charge-density wave (CDW) can slide freely without pinning, and those, further from integrable ones, where pinning occurs.

The manuscript is, from my point of view, easy to read and, except for some points I will discuss afterwards, scientifically reliable. However, I believe that a publication in SciPost is not possible without major revisions, on the following grounds.

1. This article mostly consists in a pedagogical course on commensurate and incommensurate one-dimensional systems. A lot of calculations are reported, but they are improved versions of existing ones. Some results are new, but only partially explained. So, new results mainly consist in the numerical ones. To my opinion, this is not an irremediable reason for refusing this article, but the balance between established and new results is not equilibrated and it is hard to say, whether it meets the journal's criteria or not.
2. There are too many repetitions in the text.

- The definition of "integrable" as "exactly solvable" is given three times and, at the fourth apparition of "exactly", the authors write "integrable" in the following sentence.
- "r_n and $N_n$are irreducible" is repeated within 5 lines (and the expression is, by the way, incorrect, see detailed corrections).
- "there are S unit-cell" is also repeated at very close occurrences.
- "functions of I_m" or "functions of a_i" is repeated very often, some of these repetitions can be avoided.
- "The BDK approach […] fixes […] band …" after (45) was already stated implicitly in the previous sentence (this is an opportunity to suppress "BDK approach", see afterwards).
- The explanation of approximant sequences is repeated (also see point 4).
- Definition of S is repeated.
- "The sum in Volterra equation are only for even *m*" is repeated several times.
- Several equations are strictly repeated (like (100) and (136), (106) and (135) or (85) and (98)).
- And many other repetitions, some of which however may not be retracted.

3. An important clarification must be added in the introduction. The authors recall the classical Fröhlich approach, with the simple Peierls model, where rational fillings lead to the pinning of CDW while irrational ones lead to free sliding of CDW. This approach, although very seducing, has never been fully confirmed experimentally; the origin of the pinning of CDW is still debated. The conclusion in Ref. 13 are controverted. So, the authors must rewrite the end of the first paragraph (after (1) and (2)) and the following, in order to make the hypothetical character of Fröhlich approach clear. I am not concerned here with the fourth paragraph ("The above …"), which deals with nonlinear terms in a rather naïve manner and which I prefer not to comment.
4. I have some mathematical questions. They are all of minor importance, except the first one:

- it concerns Eq. (64), in which the number of terms is g+2, since there are only g+1 independent coefficients. How do the authors establish that the first *g*+1 I_m are indeed independent? One could imagine, on the contrary, that the last coefficients I_{N-g}, …, I_N were independent and not those. I have checked carefully in Appendix D (in particular Eq. D6) and in the references, which are quoted about BDK extended model, but could not find any hint of this question. If this formula were to be found incorrect, I imagine that it can be fixed by sorting coefficients with a

special order. However, in the discussed case of section B, where N=4, the order matters, so, at least, the result must be established in that case.

- Connected to this question, I have noticed that the first possible degeneracy mentioned after (60) concerns exactly e_1. Why this index $\nu = 1$? It seems to be connected with the fact that degeneracy occurs at index $k = 0$ or index $\nu = \pm\frac{\pi}{N}$, but there is very little discussion on parameter index k in the text. Answering this question could potentially solve the previous one.
- Still may-be connected to the same question, at the end of the paragraph following (97), the authors explain that eigenvalues appear in a different order but no indication is given, which would help the reader to understand which order appears.
- All variables with an index, e.g. a_3, at the power of 2 (or more) are written a_3^2 but should be written ${a_3}^2$ to avoid any confusion.
- Why is, after (11), the case $\kappa < \frac{1}{\lambda}$ and $\lambda < 1$ excluded ?
- The definition of the envelop function is given as the limit of the infinite systems; as it is explained, this is unambiguous for incommensurate phases, but one must use a trick to handle with commensurate ones, using again rational sequences although the limit is rational. All this is very well known and correctly written. The authors however have sometimes used other definitions, which allow to draw the envelop function in discrete cases. Not only these definitions are ambiguous, but they contradict the latter. So I have removed all references using this second definition.
- After (13), S being the number of unit-cell is something obscure for non-specialists but it becomes more clear afterwards. May-be the authors can add "as will be clear in the following" or something equivalent.
- The construction of the approximant sequence before (15) is not in itself incorrect and is actually unique but **any rational sequence converging towards a real number** c **would be convenient**. Remembering that this paragraph deals with academic matter, which have been longtime ago well described, I would suppress the whole description, including the golden number example and its figure, which are unnecessary, and keep a single sentence before (15). Moreover, this explanation is duplicated in the text, unnecessarily.
- The description in (20) can be improved, using vertical or oblique dots.
- If one looks with care Eq. (55) and following ones, it appears that index ν in ρ_ν is completely useless and should be suppressed for the sake of clarity. In particular, with index ν, the sentence before (56) is formally wrong.
- In the same Eq. (55), I don't understand the suppression of spin degeneracy factor 2.
- Eq. (100), contrary to previous ones, only appears in the conclusion of part III.
- Module writes "*m* (mod *n*)" and not "*m*(mod*n*)".
- "to quarter filling" after (107) contradicts appendix C. I think the appendix is correct so this must be discarded.
- I have suggested a more clear notation $\sum_{m=0}^{2} I_{2m}$ instead of the authors' one, when the sum is restricted to even m.
- The sentence, after Fig. 6, beginning by [sic] "It is, indeed, clear that…" is wrong. Looking carefully at Fig. 4(a), as stipulated, one can observe both situations were $Q'\big(E_1(k)\big) > 0$ and $Q'\big(E_1(k)\big) < 0$. Either some explanations are missing to interpret Fig. 4(a), or this sentence must

