Nonlin. Processes Geophys. Discuss.,
doi:10.5194/npg-2016-81-RC2, 2017

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on "A Simple Kinematic Model for the Lagrangian Description of Relevant Nonlinear Processes in the Stratospheric Polar Vortex" *by* Victor José García-Garrido et al.

Anonymous Referee #2

Received and published: 13 January 2017

General comments:

This paper addresses the issue of Lagrangian transport in the Stratospheric Polar Vortex (SPV). The first part of the paper analyzes SPV data from the ECMWF using the technique of Lagrangian Descriptors (LDs, developed over the years by some of the authors of this paper and their collaborators) for a specific time period in September 2002. A three-mode kinematic model which possesses the gross characteristics of the data is then developed, and there is some discussion on how it is possible by adjusting its parameters to mimic certain behaviors of the observational data. The paper is well-written and readable. However, I believe that some more work is needed to show that LDs are *relevant* to this situation, and that the kinematic model provides useful informa-

Printer-friendly version

Discussion paper

tion. I have expanded on this in my ‘specific comments’ below. My feeling is therefore that a major revision would be required before being acceptable for publication.

Specific comments:

1. It seems that the major focus is on modeling the SPV breakdown in September 2002. If trying to use Figure 4 as evidence that LDs provides an excellent way to explain this, then I feel that there must be some comparison to other studies which show this. Beyond a few brief references (page 2, line 27-28), the authors do not seem to do much in this direction. After all, how good are the results of Figure 4? What are the other symptoms of the SPV breakdown—what other observations showed that this indeed did break down? (Using Figure 3 is a start—but this is using an Eulerian observation to predict something Lagrangian—or is it?) And is Figure 4 consistent with any other observations? Several references which might help are: Nishii & Nakamura (*Geophys. Res. Lett.*, 2004), Kruger et al (*J. Atmos. Sci.*, 2005), Taguchi (*J. Atmos. Sci.*, 2014), Fisher et al (*Atmos. Chem. Phys.*, 2008), Esler & Scott (*J. Atmos. Sci.*, 2005), Konopka et al (*J. Atmos. Sci.*, 2005), Varotsos (*Environ. Sci. Pollution Res.*, 2002, 2003, 2004) and Allen et al (*Geophys. Res. Lett.*, 2003). In addition to these, I feel that it is imperative that there be comparisons (or relevant discussions) with the paper by Santitissadeekorn et al (*Phys. Rev. E*, 2010) which provides a Lagrangian analysis and provides pictures very similar to Figure 4.

2. The term ‘Hyperbolic Trajectories’ (HTs) is used often in this paper, and described briefly in the introduction. The ideas and intuition given in the third paragraph of the introduction are however only valid in *infinite-time* flows. There are sometimes additional limitations of steadines: the cat’s-eyes structures in these models depends on drawing streamfunction contours (either in the steady frame or in a moving frame), and so are associated with steady situations. While the remainder of the discussion does not necessarily confine itself to steadiness, as far as I

am aware, hyperbolic trajectories can only unambiguously be defined for infinite-time situations, using the ideas of exponential dichotomies. The paper by Ide et al (*Nonlin. Proc. Geophys.*, 2002), for example, cites the exponential dichotomy definition—but this cannot be adequate for finite-time flows since the variational equation associated with *any* trajectory will obey the exponential decay requirement by choosing a suitably large prefactor. There have been attempts to fix this: by choosing a prefactor of 1 (Doan et al (*J. Differential Equations*, 2012), Karrasch (*J. Differential Equations*, 2013), Duc & Seigmind (*Int. J. Bifurc. Chaos*, 2008)), or by extending to infinite-times in some fashion (Balasuriya, (*J. Nonlin. Sci.*, 2016)). In general, it seems that HTs are ill-defined for finite-time flows. Throughout the paper, however, the authors seem to be using 'saddle-like locations of the LD field' as their method of identifying HTs. I understand why such locations can be called 'hyperbolic,' but there does not seem to be any justification in calling them 'trajectories' since it is not at all clear if by following these in a time-varying way by computing LDs over a range of t_0 values, an actual *trajectory* of the system (5) arises. If the flow is nearly steady, it seems that it might be possible to establish the existence of time-varying saddle-points which are *close to* an actual (infinite-time) hyperbolic trajectory in some instances (Ide et al (*Nonlin. Proc. Geophys.*, 2002), Balasuriya, (*J. Nonlin. Sci.*, 2016)). But is this necessarily so for this situation, viz. using finite-time data, with moderate unsteadiness, and specifically using LD fields to identify saddle points? If the actual *term* 'hyperbolic trajectories' is not important to what the authors are doing, then perhaps they should simply call them saddle points of the LD field? But even so, claiming a direct relationship to stable and unstable manifolds—which are undefined for finite-time flows—seems problematic.

