Comment on "Exploring complexity of a Frenkel-Kontorova-Type atomic chain" – For Physica D – Dissipative Frenkel-Kontorova chains

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Abstract

We rebut the use of a questionable method, DSM iteration (dissipative standard map), for calculating a finite dissipative Frenkel-Kontorova chain. It should be replaced by a standard optimization method, connected for instance by the Newton trajectory method.

Keywords: Dissipative Frenkel Kontorova chain, Newton trajectory, dissipative standard map

1. Introduction: the FK model

In recent contributions to this Journal [1, 2] Afsar and co-workers reported on computational studies of dissipative Frenkel-Kontorova (FK) chains. Dissipative here means that the springs between all particles x_n in the chain are different. The positions are on the x-axis with $x_n < x_{n+1}$. Our criticism is directed against the use of a method for calculating the energy of an FK chain, as well as of the formula of the energy itself

$$H = \sum_{n=1}^{N} (1-\gamma)^{-n} \left[(x_n - x_{n-1} - \Omega)^2 - \frac{K}{(2\pi)^2} cos(2\pi x_n) \right] . \quad (1)$$

The *cosine* function represents a substrate potential in which the chain is embedded. Sometimes it is named on-site potential (Pot). Often one depicts H the potential energy hypersurface (PES), $H(\mathbf{x})$. The dissipation is caused by the γ-factors with $\gamma \in (0, 1)$. However, the given ansatz (1) appears questionable, as it extends dissipation to the on-site potential as well. This, in contrast, should be independent on the random filling with an arbitrary x_n. The on-site potential should be a normal periodic potential, as shown in Fig. 1 of ref. [2]. We propose to use

$$H = \sum_{n=1}^{N} \left[(1-\gamma)^{-n} (x_n - x_{n-1} - \Omega)^2 - \frac{K}{(2\pi)^2} cos(2\pi x_n) \right] \quad (2)$$

where a pair of brackets is shifted. Eq.(2) in [1] begins with x_0 in the sum, so that the chain has $(N+1)$ particles, not N. (But x_0 can be fixed, it is not directly subject of optimization.) For x_0 there is no substrate potential in Eq.(1) in [1]. We have freely set it to zero energy at coordinate $x_0 = 0.1$ (as in other cases, see below).

The gradient system of Eq.(2) has a slightly different form than in [1]

$$0 = \frac{\partial H}{\partial x_n} = -(x_{n+1} - x_n) + (1-\gamma)(x_n - x_{n-1}) + \gamma\,\Omega + \frac{(1-\gamma)^{n+1} K}{4\pi} sin(2\pi x_n),\ n = 1, .., N-1 . \quad (3)$$

A factor of $\frac{(1-\gamma)^{n+1}}{2}$ must be used at the *sin* term for our ansatz (2); in the original formula (1), the larger factor of $\frac{(1-\gamma)}{2}$ would be missing. The missing factor is (in every case) already incorrect in refs. [2, 3]. Therefor, the FK model is not suitable for a DSM ansatz (dissipative standard map). Work [4] discusses a DSM formula for an FK chain, but the authors do not provide an explicit formula for the FK energy. So, we cannot accept their approach. We believe that the original intention of the DSM in refs. [3, 5–9] is not fulfilled by either of the two energy formulas. Note that the original development of the DSM was to model a kicked rotator [7, 10] or a charged particle in the electric field of a electromagnetic wave packet [8]. These were not FK models.

In case (1), one could apply a factor $\tilde{K} = (1-\gamma)\,K$ with an adapted constant for K. Thus, the iterations of DSM could be corrected, however, a deeper error is that system (3) in [1] does not take into account the last equation for particle x_N. This additional last line for system (3) for x_N

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is

$$0 = \frac{\partial H}{\partial x_N} = x_N - x_{N-1} - \Omega + \frac{(1-\gamma)^N K}{4\pi} sin(2\pi x_N) \ . \quad (4)$$

However, the $(N-1)$th equation in (3) is

$$\frac{\partial H}{\partial x_{N-1}} = -(x_N - x_{N-1}) + (1-\gamma)(x_{N-1} - x_{N-2}) + \gamma\,\Omega + \frac{(1-\gamma)^N K}{4\pi} sin(2\pi x_{N-1}) \quad (5)$$

and this contradicts Eq. (4) for the variable x_N in the general case. Consequences are explained in Section 2 below.

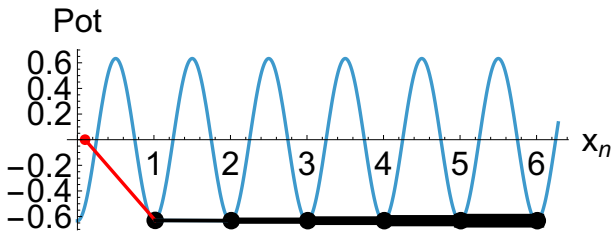

Figure 1: Symbolic picture of the global minimum of the FK model of Afsar et al., modified to formula (2). Pot is the energy of the on-site potential. The red boundary point is set to $x_0 = 0.1$. Other x_n (black dots) are optimized. For better visibility, the particles are shifted on the substrate potential. The varying strengths of the springs between the x_n are slightly indicated by the increasing thickness.

