Referee's report on revised version:
'On state space geometry of the Kuramoto-Sivashinsky flow in a periodic domain'
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resubmitted to *SIAM J. Appl. Dyn. Syst.*

The manuscript under review is a careful and detailed attempt to understand the dynamics and geometry of state space of a model chaotic partial differential equation, the Kuramoto-Sivashinsky (KS) equation. Unlike in most previous related studies which were restricted to odd solutions, the authors consider general periodic solutions and tackle the additional challenges introduced by continuous symmetry, especially the considerable increase in the diversity of allowed solutions.

The goal is to understand the dynamics of a particular (fixed L) KS system via extensive computation of its equilibria, relative equilibria, periodic and relative periodic orbits, and the heteroclinic connections between these; these computed solutions, though unstable, form (in the authors' words) a 'cage' for the full state space geometry. The domain size chosen, $L = 22$, which lies in the interval of smallest L-values for which the PDE displays chaotic behavior, is such that the chaos is still fairly low-dimensional, and does not yet display all the features of full spatiotemporal (extensive) chaos observed in larger domains (there is only one positive Lyapunov exponent); but as the study makes clear, the phase space is already quite complicated, with in particular a large number of relative periodic and pre-periodic orbits even of relatively low period ($T < 200$). The authors' hope and goal is to extend these geometric ideas to larger domains, and ultimately to fully spatiotemporally chaotic or turbulent regimes; this study makes progress in this direction but also gives an indication of the high complexity that might be expected...

I am pleased that the authors found my comments on the first version of this manuscript helpful; and I thank them for their very careful and detailed, point-by-point responses and explanations. I enjoyed reading the replies, and generally agree with their comments; and feel that the changes made have improved and clarified the manuscript, especially in the discussions of symmetries, visualization, and the extended Appendix C. I prefer the modified title (maybe: "On the state space..."), while the discussion of the use of the terms "chaos", "spatiotemporal chaos" and "turbulence" in the Introduction clarifies the authors' perspective on this tricky issue. (I also appreciated the blue-colored text, which made the modifications to the text much easier to identify!)

As the authors make clear in their response, the story is by no means yet complete, even for the $L = 22$ KS equation; they indicate that further details including more material on the relative periodic orbits and symmetry reduction will appear in a forthcoming paper, which I look forward to seeing.
However, the present work is, I feel, already a significant contribution to the understanding of the state space geometry of a high-dimensional system, and I am happy to recommend publication in the *SIAM Journal on Applied Dynamical Systems.*

I have included some minor comments, suggestions and corrections that the authors might wish to consider (I do not need to see the manuscript again):

- There is some inconsistency in usage and abbreviations. For instance, on *Page 1*, the same expression is written as (*line 7*) "∞-*d*", (*lines 17–18*) "infinite dimensional" and (*line 20*) "∞-dimensional" (similarly on other pages).
  For consistency with "low-dimensional" and "finite-dimensional", instead of one of the above three choices I would suggest using "infinite-dimensional."

- *Page 2, 3rd paragraph, line 2:* "its non-wandering set"
  Also, this sentence, the first sentence of the *3rd paragraph of page 2*, seems to overlap with the (newly added) *last paragraph of page 1.*

- *Page 2, line −6:* "…inspired by the investigation of Vanessa López et al. [38] of …"

- *Page 4, below (2.3):* "…replace the sum by an $m > 0$ sum." (?)

- *Page 7, below (2.19):* The periods of spatially periodic equilibria are surely L/n for integer n, not multiples of L?

- *Page 8, line 13:* Surely not *all* $|k| > \tilde{L}$ short wavelength perturbations are *strongly* contractive? (I would expect that, for instance, the mode with $k = \lfloor \tilde{L} \rfloor + 1$ is only weakly contractive.)

- *Pages 9 and 21:* Sections 3 and 8 have the same heading, "**Energy transfer rates**" — might one of these be changed?

- *Page 10, line below (3.2):* What is meant by "…evaluated on q equilibrium …"? Could this be "…evaluated on an equilibrium or relative equilibrium labelled by q"?

- *Page 11:* In equations (3.8) and (3.9), I think one would write u_x^2 and u_{xx}^2 (rather than " ${u_x}^2$ " and " ${u_{xx}}^2$ ").

- *Page 11, below (3.9):* "The time-averaged energy density $\bar{E}$ computed on a typical orbit is a constant" (? — of course the *computed* value "goes to" a constant; but the theoretical time average assumes infinite time).

- *Page 11, below (3.10):* Which part of Figure 8.1 is intended; should this be "…fall onto the diagonal in Figure 8.1(a), and so do …"?
  It is clear that the equilibria, relative equilibria, and averages on the (relative) periodic orbits should fall on the D-P diagonal, but not clear to me how $\bar{E}$ might be related (see Figure 8.1(b)); and indeed, while they are close, the averages do not fall on a straight line in Figure 8.1(b).