3. I have some concern about the centered nature of the definition for M in (6). If requiring to find information on the 'skeleton of transport' at time t_0 using FTLEs/FSLEs/.../variational LCSs, the basic approach is to seed initial values at

t_0. If looking for the analog of the stable manifold at t_0 (i.e., repelling LCSs, ridges of forward-time FTLEs), these needs to be advected in forward time. Similarly, the advection is in backward time if looking for analogs of the unstable manifold. *It is this information which tells us about the skeleton at time t_0.* For example, Gaultier et al (*J. Marine Sci.*, 2013; *J. Geophys. Res. Oceans*, 2014) do this advection in backwards time in order to compare with sea-surface temperature fields at the time t_0. This is also because the advected scalar field (temperature in their case, whereas in this case it could be temperature, ozone concentration, etc, depending on the specific observable of interest in the SPV) at time t_0 would depend on the advection occurring *until the time* t_0. Future times surely cannot have an impact. Therefore, why is the integral in (6) being taken from times $t_0 - \tau$ to $t_0 + \tau$? This seems inconsistent with all other Lagrangian approaches. Moreover, it's hard to argue that the SPV knows the future! The pinch-off on September 24 in Figure 4(b), for example, uses velocity data into October.

4. The authors state that 'M reveal[s]/highlights Lagrangian coherent structures' (page 5, lines 12 and 15). Is there a rigorous justification for this—that M specifically reveals *coherent* structures which move in a Lagrangian way according to the flow? If so, in what way? I am not able to find it directly in the cited references, though I am unable to get access to the latest article (Loposito et al, 2017) that is still in press. To my knowledge and judgment, a relationship has only been established in heuristic senses (and this is also so for other Lagrangian methods used and advocated by others), and in incredibly simplified test cases. Moreover, the authors talk of 'stable and unstable manifolds' here, but of course these things do not have a proper definition in finite-time flows. I believe that the description here needs to be watered down. The LD field is being used as a *heuristic*, and there is *some* evidence that it provides the right understanding.

5. The kinematic model requires more justification. Why do the amplitudes of the Fourier modes in the kinematic model have these particular r-dependencies?

The $r(r-a)$ in v_r is understandable, but why e^{-r}? And why the specific forms chosen for Φ_1 and Φ_2? And why these particular forms of time-dependencies for ϵ_1 and ϵ_2? Certain parameter values are used in the simulations—why were these chosen? In what way are they consistent with parameter values of the SPV? Since the flow for the kinematic model is unsteady, the pictures of Figure 6 must be drawn at a particular time value t_0, I guess. What is it? I also have a much more general question regarding the kinematical model: What particular understanding does it give to this situation? It is probably possible to have the LD field display all sorts of crazy behavior by choosing the Φs in various ways, and so what does this particular model do? Now, if it was possible to argue, for example, that a particular instability arising from this kinematic model led to the SPV breakdown, then that might be interesting.

6. I am confused by what the authors are trying to achieve in Section 5. Are they trying to say (page 15, line 11) that their kinematic model can be made dynamically-consistent but inserting their Φ into (14) and (15) but then treating h as unknown, and thereby getting an expression for h? This can possibly be done (though h will satisfy a PDE which may not be easy to solve), but this is highly artificial. This would be demanding that the topography adjusts to the kinematic model that we insist is a solution. One possibility in which this part of the paper might have value is if the Φs in the kinematic model were somehow chosen as modes associated with the conservation equation (14)—this would be similar to the work of Pierrehumbert (*Geophys. Astrophys. Fluid Dyn.*,1991). The discussion of the earlier parts of this section also appears to lack relevance. If Q were constant in patches, then complicated dynamics are possible subject to Q's conservation—but this simply amounts to nullifying the dynamical constraint, and adds the extra condition (not talked about here) that the streamfunction needs to be chosen such that (15), for a constant Q, is satisfied. Basically, it is true that the potential vorticity distribution imposes constraints on the Lagrangian motion, which may

Printer-friendly version

Discussion paper

be an aspect the authors are trying to highlight here. For these, the papers by Brown & Samelson (*Phys. Fluids*, 1994) and Balasuriya (*Nonlin. Proc. Geophys.*, 2001), which deal with both constant and nonconstant Q, may be relevant. In general, I'm not sure I understand the goals this section, and so it requires some attention.

Technical corrections:

1. Some capitalization is missing in the references, for example in words like Rossby and Lagrangian.
2. Page 3, line 17: ERA needs to be capitalized, consistently with previous lines (e.g., line 12).
3. Page 3, line 20: space between 'fields' and 'on.'
4. Page 5, line 4: the citation to de la Cámara et al should not be within parentheses.

Interactive comment on Nonlin. Processes Geophys. Discuss., doi:10.5194/npg-2016-81, 2016.