The authors of ref. [1] fix x_1, whereas we fix x_0; this is an equivalent action. (For a fixed x_1, the first equation in system (3) would be useless, but then would be x_0 open, and we had an $(N-1)$-dimensional gradient system but with an analogous property.) Note that we do not have a free-end chain as it is claimed in the abstract and in line 1 of section 2 in [1]. This point is important; it is another model. Using models (1) or (2) and excluding a fixed particle x_0, the chain with a free x_1 will have another minimum structure, in comparison with the chain with x_1 connected to a fixed x_0.

For a finite N, an optimization calculation can be performed for the structure of an equilibrium chain, see Fig. 1. We use $K = 25, \gamma = 0.9, \Omega = 1$, and $N = 6$, particle x_0 is fixed and x_1 can move. A corresponding minimum is at
x=(0.1, 1.02, 2.01, 3.01, 4.01, 5.01, 6.01) with H=-3.65.

It is obvious that the model of strong strings in Eq.(2) forces a chain close to the lowest points of the on-site potential with distances $x_n - x_{n-1} \approx \Omega$. This also occurs for some further possible minima and a saddle point (SP) in Fig. 2. The different minimum structures are moved on the axis; they arise by shifting x_0 on the interval (0,1) as in [1], but above all, by different start chains for the optimization. We have found the chains:
a) **x**=(0.05, 0.19, 1.12, 2.12, 3.12, 4.12, 5.12), H=5.31
b) **x**=(0.31, 1.07, 2.05, 3.05, 4.05, 5.05, 6.05), H=-1.54
c) **x**=(0.61, 1.91, 2.94, 3.94, 4.94, 5.94, 6.94), H=-1.11
d) **x**=(0.05, 1.83, 2.89, 3.9, 4.9, 5.9, 6.9), H=3.67

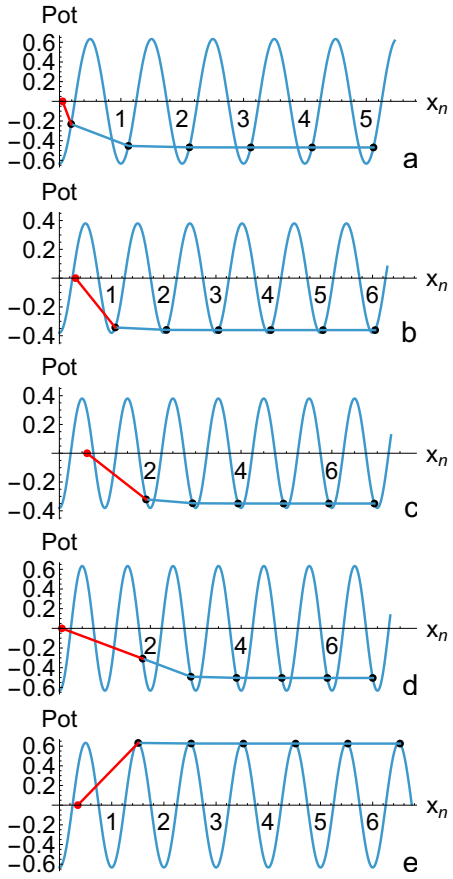

Figure 2: Symbolic picture of FK minima as in Fig. 1 for (a) $K = 25$ and $x_0 = 0.05$. We start with a movement of the chain to the left hand side, and indeed there is such a minimum. For the weakest spring between x_0 and x_1, the distance is reduced. (b) $K = 15$ and $x_0 = 0.31$: The chain has a little moved but the particles still are in their bowls. (c) $K = 15$ and $x_0 = 0.61$, we start with a movement to the right hand side, and there is a minimum where the chain is moved one bowl further. (d) $K = 25$ and $x_0 = 0.05$, we start with a movement to the right, and there is a chain in a labile equilibrium structure, a shoulder point with a zero eigenvalue of the Hessian matrix. (e) is an SP of index 1.

State (a) is an anti-kink, whereas state (d) is a kink. Both states have more energy. However, these states cannot move like a soliton [11, 12], due to the dissipative nature of the potential (2).

Various stationary chains in Fig. 2 show that there is more than one minimum structure. There is also a dependency on K. For low K values, the strong strings in Eqs.(1) or (2) lead to a simple chain with $x_n \approx n$. There is also a transition state, a normal SP of index one, at
e) **x**=(0.35, 1.51, 2.52, 3.52, 4.52, 5.52, 6.52), H=4.04,
where all particles x_n are more or less at the peak of the on-site potential, again with distances between the particles at $\approx \Omega$. This transition state depicts the step from a chain of type (a) to a chain of type (c) or (d).

