Response to referee 2.

We would like to thank the referee for his tremendous work to improve the paper and its clarity. We have incorporated almost all of his advices and we think that it improves the paper a lot. We answer below various questions.

1. Both referees are concerned by a lack of clarity about what is new. To make it clear, we have rewritten part of the introduction and of the abstract. What is clearly new is the existence of an Aubry transition when the BDK model is perturbed by the nonintegrable terms that we have introduced. This is the result of our numerical work. Regarding the first part and the derivation of the analytic solution when the model is exactly solvable, we do not feel that it is a simple rewriting (or review) of the original derivation, but a one that stresses and tests the limits of applicability (in particular when the model is perturbed by other integrable terms). A reader interested in understanding the original article will have to go through (as we have done) a great deal of literature of the 1980's on classical integrable models (the referee can consult the original article). We feel that it is important to reformulate some arguments, make the points more readable and pedagogical (both aspects that the referees have acknowledged). This part is therefore as original and "new" as the other part.

2. We have suppressed all unnecessary repetitions. However, sometimes, there is a delicate balance between repetitions and clarity and we have kept some of them (in particular a few equations are duplicated in order to be able to read the paragraphs independently).

3. It is true that the origin of the pinning has never been clearly established. A current debate is in terms of intrinsic versus extrinsic (impurity-based) mechanisms. This is why the subject and the possibility of intrinsic pinning is worth pursuing, from our point of view. We have now made that discussion in the introduction clearer.

4. - The functional independence of the I_m has been established in the 1970's. Eq. (64) (now Eq. (63)) is a sum over $g+2$ (not $g+1$) independent δI_m terms. It applies to Toda's case. From the original $N+1$ I_m terms that can varied independently, each closed gap gives a linear dependency relation between all δI_m. For g gaps, there are $N-1-g$ such relations, reducing the number of independent terms to $N+1-(N-g-1) = g+2$. It does not matter which ones are used in Eq. (64) (now (63)): one can express the variation of energy either in terms of the first $g+2$ δI_m or the $g+2$ last ones. In Appendix D, this is explicitly proved, leading to Eq. (D16). We have improved the presentation of Appendix D.

     For $N=4$ (Volterra), the argument is simpler to follow. δE_{elec} is in general a sum of all δI_m, $m = 0, 2, 4$ ($N/2+1$ terms for Volterra).

If the gap is closed at $E = 0$ (there remains $g = 2$ open gaps), one gets a dependency relation, Eq. (112) (new numerotation). One can eliminate delta I_4 in favor of δI_0 or the contrary. One gets a sum over two terms δI_0 and δI_2 (or δI_2 and δI_4). Eq. (64) (now (63)) gives precisely that: the sum runs from $m = 0$ to $g+1 = 3$ with even m, i.e. is a sum over 2 terms.

We think that there was a possible confusion as Eq. (64) applies to Toda's case only and we have made that clear. The order does not matter, however.

- Eq. (60) used e_1 as a notation for any double root. It was not aimed to say that it is the first. A better notation is e without any index which should suppress the confusion.
- The question addresses the position of the double roots with respect to the single roots (i.e. the position of the gaps). This is a different question from the previous ones. We mentioned in paragraph III.E.1 and III.E.2 that the order is fixed by k_0 for the particular 1-gap solution. We have not found any mathematical proof in the literature but we have added the note [55] which makes it physically transparent.
- We have corrected for example a_3^2 to ${a_3}^2$.
- Although l_0 is also positive in the referee's case, W'' is negative (maximum of energy), so the system is unstable. We have changed the sentence so that it includes the referee's case.
- ok.
- It is true that the notation is not completely standard, here N is the periodicity (not S) but we have adopted it for the connection with integrable models for which N is the number of sites (it should be the same N). Contrary to standard treatments of the Aubry transition (with one large single cell), we use Bloch waves and a large number of repeated unit-cells.
- It is of course true that any rational sequence converging towards a real number c would be acceptable. The one we use (based on continuous fraction) is convenient and converges fast enough without requiring too long periods. We have shortened the paragraph, which is well-known matter indeed.
- The matrix (Eq. (20)) has been rewritten more clearly.
- The index ν is not needed indeed, we have removed it.
- The spin degeneracy factor is included.
- ok.
- $m \pmod n$ replaces $m(\mathrm{mod}n)$.
- After (107), a coma was missing, it is not the band that is quarter-filled indeed. The "at quarter filling" is simply removed.

- The notation $\sum_m I_{2m}$ is more compact than specifying a sum over even m and is adopted.
- The sentence after Fig. 6, "It is, indeed, clear that..." is correct, in fact. It is true that $Q'(E)$ can be positive or negative but $Q'(E_1(k))$ is always negative, whatever k for $N = 4$ (or even N).
- The phrase "This result is independent on the factional...case" is removed.
- Changes suggested for the Appendix A (polynomials $p_i(E)$) have been adopted.
- Idem for the Appendix B.
- We have uniformized the sign of I_4 everywhere, to avoid two different confusing notations.
- The referee is correct, we must introduce a new notation for that polynomial which we have done.
- The standard algorithm follows the direction of the negative gradient. We have changed opposite by negative.

5. Fig. 5 (new notations) shows the regions where extrema are found. The 3-gap phase is an extremum but not the ground state in the gray area. It is true that the 0-gap phase is also an extremum (not the ground state) in the same area where the 2-gap and 3-gap phases are found. We have added a sentence in the caption.

   - "bare ϕ" is an english mistake. On must read "except ϕ", which is now corrected.
   - Fixing a_1 is done only for the result of Fig. 9 (new numerotation) and for convenience: this is to organize the numerical results as a function of a phase ϕ. Anywhere else, a_1 is not fixed (otherwise indeed it would introduce a bias).
   - Fig. 16 is a "uniform" structure as defined in Ducastelle et al. [63]: a certain number of dimers are placed in such a way as to minimize the repulsion. For the 5/13 structure, one makes two groups of 2 dimers and a single isolated dimer with a special order (see Ref. [63]).
   - In the last sentence of the conclusion, by the "phases are discontinuous" we did not mean the "phase" of a wave, but a physical "state". We have modified the sentence to avoid the confusion.

6. We thank the referee for taking the time to point out our english mistakes and we have corrected almost all the points raised. A few expressions, such as "the SSH model", appear to be both of common use in the scientific literature and not grammatically incorrect, according to a native english speaker whom we questioned. We do not have any personal prejudices and are happy to change these points, depending, for instance, on editorial style. Some remarks we made, if sometimes perhaps too basic, were not aimed to be despising. We have corrected them too.