Review of "Integration-based Extraction and Visualization of Jet Stream Cores", by Bösiger et al.
(Review by Gloria Manney)

Recommendation: Publish after some clarifications and minor revisions.

General Comments: This paper presents a new method and software for tracking and visualizing jet stream cores. This is a potentially very useful new method with some important advantages and should be a valuable addition to existing tools for jet characterization and analysis. As such, it should be appropriate for publication in GMD and I would expect there to be much interest in it among atmospheric scientists who focus on studies of the jet stream (including myself!). However, I feel there are some important changes to the presentation needed to (1) better reflect previous work on jet stream characterization and the phenomena (WCBs, tropopause structure) the jet streams are related to here, and (2) to make the paper more accessible to an audience of atmospheric scientists for whom this method / software may be very useful but who may not in general be computer scientists or mathematicians. (I believe I'm a reasonable example of this class of atmospheric scientist, so that if I don't understand some things it is not unlikely that many other interested readers will be in the same position.) These changes are summarized here (with some further specific examples given in the "specific/minor comments" below):

(1) A few terms and some notation are used throughout this paper that are not (clearly) defined or are things many in your audience may not be familiar with, and should be defined and/or expressed in plainer language:
    (a) Voxel -- should just be defined the first time it is used as it will be unfamiliar to many readers (as I understand it from looking up the definition, it is nothing more than the 3-D analog of a pixel).
    (b) Heuristic (heuristics, heuristically) -- in general (and in many fields) this term is often (perhaps over) used and frequently not clearly defined (thus sometimes mis-used). Indeed, dictionary definitions are many and varied. My impression is that the way you use it here is something akin to "pertaining to a trial-and-error method of problem solving used when an algorithmic approach is impractical", or to simply say that the process in question requires human intervention (e.g., the necessity to make choices based on things the human eye does very well but we tend to have trouble telling computers how to do). A more specific statement (and perhaps examples from some of the previous studies you cite) of what you mean by "heuristic" would be very helpful in motivating the development and advantages of your method.
    (c) Manifolds -- I question the need to use this term (which readers unschooled in topology may not recognize or may immediately assume is expressing some complicated concept) when the fundamental information conveyed in this context by "instantaneous 1-manifolds" is that it is a line/curve (1-dimensional) at a particular time, and by "time-dependent 2-manifold" that it is a time-varying surface (2-dimensional).

(d) Several definitions (tropopause, WCBs, and “filtered” WCBs) used are expressed in set-builder notation (which many readers may not be familiar with); in general, you explain these (though not always completely) in words beforehand, but it isn’t always obvious that that is what you are doing. I’d suggest that you make this relationship explicit, by saying something like (e.g., for the tropopause case): “...largest connected surface of 2 PVU (-2 PVU) in the northern hemisphere (southern hemisphere) at pressures below 740 hPa, that is: <insert equation 6>” (in fact, since this is a very common way to define the tropopause, any atmospheric scientist interested in your work will immediately understand this without the equation, so it is not obvious that you need the equation at all -- however, I have no problem with including it as long as it is also explained in plain words so that the reader will understand regardless of whether they are familiar with the notation.

(2) The citation of previous literature is lacking, especially with regard to upper tropospheric jet streams and characterisation thereof. While I understand that the focus here is on the software tools developed, these are being presented specifically in relation to jets in the Earth’s atmosphere and thus the primary audience is atmospheric scientists -- hence it is important to accurately relate this work to previous work in the field and to the reasons why the method may facilitate future work. In particular:

(a) Section 2.2: There are much better references than Dameris (2015) for what the tropopause is and why it is important; in addition, Dameris (2015) is not readily publicly available. I would start with the reviews by Holton et al (1995, Rev Geophys) and Stohl et al (2003, JGR). In addition to Skerlak et al (2015), I would add a couple of classic papers for tropopause structure such as (cited by Skerlak et al) Danielsen (1968) and Shapiro (1980) -- or at least add “and references therein”. Highwood et al (2000, QJRMS), Schoeberl (2004, JGR), and Kunz et al (2011, JGR) are good references for the range of PV values that have been used for dynamical tropopause identification and for what regions / purposes different values are appropriate.

(b) Section 2.3: This is a very incomplete and biased discussion of upper tropospheric jets and previous work characterizing them.