In general, stationary structures can be found using the method of Newton trajectories [13–15], see section 3.

2. The DSM iteration works not correct – an intrinsic comment

If one fix $p_1 = x_1 - x_0$ (a logical continuation of the p_n-definition downwards in [1]), this determines x_2 in the first equation of system (3), then x_3 in the next, and so on, i.e. the entire DSM iteration of [1]. However, the system of N equations cannot be solved by a prescription of one variable. Because the last equation (4) is usually not satisfied.

Of course, if one starts with the 'correct' x_0, x_1 of a minimal chain, i.e. a correctly fixed p_1, the DSM iteration works correctly. But note that any tiny numerical deviation of an particle x_n (by an approximation of π or the *sine*-function) causes the entire end of the chain to deviate. As N increases, such numeric errors can accumulate.

However, starting with random boundary values will fail. In the FK chain, one can it imagine by the back action of the last particles on x_1. This means that the claim that (x_1, p_1) can be chosen at random is incorrect. Most of the calculations of paper [1] are false, if they are understood as values for an FK chain in equilibrium with a fixed x_0. If an iteration starts incorrectly, all other values are also false.

And if N becomes larger in ref. [1] then the exorbitant deep harmonic potentials in (1) or (2) for $\gamma = 0.9$ also enforce a distance that is quasi equal to Ω between every pair x_n and x_{n+1} for larger values of n. Thus, different equilibrium structures of the chain seem to be possible only for different movements of the first particles of the chain, as it is the case in Fig. 2.

We firmly refuse the results regarding the bifurcations of (FK minimum structure)-energies. They are connected with the (incorrectly) used dissipative standard map iterations with incorrect starting points. We point out that we already discussed the corresponding problems several years ago [15–19]. The point is the fundamental failure of the so-called Aubry theory. Works that use this theory, see

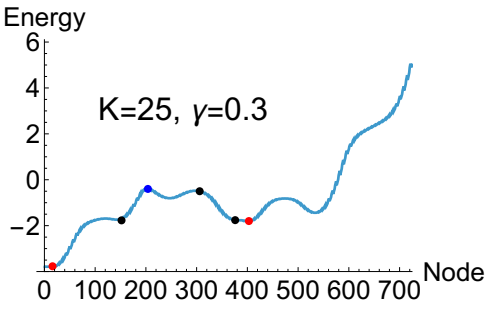

Figure 3: Example of an energy profile over an NT for an FK calculation where H of Eq.(2) is the energy. Node counts the number of base points of the calculation. Start is at the global minimum. Red dots are minima, black points are SP_1 and blue is an SP of a still higher index. Further seemingly stationary points are turning points of the NT in the PES mountains.

[3, 20, 21] to name but a few, shift the boundaries of the chain to $\pm\infty$. However, this is not manageable for computational studies. Therefor, like the works [1, 2], they start at arbitrary points on the iteration ladder, thus with arbitrary start points. Here, too, we must conclude that this way is incorrect: Ex falso quodlibet.

From the point of view of Mathematics, we could not find any proof of convergence in the corresponding papers. This would be necessary for handling with infinity.

A scurrile way to verify some of the calculations of Refs. [1, 2] for a finite N could be to compare the final values of x_N of the two Eqs.(4) and (5). If these are equal, the entire chain is a correct stationary state. We guess that very few structures in [1] meet this condition.

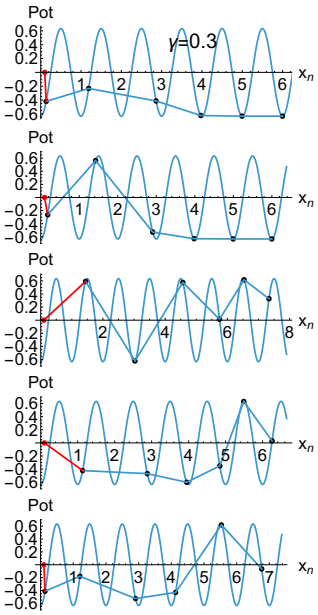

Figure 4: Symbolic picture of FK structures to model (2) with $K = 25$ and $\gamma = 0.3$. Line 1 is a minimum with an anti-kink at 0 and a kink at position 2. Along the profile in Fig. 3, starting from second line: SP_1 with an anti-kink at 0 and a kink at 1 and 2; an SP_3 with three particles on the tips of the on-site potential; two further SP_1 with first a kink at position 2 and an anti-kink at 5 and 6; and the second has an anti-kink at 0 and a kink at 5 and 6.

3. Application of Newton trajectories (NT)

An NT is a curve in which, at every point, the gradient of the PES points in the same normalised direction $\mathbf{f}$

$$\mathbf{g}(\mathbf{x}) = F\,\mathbf{f}\,, \tag{6}$$

where $\mathbf{g}$ is the gradient of the PES. The value F is the norm of the gradient. It changes as a non-linear parameter along the solution.

