Review of WCD-2021-24: Stratospheric intrusion depth and its effect of surface cyclogenesis: An idealize PV inversion experiment

Overview: This manuscript sought to explore the role of Southern Hemisphere stratospheric intrusions (lowering of the stratosphere into the troposphere, realized as a lowering of the dynamic tropopause) on tropospheric circulations. The authors used a simplified atmospheric state to compute a variety of PV inversion experiments examining the relative impacts of a deeper intrusion, an intrusion from different tropopause heights, a combination of the two, a stronger intrusion, and a wider intrusion. The results are generally as expected from existing PV thinking – the closer the cyclonic PV anomaly to the surface, the stronger the influence at the surface. Some interesting results were either affirmed or introduced (in particular regarding the intrusions from a higher dynamic tropopause, and the role of wider/narrower PV anomalies), but in general the results re-affirmed known relationships that are applicable to either hemisphere. I hesitate to say that the article isn't introducing anything new – often these relationships are taken for granted through theoretical arguments but aren't shown through simple modelling exercises – but little about the results seemed new. Perhaps more importantly, the connections drawn by the authors to both cyclogenesis (rather than just a cyclonic response to a PV anomaly) as well as to real-world scenarios were not robust enough to make it clear that this presented a substantial contribution to the science. There were also concerns about the model set-up in the first place, which appeared simplified to the point of violating key laws of the mid-latitude dynamics that appeared to be the focus of the manuscript. Lastly, the manuscript was written too colloquially and lacked focus in many places. I do believe that with a careful and attentive revision the paper will represent a good contribution to the field, and as such I recommend a major revision and re-review.

Major Comments:

1. *Model and Experiment Set-up:* The study seeks to study, and compare to, mid-latitude dynamic processes (a prescribed latitude of 43°S) that occur in a baroclinic environment, but the model itself is run as a barotropic model. The authors need to clearly justify their model set-up and how to interpret their results appropriately. In the Barnes et al. 2021a paper that they base the climatology off of, the authors made clear that the COLs of interest occur in a baroclinic environment (which is required for the jet to occur under thermal wind balance). The reviewer recognizes that idealized barotropic models are an important tool in trying to diagnose these questions, but the authors need to make a much more clear and stronger justification for doing so.

We thank he reviewer for this suggestion of adding in more justification for our use of experimental design. The idea behind this choice of barotropic model was made in order to strip the environment of any other forcings and isolate the PV intrusion itself. We would like to test PV intrusions in a baroclinic environment as well but it is felt that this could be for further study which include temporal aspects and additional advection aspects as well. We have added this aspect into the manuscript

Regarding the experiment set-up, the authors also to more clearly justify their decisions for the spatial extent and intensity of the PV anomalies. There are many studies out there that have studied PV anomalies, so a more clear justification for why they determined a PV anomaly to 'look' the way it did is necessary.
Of particular focus needs to be a clear justification of the horizontal and vertical extent, as well as a justification for a sudden and total relaxation of the PV gradient in the anomaly itself once the -1.5 PVU threshold is met (eg. Fig. 4).

The relaxation of the PV gradient within the anomaly itself was chosen for ease of controlling the magnitude of the PV as shown in the resulting PV intrusion. In these experiments we use the

PV anomaly to attempt to recreate PV intrusions as closely as possible to as would be seen in reality ie. A tongue of high magnitude PV extending from the stratospheric pool of high PV. Within the numerical framework used. Restricting the gradient within the anomaly was the most effective way at ensuring a tongue of -1.5 PVU resulted. We do agree that there would be a PV gradient in a real-world PV intrusion. In Experiment 4, the magnitude test looks to test this out by adding additional PV values within the 1.5 PVU and shows that it results in little effect on the cyclogenetic forcing induced. We have added this argument into the manuscript during our discussion of the anomaly in the experimental setup.
"It is acknowledged that within real-world PV intrusions, there would be a PV gradient within the PV intrusion lower than PV intrusion boundary is defined. However, within the experimental framework the interior of the PV intrusion is kept constant to more easily control the magnitude of the PV intrusion that results from the PV anomaly."