(i) Ahrens and Henson (2018) is not readily publicly available, and there are numerous choices for classic work describing the jet streams and their importance to the atmospheric circulation. Koch et al (2006) and Schiemann et al (2009) (already cited in this preprint) both give concise historical introductions. Harnik et al. (2016) provide a nice brief review in relation to jet regimes and extreme weather events.. Manney et al (2014, J Clim) and Manney and Hegglin (2018, J Clim) have in their introductions comprehensive discussions of the literature in the context of the importance of and variations in jet streams, which provides many of those classic references.

(ii) Several methods for identifying and characterizing jet streams that seem very relevant to this work are not mentioned, including the method introduced by Manney et al (2011, ACP) and used in Manney et al (2014, 2021, J Clim) and Manney and Hegglin (2018) (with more physically-based distinctions of the subtropical and polar jets in the latter two of those papers); the method used by Winters et al (2020, MWR, and several references therein); and that of Maher et al (2020, Clim Dyn). Should also cite Spensberger & Spengler (2020, J Clim) in addition to Spensberger et al (2017) and note that their method does the characterization on the dynamical tropopause.

(iii) There are many jet characterization methods / studies in which the "assumption that the flow is oriented eastwards" is not made (and others where it is only used after the fact), including Manney et al (2011 ACP,) (method also used by Manney et al (2014) and refined for Manney and Hegglin (2018) and Manney et al (2021).) In addition several methods (including that of Manney et al, above references) do characterize the jet position/extent in the vertical as opposed at a level or in a layer.

(iv) The statement that the jets "can be further classified into different types based on their location" is vastly oversimplified, and, given the usage of these terms later in the paper, the physical distinction between subtropical and polar jets should be discussed accurately here, as well as the fact that there is indeed a spectrum of jets that may have characteristics that are a hybrid between the two (see Lee & Kim, 2003, JAS; Manney et al, 2014, 2021; Winters et al, 2020; and references therein). Peña-Ortiz et al (2013), in fact, noted that attempting to distinguish polar and subtropical jets by latitude was commonly unsuccessful; Manney et al (2011, 2014) noted that using a simple latitude criterion was only useful for very broad climatological studies, and Manney and Hegglin (2018) introduced a more physically-based method of distinguishing subtropical and polar jets based on tropopause height changes across the jet region. Winters et al (2020, and references therein) distinguish subtropical and polar jets by identifying them in different isentropic layers, and show clear instances of them merging to form a jet with hybrid characteristics.

(c) I am not as familiar with the literature on WCBs, but the discussion strikes me as often making general statements without giving citations (some instances noted below in the specific comments).

(3) General Questions (those with ** at the beginning are more further information / general interest questions, rather than necessary changes to this manuscript):

(a) Rationale for choices of thresholds/definitions, and discussion of sensitivity to those thresholds/definitions (there is a statement on line 279 that "thresholds used in the definitions have been chosen based on common practices in atmospheric science", but you need to give references and briefly note the physical basis for that "common practice", including:

(i) 40 m/s for the minimum windspeed for the seed points. (Some information on the sensitivity of the performance of the software to this is given, but no rationale is given for the default choice, nor is the sensitivity of the physical results to this discussed.)

(ii) 190 to 350 hPa for the domain for jet extraction. 190 hPa is not low enough to exclude the stratospheric “subvortex” jet, which commonly extends down to between 150 and 250 hPa (eg, 340K), especially in the SH late winter and spring (e.g., Manney et al., 2014). Manney et al (2014, their Fig. 6) fairly commonly identified jet cores of over 40m/s as low as about 5 km, near or at their high pressure search boundary of 400 hPa, as well as jet cores (distinct from the stratospheric subvortex jet, which they characterized separately) near 13--14 km (typically for subtropical jets at latitudes equatorward of about 30 degrees), very near or at their low pressure search boundary of 100hPa; as they noted (and identified), the stratospheric subvortex jet often overlaps considerably in altitude with the upper tropospheric jets.

(iii) 2 PVU for the dynamical tropopause (many other values are used, and higher values of 3 to 5 PVU have often been recommended for mid to high latitude features, eg, Highwood et al, 2000, QJRMS; Schoeberl, 2004, JGR; Kunz et al, 2011, JGR), and 740 hPa for the maximum pressure (e.g., intense sub-synoptic scale events can be associated with the tropopause dropping well above this pressure, eg, Lillo et al, 2021, JAS, and references therein). Also, if the domain studied extends into the tropics, how is the tropopause computed there since PV goes to zero (the most common procedure is to use an isentropic surface, commonly 380K, wherever the magnitude of the PV is less than the threshold above this isentropic level, eg, Schoeberl, 2004; Manney et al, 2011; and references therein)?

