This manuscript is a revised version of a similar manuscript by the first author which I reviewed some time ago. As a result, some, not all, of the points made in this review are borrowed from my first review.

The authors explore potential artifacts in the estimates of physical invariants which are obtained from the quantitative combination of time series data and dynamical models using sequential estimation procedures (Kalman filter and related smoother). Two physical systems are being studied: a system of three coupled oscillators and the wind-driven flow of a uniform-density fluid in a closed square basin. Emphasis is placed on the determination of "trends" of quantities of oceanographic/climatic relevance, such as mechanical energy and western boundary currents. It is emphasized, among other results, that (i) entry of data in the filter leads to violation of conservation laws while smoothing restores conservation rules; and (ii) a robust identification of trends in reanalysis estimates requires a complete understanding of both models and observations.

The determination of trends in oceanographic time series such as produced by basin-scale monitoring programs (e.g., RAPID and OSNAP) is a subject of growing interest. The study of trends in simplified physical systems such as reported here is essential for developing a better understanding of the challenges associated with the determination of trends in more realistic situations. The approach applied in the present manuscript is inspiring. Although the models employed are idealized descriptions, they permit a discussion of the some of the major issues, such as those associated with the nature of the data, their uncertainties, as well as their spatial and temporal distributions, which are likely to occur in more realistic situations. Overall, I think this manuscript should be a significant contribution, one that will hopefully invite researchers to interpret cautiously apparent trends or similar features in reanalysis estimates.

Reading through the manuscript, it appears that most, but not all, of the comments of my first review have been considered. Major comments are listed below, followed by a list of specific points. I hope these will help the authors to improve their very interesting work.

MAJOR COMMENTS

1) I am intrigued by the statement in the Abstract that a finite interval smoother restores conservation rules which are violated by the Kalman filter. Much like the Kalman filter (KF) estimates, Rauch-Tung-Striebel (RTS) smoother estimates are weighted least-squares estimates, with the weighting provided by data and model error covariances (see Bryson and Ho, 1975; in particular their Problem 3 on p. 395). As the authors are well aware, the basic difference between the KF estimates and the RTS estimates relies in the time span of the data being used. KF estimates are based on past and present data, whereas RTS estimates, which have the appearance of a correction to KF estimates (Eq. 19), are based on past, present, and future data. In fact, an optimum linear smoother such as the RTS smoother can be interpreted as a combination of two optimum linear filters, one which works forward over the data interval and the other which runs backward over the interval (Fraser and Potter 1969). From these considerations, I would expect the RTS smoother to share the same limitation as the KF filter, which is the introduction, when data are entered, of a violation of conservations laws as implemented in the dynamical model.

The above considerations, however, are purely qualitative and might be misleading. The statement in the Abstract is justified in the text by the fact that the system forcing q(t) is modified during smoothing (p. 17). While it is true that q(t) is modified during smoothing, it is unclear to me that the state evolution forced only by the modified q(t) and which would thus satisfy conservation laws is the same as the state evolution computed from smoothing. I would recommend that the authors illustrate in the manuscript a numerical example that would support their statement.

2) The manuscript reports results from a large number of filtering and smoothing experiments. To guide the reader through them, it would be useful to label the different experiments in the text, and to add a table where the design of the different experiments is described (the authors made a welcome move in this direction through their Table 1). For convenience, the first reported KF experiment for the coupled oscillators could be labelled as KF1-O-n, where n is a number or (perhaps better) an evocative character string. One table would list the experiments for the system of coupled oscillators, and another table would list the experiments for the wind-driven flow.

3) The choice $L = a$ (a = the Earth radius) to scale the potential vorticity equation (21) is not consistent with the beta-plane approximation. A choice of L consistent with the beta-plane approximation should be assumed (e.g., Pedlosky 1987).

4) I think both terms on the right-hand side of equation (29) (boundary-layer solution of Stommel model) must be divided by beta'.