There is a differential equation along the curve [22]

$$\frac{d\mathbf{x}}{dt} = Det(\mathbf{H})\mathbf{H}^{-1}(\mathbf{x})\,\mathbf{g}(\mathbf{x})\,. \tag{7}$$

$\mathbf{H}^{-1}$ is the inverse of the Hessian matrix of the PES, and $Det(\mathbf{H})$ is its determinant, and t is the curve length parameter. Solutions to Eqs. (6) and (7) are denoted as NT.

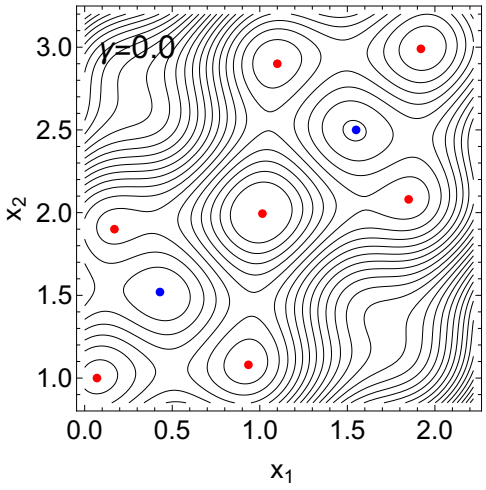

Figure 5: Level lines of the PES for $K = 25$, $\gamma = 0$, and projection in the plane $x_3 = 0.5\,(3 + x_1 + x_2)$. Red points are minima, SPs of index 2 are blue.

Eq. (7) was formulated by Branin [23]. General solutions of Eqs. (6) and (7) in different directions $\mathbf{f}$ connect a minimum with a saddle point (SP) of index one, or they connect higher index SPs of an index difference of one. In this case of a dissipative FK model, the Hessian matrix quickly assumes very large entries as N increases. This makes the calculation somewhat complicated.

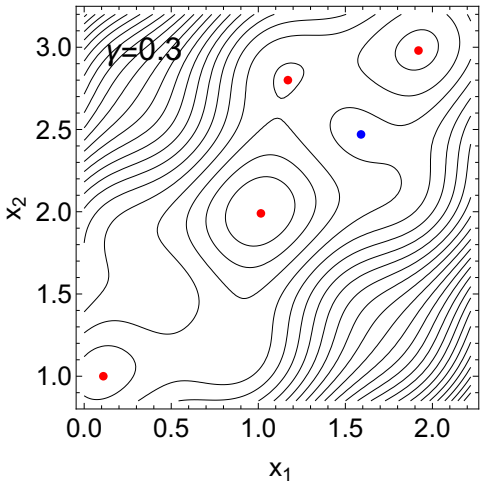

Figure 6: PES for $K = 25$, $\gamma = 0.3$, projection in the plane $x_3 = 0.5\,(3 + x_1 + x_2)$. Red dots mark minima, one SP of index 2 has disappeared, as well as many minima of Fig. 5.

3.1. Example

We try $\gamma = 0.3$ and form a model close to a typical FK chain, which is mild compared to the strong harmonic forces between the particles in the dissipative model [1]. We use for the search direction the vector

$F\,\mathbf{f} = (-0.3, -0.15, -0.05, 0.2, 0.1, 0)$

where we 'pull' three particles to the left side, and 'push' x_4 and x_5 to the right side. For the usual constants $K = 25$ and $\Omega = 1$, we obtain an energy profile in Fig. 3. We can identify many stationary structures along the NT, see Fig.4.

It is obvious that the slight change in the usual FK model by $\gamma = 0.3$ allows us to explore the PES over wide ranges. However, the known solitones of the case $\gamma = 0$ do not occur here. And it is obvious that the NT of Fig. 4 forms loops.

The favourable possibilities for exploring the PES change with a higher value for γ. We try to explain this circumstances with some PES contour line pictures for different γ values. We start with $\gamma = 0$ in Fig. 5 where x_1 and x_2 are symmetric for their harmonic potentials. In order to draw a two-dimensional figure, we project out the remaining particles. We only need to consider the distance to particle x_3. We use the plane $x_3 = 0.5\,(3 + x_1 + x_2)$. It is clear that there are many paths for internal movements of the particles within the chain.

Next we study the case $\gamma = 0.3$ in Fig. 6.