(iv) Thresholds for proximity of WCBs to jet coreline.

(b) I find the discussion overall somewhat unclear in the usage of “steps” -- there are spatial steps (eg, the grid spacing used for the prediction step), time steps, and procedural steps (eg, prediction and correction steps) and it is not always clear from the context which is being discussed.

(c) The representation of the corelines (which, as I understand it, are simply that, that is lines approximately connecting the core locations) as tubes, with wider tubes for higher windspeeds has the potential to confuse the reader into believing they show the jet region (analogous to the “regions” discussed in Koch et al, 2006 and Manney et al, 2011). While there will be some information since regions with higher windspeeds will have windspeeds above the threshold(s) over a larger area, there is by no means a direct correspondence since the wind gradients are not uniform or symmetrical around the core. The text needs to be very clear about this point, so as to not mislead the reader into thinking they are seeing the physical region where a jet is defined.

(d) While the terms subtropical and polar jets are tossed around in the paper there is apparently no attempt to distinguish these in a physically meaningful way. Thus statements suggesting that a jet coreline represents a subtropical or polar jet should not be made. **Also, it would be interesting to know if there are plans to add such a distinction to the method.

(e) Use of pressure rather than potential temperature for the vertical coordinate: Why was the pressure coordinate chosen for the jet extraction? Given that an isentropic coordinate would be more “flow-following” on short (days to a week or two) timescales, would one expect substantial differences if the procedure were implemented in an isentropic coordinate? **Would it be feasible to implement it in isentropic coordinates?

(f) **I would be interested in some more discussion (perhaps largely in an appendix or in the supplemental material) on the performance. The description given is all per time step (and it is not entirely clear what the time step being referred to is). Your study period is two months. What is the total time to process that period? The description of the procedure sounds storage-intensive -- what is the total storage needed for output for your study period? What do the performance results imply about the feasibility of using this procedure for climatological studies? From all of this, can you say something about the system requirements (CPUs/speed, memory, cache, storage) for running this effectively?

(g) **I would also be interested (again, in an appendix or supplementary material) in more specifics about the algorithms used for various steps. I think such information could be very helpful to the reader who might want to implement something similar to parts of this but is not conversant with C++.

Specific / Minor Comments (in order of appearance):

Line 22, please provide (a) more accessible and foundational reference(s) per general comment (2).

Line 28--29, why is the tropopause expected to show highly 3-d structures around split and merge events? Please give references for this.

Line 76--80, would be good to note somewhere in here that the tropopause altitude is generally highest in the tropics, lowest near the poles, and drops sharply across the subtropical jet. It not uncommonly extends below 6km in folds or other tropopause depressions (see general comment (2a)).

Line 105, this is presumably a right-handed coordinate system, and v is defined as positive if northward?

Lines 108--112 (through eq. 3), this sentence / equation aren’t very clear. I’m guessing that the text is supposed to be a description of the following equation, but I don’t know what the || means in this context (where it looks like an operator or something stating a relationship) nor whether the equation is supposed to represent the rephrasing of the problem or something related to the solver. Please re-word.

Figure 1 caption, please clarify that the date/time at the end of the caption is that shown in the figure.

Lines 121-122, some general reference(s) should be given for cyclones.

Line 192, please explain what is meant by easing “the balancing between prediction and corrections steps” and why normalization accomplishes this.

Line 214, and Figure 2 caption. If the weak endings are removed, why are there still green segments in Fig. 2(d)?

Line 292, what is the method for integrating dx(t)/dt for the trajectories?

Lines 316-317 & 331, I don’t know what you mean by “transfer functions”, please explain or reword (since it sounds like you are just saying both the radius and color are dependent on magnitude, you could simply say that). It would be helpful to have some sort of a key for the radius on the plots; if the radius relationship is also linear (as the color one appears to be) you might make the color bar a wedge rather than a rectangle. If the radius change is not linear with windspeed, you need to say that. Related to this, and Figs. 10, 11, and 12, it needs to be explicitly stated that the radius does not show the region wherein a jet is defined, per general comment (3c).

Line 318, how did you determine that it was a stratospheric jet?