be discarded. I believe that it has no consequences, because this demonstration is only written to let the reader agree with Fig. 6. There is no reason, however, to doubt that Fig. 6 is valid.

- The phrase in part IV C §2 "This result is independent on the fractional…case" must be removed definitely from the text, since it is not entirely true. Numerical methods cannot reach the infinite limit. What is true is that, whatever sequence is chosen, one finds the same numerical limit, which relates to the definition of the incommensurate phase. But this does not correspond to the numerical realization here and keeping the sentence would be scientifically dishonest.
- I have added a general precision about polynomials p_i in Appendix A, because the text only mentioned p_1 and p_4, but I have hesitated about the fact they are algebraic.
- In Appendix B, I have added a whole phrase, because the authors had not explained that (B1) is already obtained by a perturbation theory at first order.
- The change of sign of I_4 in (C4) in Appendix C is totally inacceptable. The authors must synchronize notations, whatever their choice.
- In (D2), I have added a tilde sign of R_{2g+2} because nothing indicates that a factor would not appear, that leads to a different polynomial. It is actually very plausible that no tilde sign would be needed, so the authors should write these calculations more explicitly and give more indications.
- In Appendix E, before (E1), the "(opposite)" indication is very puzzling and completely contradicts the following equations, where, indeed, the direction is well defined by the gradient; so why this possible change of direction?

5. I do not understand, in the discussion of part IV, why 0 gap states are discarded because they have higher energy while 3 gap ones still appear in the synthetic diagram of Fig. 6. It could result from the curves of 0 gap states in Fig. 7, which never come close to 2 gap ones, contrary to the 3 gap ones (this is coherent with the phase diagram where ξ_4 is close to -2,5). The role of energy difference should be explained in more detail.

- Why is there a "bare ϕ" in the discussion after (129)? It would be linked to the renormalization of ϕ but this is not explained.
- There is another problem in the same discussion: the minimization process is done whilst keeping a_1 constant. Doesn't this induce a bias? One could believe that it does.
- The structure in Fig. 15 does not appear as uniform as claimed in IV C §2. The § should be rewritten in order to discuss the differences between the authors' numerical results and the expected uniform structure of Ref. 62.
- The phase discussion at the end of the main conclusion appears abruptly, the whole article hardly discusses the phase discontinuities. Some insights on this matter would be useful.

6. Eventually, the text contains a huge number of English mistakes, or style misuses.
    - For instance, e.g. is often used improperly,
    - comma often separate a verb and its direct object,
    - "reads" is used instead of the correct "writes", etc.
    - I must precise that none of these mistake is done systematically.
    - The authors also abuse of telegraphic or colloquial (like using "that" instead of "which") or, on the contrary, pompous style.
    - The past is sometimes used without necessity.
    - There are many Gallicisms, either.
    - I suggest to suppress all expressions such as "it is easy".

- In the same trend of ideas, I think that it is not elegant to address the authors by BDK.
- The authors address the two models studied by Brazovskii, Dzyaloshinskii and Krichever in many ways, I suggest to use, when needed, BDK-Volterra and BDK-Toda in the whole text.
- I suggest to change all “we see” by “one sees”, “we need” by “one needs”, etc.
- I also suggest to change some “we have” into “one gets” or similar, but some “we have” are justified.
- I also suggest the suppression of despising or excessively detailed explanations in the corrected manuscript.
- On the contrary, I have suggested the transformation of some indications in parenthesis by explicit sentences.
- In the text, “special” is used with a particular meaning, concerning BDK model, so I have suggested not to use this term on other occasions.
- The authors systematically write 1D or 2D for one-dimensional and two-dimensional. This abbreviation is commonly admitted, so, although I prefer explicit writing, I do not correct it.
- Idem about r.h.s. (right hand side).
- Other mistakes are described in the corrected manuscript corrCepasQuemerais.pdf, which link I write after this report. I have tried to use constant notations and apologize for all mistakes. I use generally green color, when it concerns an improvement (or what I hope an improvement), and red for serious mistakes (not only English misuses but heavy repetitions, incoherencies, mathematical or physical mistakes or imprecisions, etc.). I could only write by hand, for it would have taken too much time (I have calculated about 40 hours) otherwise.

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