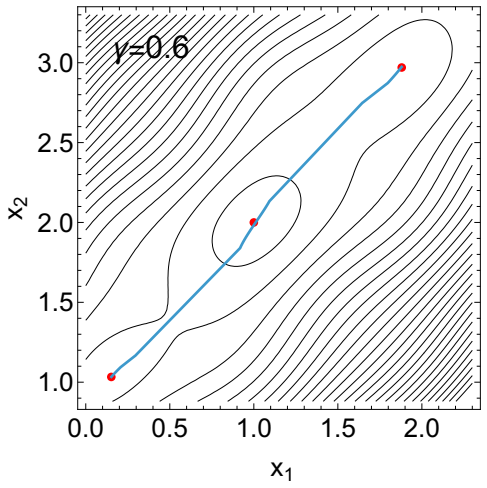

Figure 7: PES for $K = 25$, $\gamma = 0.6$. We use a skew plane of x_3 for the projection. The blue line is the projection of an NT, see text. Red dots are minima. The central minimum is the global minimum at $(x_1, x_2, x_3) \approx (1, 2, 3)$. The SPs of index 2 from Fig. 5 have disappeared, as have many other minima.

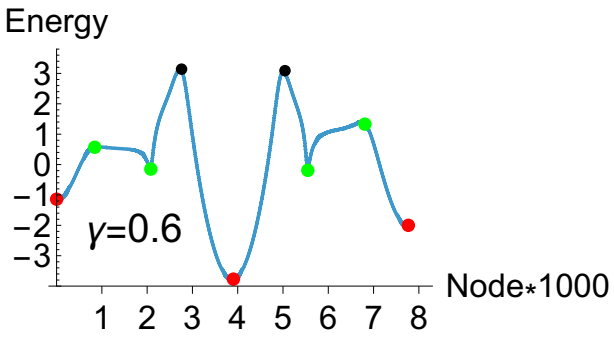

Figure 8: Energy profile over an NT for $\gamma = 0.6$. Start is at the left minimum in Fig. 7. Red dots are minima, black dots are SP_1. Further seemingly stationary points in green are turning points of the NT on the slope of the PES.

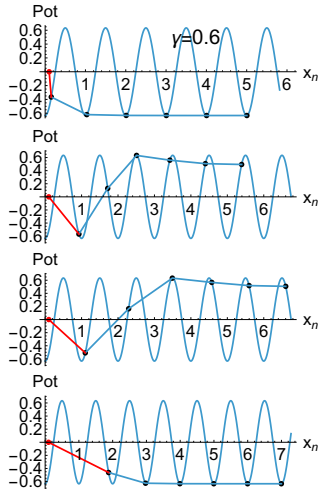

Figure 9: Symbolic picture of FK structures to model (2) with $K = 25$ and $\gamma = 0.6$. First two lines are a left anti-kink minimum, and a subsequent SP_1, which can be seen in Fig. 7. The global minimum is omitted. Along the profile of Fig. 8 we then have an SP_1 and a kink of particle 2 at places 2 and 3; and the last is a minimum with a kink of particle 1 at place 2.

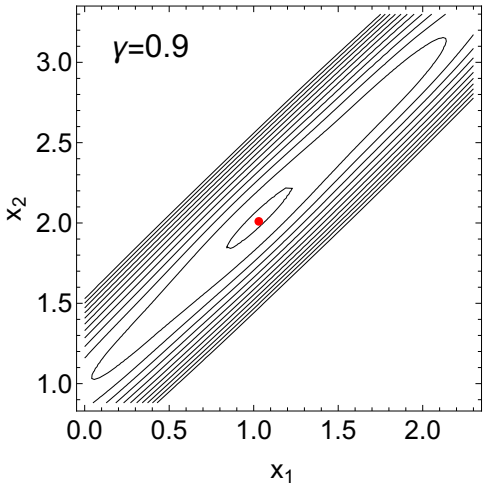

Figure 10: PES for $K = 25$, $\gamma = 0.9$, projection onto the plane $x_3 = 0.5\,(3 + x_1 + x_2)$. In this plane, only one red dot marks a remaining minimum.

The case of $\gamma = 0.6$ in Fig. 7 becomes even stranger. We draw a projection of the contour lines in a skew plane $x_3 = 0.5\,(3 + x_1 + x_2)$. With this view we can observe three minima and two saddles in between. The blue line is an NT which connects all stationary points. (The NT is calculated in the complete 6-dimensional space; here is only shown the 2-dimensional part.) We draw the profile over the NT in Fig. 8. One may observe that such a calculation is already difficult: the steep slopes in Fig. 7, at the 'corners' at the top left and bottom right, indicate that only NTs with a small range of search directions can be calculated well. This direction is determined heuristically, by trial and error. The NT is found with the search vector $F\,\mathbf{f} = (0.2, 0.3, 0.4, 0.45, 0.5, 0.5)$ which is a movement of all particles in one direction. We had to use a very small step size for the calculation, and the convergence is poor. The occurrence of turning points (shown in green in Fig. 8) indicates the increasing difficulty with rising γ. We tried other search directions, but we could not find any with an NT without TPs. The stationary points (apart from the trivial minimum) of Fig. 8 are depicted in Fig. 9. The problems arise here because the spring between x_2 and x_3 becomes much stronger in this dissipative model. For $\gamma = 0.6$ we have strength factors for the first spring 2.5, for the second 2.5^2, but for the third by 2.5^3, and so on, up to 2.5^6. The chain becomes very stiff, compare Fig. 9. The development from $\gamma = 0$ to $\gamma = 0.3$ to $\gamma = 0.6$ shows the increasing stiffness of the corresponding chains. The larger γ becomes, the stiffer the chain of the right 'end'- particles becomes. Only particles x_1 and x_2 can form compressed or stretched structures. This is even more extreme in the structures of Fig. 2 for $\gamma = 0.9$. It is also demonstrated in Fig. 10 where only one minimum remains in the main valley. From this projection one would not expect the other stationary states of Fig. 2.