Line 326, why not use a north polar orthographic or a north polar Lambert equal area projection? These emphasize the mid to high latitude regions more than the stereographic.

Figures 2, 8, 9 10, and 12, the color bars are too small. Also, the choice of a diverging color palette for the windspeed in these and other figures seems a poor one, since it is a positive definite quantity for which it does not appear that there is a reason to emphasize a transition at one particular value -- a perceptually uniform sequential palette would be preferable.

Lines 340-343, per general comment (3d), you have not done anything to identify polar vs subtropical jets, which have different primary driving mechanisms and thus different characteristics (e.g., Lee & Kim, 2003; Manney et al.,2014; Winters & Martin, 2020; and references therein). There are not “generally” two jets, in fact the patterns of jets and how many there are (with one to three being most common, but more possible at a given time/longitude) vary strongly with region and season (e.g., Manney et al., 2014, 2021, especially see Fig. 1 in the latter). If you are going to use the terms, you need to provide some justification for referring to a particular jet as polar or subtropical since there are important physical distinctions between the two (and of course, some jets may have hybrid characteristics between the two).

Line 355--363, it would also be good to cite Winters & Martin (2020) and Maher et al (2020) (and references therein) here, since the methods they use (unlike Koch's) rely on those strong PV gradients. There is a large body of work (much of it cited in these recent papers; also see Manney et al, 2014, and references therein) showing that extratropical westerly jet cores lie near/at the dynamical tropopause in the region where there are rapid altitude decreases in the tropopause altitude with increasing latitude, thus the "expectation" of it lying on the flanks of valleys in the tropopause, and of tropopause folds "wrapping" along the flank of a jet are well-known results, as is the complex 3D structure of the tropopause. Per general comment (2b), there are many papers (including those cited previously in this review) that characterize the jet structure in both the horizontal and vertical, so the largely 2D views described in lines 362--363 are by no means "typical" and have not been for on the order of the last decade.

Lines 366--367, can you say anything about how the visualization (which, though informative and interesting, is qualitative) will help shed light on mechanisms.

Line 371, why 270 hPa? Also, why on an isobaric rather than an isentropic surface?

Lines 372 & 378, is this really entirely an effect of the WCB on the jet? That is, is there no effect of changes in the jet on the WCB? How do you know which is causing which to change?

Lines 373--375, some references for these effects are needed.

Lines 385--386, it would be appropriate to cite Manney et al (2014, 2021), Homeyer & Bowman (2013, JAS), Winters & Martin (2020), Spensberger & Spengler (2020), and references therein here, per general comment (2b).

Lines 404--407, please provide some references for these statements.

Line 408, "directly linked to the jet coreline", "WCB outflows influence the jet corelines" -- it isn't obvious to me how these methods may accomplish this, unless combined with some dynamical analysis that suggests the causality.

Figure 13, I think the lower panel of this figure would be seriously compromised if viewed in grey scale (you would not be able to distinguish the "coolwarm" type color palette from the grey scale tropopause surface. You might thus want to think about changes to the presentation here.

Lines 414--415, Per general comment (2b), there is already a vast body of research on these topics, including the few papers I've mentioned here along with many others, covering topics such as relationships of jets and tropopauses to storm tracks, extreme weather events, etc. Jet regimes of various sorts have been defined based on characteristics of the jet stream, see general point (2b). While I think the methods in this paper can be a very valuable addition to the existing tools and literature aimed at more fully characterising the jets as dominant features influencing the tropospheric circulation, it is disingenuous to state these solely as future aims.

Lines 419--420, I’m not sure what you mean by a “non-incremental” search, perhaps you can explain this briefly.

Typos / Grammar / Minor Wording / Etc:

Line 16, “is” should be “are” (“data” is plural).

Line 17, “time-dependent” should be followed by a comma.

Line 17, “This data is” -> “These data are”.

Line 66, I don’t think “package” is the best word here; I would just say something like “rotation of the air enclosed between two…” (in fact if there are diabatic motions, it is not “trapped” in any sense).

Line 84, I would hardly call a paper published in 2001 “recent”.

Lines 260 and 262, “is” should be “are”.

Line 350, “he jet streaks” should be “the jet streaks”.

Line 379, by “attained considerable focus” do you mean it is a topic currently under investigation? If so, just say that.

Line 421, “Dur” should be “During”

References not already cited:
(Apologies for non-uniform format, these are pasted from most convenient sources.)

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