5) The Discussion section is very short, and the manuscript lacks a Conclusion or Summary section. I would suggest to extend the text, so that an explicit concluding section appears at the end of the manuscript.

SPECIFIC POINTS

L. 5: "… oscillator system and …"

L. 12: "… the climate system as represented in a reanalysis. As a result, some simple …"

L. 21: "… As in geophysical fluid dynamics, two …"

L. 25: "… and models, and both …"

L. 61: "… transport of a western boundary current."

L. 99: "… mean error), respectively. The operator P(t,-) …". Also here and everywhere: I would suggest labeling P(t), P(t,-), P(t,+), and similar matrices, not as operators but as error covariance matrices or uncertainties.

L. 100: "… evolve …"

L. 103: "… both $y(t+\Delta t)$ and $E(t+\Delta t)$ vanish, …" (y is a vector while E is (generally) a matrix, so they could not be set to the same value).

L. 107: "… and $R(\tau)$."

L. 115: "3. Example 1: System of Mass-Spring Oscillators"

L. 116: "… system of mass-spring oscillators, following …"

L. 151: "Equation (1) is considered at …" (Eq. 1 is a difference, not differential equation, and so is already discretized in some sense).

L. 176: "…, data, etc."

L. 188: "Added white noise in the data …". Please make sure to use different symbols for the noise in the data and in the model equations.

Fig. 3, caption: "… Energy varies with the purely random process $\varepsilon(t)$ as well as …"

L. 205: Please elaborate.

Figure 4: the ordinate of the bottom panel is "velocity", not "displacement". In the caption, perhaps write "… (c) Estimated velocity of the first …"

L. 264-265: "… observations (see Appendix A; Eq. A3). Suppose …"

L. 305: "… horizontal dimension L. Here $\beta = df/dy$ is the variation of the Coriolis parameter, f, with the latitude coordinate, y. This problem …".

L. 313: Drop " $q = q_0 q'$ " and write " $\psi = (a^2 f)\, \psi'$ "

L. 333: "…the coefficients c_p do not strictly …"

L. 336: "… by addition to ψ of …"

L. 339: "… Rayleigh friction coefficient."

L. 343: "… wind-stress curl."

L. 347-348, "… diagonal elements diag(…) … NM+1.": Please rephrase.

L. 350: "… M diagonal elements equal to …"

L. 366: "… generate the true fields."

L. 368: "… the random forcing q_j has …"

L. 377: "… is more than …"

L. 385: "… Observations cease posterior to T_F, …"

Fig. 11, caption: "… (c) Difference between the true values and the KF values (solid line), and difference between the true values and the RTS smoother values (dashed line). The vertical lines show …"

L. 408, "… briefly exceeds the true energy prior to the appearance of the first observations": I do not see this in figure 11.

L. 410, "… Estimated unknown elements u(t) …": Please remind the reader what u(t) represents.

L. 422: "… (b) the true time-dependent value …"

L. 424: do you mean "… full solution of Eq. (28)"?

Fig. 13, caption, "… slow increase towards a higher value when no data are available.": This is not apparent in figure 13b.

L. 435: "A test of the null hypothesis that …"

L. 444: "… smoother (see Fukumori et al. 2018)."

Fig. 15, caption: "… equation), (b) …"

L. 482, "… observations are not independent of the state vector …": Please drop or rephrase.

L. 490: "… Wunsch and Heimbach (2007)), a key …"

Eq. (A1): "… = $A(t-\Delta t)\ P(t-\Delta t)\ A(t-\Delta t)^T$ + …"

L. 501-502: I do not see the rationale for introducing the two distribution matrices, Γ and B. It seems to me that Γ and B are always the same matrix (e.g., Bryson and Ho 1975).

L. 517: "Appendix B: …"

L. 520: "… innovation vector,"

L. 523, "innovation equation": Please identify or report this equation in the manuscript.

L. 551-552, "… depend upon … and with many observations including those …": Please rephrase.