Here we can argue for our choice of only $N = 6$: examining longer chains up to $N = 100$, as in Refs. [1, 2] does not change the situation. The long 'right' end of the chain appears fixed.

4. Conclusion

The papers [1, 2] do not correctly describe a Frenkel-Kontorova model, as the titles claim. In the general case, one cannot use the dissipative standard map with random start to find stationary structures of a finite FK chain and their energies. The DSM iteration itself may be correctly reproduced with its bifurcations, but its fundament is open.

Dissipative FK chains are physically 'boring' for large $\gamma > 1/2$: they can only have different stationary structures at the left end of the chain. Note that there is also another definition of a dissipative force for FK chains [20, 21] which we do not discuss here.

Authorship contribution statement

Both authors contributed equally.

Data availability

The data will be made available upon request, as will the Mathematica procedures for minimising and for representation of the figures, and a Fortran program for calculating the NTs.

Conflict of Interest

There is no conflict of competing interests.

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Appendix

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Second offer for a major revision: 14.7.2026
Rejection: 4.8.2026

After a long time-lasting until August 4, 2026—and several revisions, the journal did not accept our Comment.

We have added the "discussion" with the so-called Reviewer No. 1 to the first version to show the reasoning we had to employ.

Answers to reviewer #1

Reviewer:
The current manuscript cannot be compared with the works of Afsar et al., as claimed by the authors, for the reasons I have detailed below.
1) Upon examining references [1,2] in the present manuscript, it is evident that Afsar et al. utilized a modified version of the Frenkel-Kontorova (FK) model containing dissipation, derived from the definition of an infinitely long atomic chain (infinite number of particles).
Answer
We reject this argument: References [1,2] use a finite number N of particles. Never in these papers are supposed infinite chains; the combination "infinite chain" does not emerge, at all. Such a connection is a free speculation of the reviewer. And note: nowhere in reality does an infinite chain exist. Furthermore, we could not find a correct mathematical limit proof in any of Aubry's works, nor in that 3500+ Aubry citations. There are merely statements about infinity, but not a single credible proof! The reviewer appears to be one of the 3500+ followers of Aubry's theory of the FK model. Hence, the grumbling is understandable. But the review #1 demonstrates that we have no possibility to teach his (or her) understanding.

They formulated their equations (see the force equation) based on this infinite chain and subsequently focused on the dynamics of N=100 atoms within it. It is clearly seen from the force equations in these studies that the Hooke interaction between atoms is at the nearest-neighbor level (X_{n+1} - X_n), and the force equation is written for the static equilibrium state of atoms with all indices n in the range $-\infty < n < +\infty$, as in the standard FK model [1]. According to the FK model approach, any atom in the middle of the focused 100 atoms only interacts with the atoms to its right and left; it does not "know" whether the system is infinite or not. These atoms only feel the forces exerted by their neighbors. Therefore, the configuration within this small chain segment actually reflects the characteristics of the infinite chain at that specific energy level. In other words, the static equilibrium configuration of the selected segment is part of a pattern that repeats itself or changes according to a specific rule throughout the infinite chain. For example, if the equilibrium configuration of the infinite chain is in a periodic or quasi-periodic structure, the selected segment behaves as a representative sample of this structure. It is clear that the authors in [1,2] established their equations over an infinite chain and then focused on a segment of N = 100 atoms.
Answer
See above answer. Even if the atoms only "feel" their neighbors, the finite chain forms a single unit in an equilibrium state. All other claims are neither proven nor demonstrated, for example, the existence of an infinite chain.

In the present manuscript, however, a segment of N+1 atoms has been "cut and detached" from an infinite chain. What I wish to emphasize is that the first (x_0) and last (x_N) atoms of this detached segment, unlike the approach of Afsar et al., interact only with the atoms on their right (with a fixed x_0) and left sides, respectively. As the authors also state, while examining the remaining N-1 atoms within the framework of the force equation, one must pay attention to the imaginary ends of this chain or the boundary conditions to be employed. Consequently, it is a known fact that if we neglect one neighbor of the atoms at the ends of the chain, the equilibrium configuration will vary depending on the chosen boundary conditions (fixed or periodic). While the N=100 atom segment in the approach of Afsar et al. represents the "bulk" properties of an infinite system, the system in the current manuscript enters a different physical regime completely dominated by "edge effects". Therefore, it is physically unexpected for the numerical results of the two studies to be consistent with each other.
Answer
In contrast to the reviewers' interpretation, Afsar's articles [1,2] deal with finite chains. So we can therefor compare a shorter chain insteed of the N=100 atoms used by Afsar et.al. And why Afsar et al. should need an N=100 bulk, but not an N=7 bulk, if the boundaries are not taken into account? Or an N=1000 bulk? Who decides this question? This is a completely subjective, not objective, question or viewpoint.