- *Page 11, 2 lines above Section 4:* "Michelson"

- *Page 11, last line:* "attracting periodic states" (not *attractive*?!)

- *Page 12, 2nd paragraph:* In the light of the discussion of the low-dimensionality of the KS dynamics, and the discussion, based on a computation of the leading Lyapunov exponents, that the chaos is mainly confined to a four-dimensional manifold, the following recently published paper (which also uses the KS equation as an example) may be relevant or of interest: H.-l. Yang, K.A. Takeuchi, F. Ginelli, H. Chaté and G. Radons, "Hyperbolicity and the effective dimension of spatially extended dissipative systems," *Phys. Rev. Lett.* **102**, 074102 (20 February 2009).

- *Page 12, lines −6,7,8:* Suggestion: "E_2 and E_3 essentially lie in the 2nd and 3rd Fourier component complex plane, with small deformations of the $k = 2j$ and $k = 3j$ harmonics, respectively." (?)

- *Page 13, last paragraph, and Table 5.2:* The first sentence is not quite accurate, as for the relative equilibria $TW_{\pm 1}$ and $TW_{\pm 2}$, only E is given in Table 5.2. (Can the quantities T, Λ_e and Λ_c be computed for the relative equilibria?)
  Similarly: Thank you for the explanation of how T was computed for E_3. The use of the

characteristic decay time scale makes sense to me, but the description on *page 13* implies that for all equilibria (and relative equilibria), T is computed as $T = 2\pi/\nu_e$. Could this description be clarified?

- *Page 14, below (5.1):* "... where δ takes a set of values ..."
- *Page 15, (5.2) and the line below:* The notation v_1 and v_2 for orthonormal *vectors* seems unfortunate, since (i) elsewhere, such as in (5.1), vectors have been denoted in **boldface**, specifically as $\mathbf{e}^{(j)}$; while (ii) in Figures 5.4-5.6 and 5.8, the notation v_1 and v_2 (and v_3) is used for the *scalar* coordinate axes (that is, the projections onto the three chosen orthonormal vectors). I suggest using boldface vectors also in (5.2).
- *Page 17, line 4:* Should this read "We adapt their arguments ..."?
- *Page 18, Fig. 5.7:* In Fig. 5.7(a), are the axes the same as in Fig. 5.6(a), just rotated? What are the axes used in the representation of Fig. 5.6(b)?
- *Page 18 and Fig. 5.9:* I was unable to find a reference (at least in Section 5) to Figure 5.9. In the author responses, this figure is described as a "global picture that links all these together ... — a cage that organizes the dynamics", and I agree; so might it be useful to include a brief description such as this, say at the end of Section 5, to introduce Fig. 5.9 and highlight its significance?
- *Page 19, caption of Fig. 5.9:* What exactly is meant by the vectors $\mathrm{E}_2 - \tau_{1/4}\mathrm{E}_2$ and $\mathrm{E}_3 - \tau_{1/2}\mathrm{E}_3$: just subtraction? (This is surprising mainly for being unlike any other projections performed in the manuscript, and is not discussed or motivated, especially the presence of $\tau_{1/2}\mathrm{E}_3$.)
  "... E_3 equilibrium to $\tau_{1/4}\mathrm{E}_2$ ..."
  "... have been omitted for clarity."
- *Page 21, Fig. 8.2:* It is rather tricky to identify the locations of the equilibria, especially of E_2, in these figures; could they be labelled explicitly as in Fig. 8.1?
  Also, there doesn't seem to be any discussion of Fig. 8.2 and its significance in the text (only a comment that such a projection can be misleading).
- *Pages 21–22, Section 8 and Figures 8.1–8.3:* Is it appropriate (in the light of the discussion regarding terminology and the changes made elsewhere in the manuscript) to refer to the $L = 22$ chaotic trajectories as 'turbulent' in this section and these figures? (I appreciate the authors' reasons for wishing to retain some use of the term 'turbulence' in the light of the goal of their overall research.)
- *Page 24, below (A.3):* $x_n = n \cdot 2\pi\tilde{L}/N$
- *Page 27, below (C.5):* in "... evaluated using equation for the vector field": which equation is meant (reference); or should it read "... the equation ..."?
- *Page 29, Ref. [3]:* The reference for the paper by Bronski and Gambill is: Nonlinearity, 19 (2006), pp. 2023–2039. (Also: "*and L_2 bounds ...*" in the title.)
- *Page 30, Ref. [28]:* "*... for the Kuramoto-Sivashinsky equation*"
- *Page 30, Ref. [42]:* The first author is R. E. LaQuey.

While overall the article is well-written, it would be improved by some careful proof-reading to take care of several minor errors of grammar or English usage; in particular, the definite article "the" is often missing when needed (and sometimes present when not needed...). I have not attempted to point out most of these language aspects in the list above.