In conclusion, due to this significant difference in approach, it is not logical to compare the results of Afsar et al. [1,2] with the current manuscript or to draw comments based on the differences between the results.
Answer
These conclusions of the reviewer are not correct considering the arguments that have been previously presented.

2) Another important reason why the manuscripts can-

not be compared stems from the method of obtaining the atomic configurations on the chain and the difference in the physical meanings of these configurations.

It is understood from the works of Afsar et al. that they use their equilibrium equations to numerically seek the point where the net force on each particle is zero. To provide an analogy, if we imagine the energy surface consisting of deep wells and peaks, in the authors' calculations, the system gets trapped at the "zero-force" point closest to the initial guess (conditions). A situation where particles fall into an energy trap (local minimum) represents static friction and metastable states here. In a configuration obtained with this approach, which depends on the initial guess, the particles are in equilibrium (total force is zero) but - with exceptions (metastable stable equilibrium points) - not in the state of lowest energy.

Answer

But what is the problem here? More specifically: what exactly is the critique to our comment? The reviewer is being a bit cryptic here.

In the current manuscript, it is observed that the authors seek the global minimum point on the energy surface by minimizing the system's energy (see Figure 1 and 2). This method attempts to minimize the total energy of the system. Consequently, the obtained configuration represents the most stable (total force is zero) and strictly the lowest energy configuration of the system.

Answer

Please note that we determine global and local minima as well as saddle points of any index even for the case N=7.

Furthermore, in the current manuscript, for example in Figure 4, the authors used a "Newton Trajectory (NT)" calculation method to find the configurations on the chain. This method also shows the intermediate steps obtained while scanning the path between two equilibrium states on the energy surface. At these intermediate points, there is a net force on the atoms (the gradient is not zero). However, by the definition of NT, this force is parallel to a chosen fixed direction. Therefore, as can be understood from Figure 4, in connection with Figure 3, while the total force on the atoms is zero only at the minimum, SP1, and SP3 points (saddle points), the configurations in the 2nd and 4th graphs from top to bottom are intermediate configurations, and the total force on the particles in these chains is non-zero. By nature, the NT method is not a scan of an "equilibrium pathway" but a "reaction path". For this reason, the fact that some configurations in Figure 4 are not stationary points makes it methodologically impossible to compare this study with other studies focused on "static equilibrium".

Answer

See please the answer above: The gradient is also a zero vector at saddles.

What is, or how is defined, an "equilibrium pathway"?

At this stage, I may need to remind the authors: In a real physical system (e.g., atoms on a metal surface), individual atoms do not have the "intelligence or energy" to find the global energy minimum. They simply settle at the first location where the forces acting upon them are balanced.

Answer

We think that this remark is whishfull thinking of the reviewer: The chain behaves as a whole unit in the general case, if one treats a minimum. If the forces are large enough than the global minimum is enforced. In the contrary case, local higher minima can be found, as can be seen in our Fig.2. Only so-called solitones can move locally trough the chain. But this is a different problem which we do not discuss here.

In conclusion, the reason for the difference between the authors' results and those of Afsar et al. is not "incorrect calculation" but rather the difference in what the methods used in these manuscripts seek within the energy landscape. Therefore, a comparison between the aforementioned manuscripts cannot be made.

Answer

From the arguments we have presented and those described in our manuscript, we cannot conclude what the reviewer has described.

3) It is seen from Eq.(2) in the current manuscript that the authors propose a new Hamiltonian for an atomic chain containing a limited number of atoms.

Answer

Note: The situation is the same as in the commented papers [1,2], in which N is a finite number. N is limited in Eq.(1) and in Eq.(2).

The analysis performed and the results obtained by the authors through this proposed Hamiltonian are original and can be evaluated as a separate study.

Answer

We appreciate the reviewer's generosity, his (or her) magnanimity. However, we reject the propose for a separate study. Our comment has its meaning to be a comment to the Afsar papers [1,2]. We firmly believe that it is a very well-founded commentary.

It is observed that the equilibrium equation the authors obtained from this Hamiltonian is:

See Eq.(1) of review 1.

The authors perform their calculations in the current manuscript using the Hamiltonian that satisfies this equation. In contrast, the equilibrium state equation studied by Afsar et al. (completely different from the equation proposed above) is derived from an FK-type Hamiltonian containing an infinite number of atoms, also including dissipative effects, and their force balance equations are written for all n values ($-\infty$, ..., n, ... $+\infty$) [1].

Answer
The step from Eq.(1) to Eq.(2) in our comment was not taken to avoid the absurdity of infinity; no, we are avoiding the nonsense of a side potential that also exorbitantly rises, just like the chain springs. See please our reasoning in the first section of the comment.

If the authors of the current manuscript were to write their force equation for this type of chain, it can be easily seen that a two-dimensional map equation of the form
See Eq.(2).of review 1
can be obtained. It is clearly evident that this map equation is a new map, distinct from the map equation studied by Afsar et al. [1,2]. Therefore, the results of the current manuscript cannot be compared with the works of Afsar et al. On the other hand, the analyses to be performed on this new map and the potential results to be obtained could be introduced to the literature as a completely original study.
Answer
The reviewer completely ignores the fact that we require the boundary equations for this map (or any other analogous map): that is our main argument, and the reviewer purposely does not understand it. No one in the scientific community has studied the boundary relations of an "infinite chain" (as this is not possible). This is our argument against all of these studies.

Upon reviewing the authors' current manuscript and their other cited works [15-19], I note that they emphasize their opposition to Aubry theory and, consequently, the concept of an infinite atomic chain. I advise the authors to reconsider how the concept of infinity is physically perceived by the n-th particle on the chain, which interacts only with its neighboring atoms. Furthermore, I would like to remind the authors that the pioneering papers of Aubry theory published between 1978 and 1983 have collectively received over 3500+ citations in the literature.
Answer: We have no choice but to quote the German writer Tucholski: Even 3500+ academic papers can be wrong.

Due to the aforementioned reasons, unfortunately I cannot recommend this manuscript for publication in Physica D in its present form. I strongly recommend the authors to revise their manuscript by removing these ungrounded criticisms/comparisons with refs. [1,2] and rewrite it focusing on the novel results of the original part in their manuscript.
Answer
The authors believe that this reviewer's conclusion is incorrect for all the reasons we have described and discussed above. We completely disagree with this reviewer. We reject his (or her) rejection. We ask the Editor to select another reviewer.

After submission of a third version of the comment (this one above) we get by this

Reviewer #1:
As I explained in detail in my first report, the calculation made in this article is different from the calculation made in the 2 articles criticized in the article. However, the authors still claim that they made the same calculation and criticize the 2 relevant articles. Unfortunately, as can be seen from their response to the referee report, they are unteachable on this issue.

In addition, for some unknown reason, they unfoundedly attack the Aubry method, which has become a standard in the literature for years.

In my opinion, it is not appropriate to publish an article that is both speculative (and therefore nonscientific) and confuses two different accounts in a high-quality journal such as Physica D.

For these reasons, unfortunately, I reject the manuscript.
Answer
A former high-quality journal (Physica D) now makes use of such 'reviewers' to reject a Comment.

Elsevier's offer:

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Note, the rejected paper was a special Comment for 2 papers in Physica D. Now they offer a publication against money:
This way of handling money does not align with the ethical conduct that every editor and publisher of scientific journals should uphold.

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Appendix 2

Appeal to Physica D: comment PHYSD-D-26-00561R2

Dear Prof. V. M. Pérez-García,
Editor-in-Chief
Physica D: Nonlinear Phenomena

We cannot accept the current decicion on our Comment PHYSD-D-26-00561R2.

Unfortunately, there was not added a name in the letters of the editor (only: em.physd.0.9d31e9.f49d72cc@editorialmanager.com). So, we try to send directly this appeal to You.

We appeal agains the decision because in the commented papers are mathematical errors which a first course student can understand:

One cannot give a single variable in an equation system an arbitrary value. The solution is that all variables must fulfill the system; this is the name system. This is exactly the error of the Afsar-papers, which we have commented, and it is also the error of the so-called Aubry theory which a reviewer defends vehemently.. We guess that You as a Mathematician will understand it.

We hope that You can look back in the process of the edition of the comment. And revise the decision. We hope for the scientific conscience of a journal with a long history.

Sincerely
Wolfgang Quapp,

Answer of Prof. V. M. Pérez-García,
Editor-in-Chief

Dear Prof. Quapp,

Thank you for your email.
Your appeal has been carefully considered. I have reviewed the evaluation process and the comments provided by the handling editor and the two reviewers. After this assessment, I see no grounds to overturn the editorial decision.
Therefore, I regret to inform you that we cannot offer publication of your Comment in Physica D.
Thank you for considering Physica D for your work. I wish you the best in finding a suitable venue for its publication.

Best wishes,

Víctor

Prof. Víctor M. Pérez García
Director, Mathematical Oncology Laboratory, University of Castilla-La Mancha, Spain.
Vice-president, European Society for Mathematical and Theoretical Biology
Coordinator, "Mathematics" panel, Spanish National Research Agency
Editor in Chief, "Physica D: Nonlinear phenomena"