2. *Conflation of the idea of cyclogenesis and a cyclonic response:* The authors routinely refer to a cyclogenesis term (based on thresholds of cyclonic vorticity – lines 213-215) but are looking at the response at the surface given the existence of a PV anomaly at the tropopause. In other words, by using a PV inversion (rather than integrating a model forward where a PV anomaly is introduced, and the surface is allowed to evolve in response), you are not looking at cyclogenesis, but instead the existence of a cyclonic circulation due to the existence of a PV anomaly. This undermines several components of the results, both when the authors discuss the surface circulation due to the PV anomaly as well as when they make points on the potential evolution of the anomaly were it allowed to evolve in time. Further, their discussion of these points leave concern about a lack an understanding of PV dynamics. If the PV anomaly and surface cyclone are vertically stacked (as they are in the results), and this is a dry barotropic environment, the vorticity anomalies are the only factor at hand that can influence the system, meaning only movement is allowed rather than intensification. Thus, the surface cyclone cannot undergo further development from an intensification standpoint, and the same holds for the upper level cyclone – thus leaving the question of how the system can ever 'develop its own closed, cyclonic circulation (or COL)' (line 303). A very careful examination and reworking of this discussion is critical for the interpretation of the results.

The authors understand the confusion of our arguments with respect to cyclogenesis and cyclogenetic forcing. We have removed our interpretation of the cyclogenetic forcing resulting at the surface with respect to cyclogenesis to remove the temporal implications of our arguments. Instead with only compare the cyclogenetic forcing in terms of pressure reduction and cyclonic vorticity that results on the surface due to the existence of the PV anomaly comparatively between the experiments. We do acknowledge that future work should look at the temporal aspect of these PV intrusions utilizing a dynamical core or similar.

3. *Dynamic interpretation/explanation of experimental results:* The results here are interesting – but lack a fair bit of interpretation from a PV framework regarding why the responses are occurring. The authors make some efforts on this front, but more needs to be done beyond just reporting the results to really enhance the impact of this study. For example, experiment 4 (changing the intensity of the PV anomaly) shows almost no change in response despite a presumably stronger PV gradient (though it might not be that much stronger given the experiment set-up). The results are interesting – but there's little to no interpretation for why we see the response we do. The same goes for experiment 5 – the authors report the change in tropospheric circulation but provide little interpretation for why. For example – why do we see a decrease in cyclonic relative vorticity with a widening PV anomaly? How can this be interpreted in a PV framework? Why is the surface circulation so much stronger?

We have tried to strengthen the dynamic interpretation of this work as the reviewer suggests. In experiment 4, we agree that is the lack of the change of gradient within the PV intrusion that is the likely cause of the lack of change in the circulation around the anomaly. However, we make the argument that PV values within these intrusions are unlikely massive values far greater than that of the dynamical tropopause and therefore it reflects the real work atmosphere relatively well, despite this limit within the experimental design. In experiment 5, it is explained why we may see an increased relative vorticity value with broadened PV anomaly in the mid-tropopshere.

Minor comments:

General comments:

- The manuscript reads *very* colloquially which is problematic. Please carefully check through the manuscript to identify instances where this occurs – I've identified some examples here, but there are many others throughout:
  - L40: Air can be advected or diabatically altered, but it cannot be 'introduced' to another region of the atmosphere.
    "introduced" changed to "advected"
  - L42-43: The term 'basic' here isn't necessary, and acts to undermine your study (there's little basic about PV theory – it's an advanced synoptic-dynamic topic that readers may not be familiar with).
    "Basic" has been removed as per suggestion
  - L67-80: You use the term 'This study examines/looks at/aims' too much here – aim to rework a bit.
    We have tried to lessen the use of these words as suggested
  - L92: 'diagnostic for reanalysis sets to diagnose' – aim to avoid repetitive words in a single sentence (there were several of these in the manuscript)
    Remove repeated words
  - L237-238 and elsewhere: Unless quantifying, avoid using the term 'stronger' and 'weaker' or similar qualitive statements (other examples include 'meagre' in L370 or 'massive' in L378)
    We have removed the use of qualitative words throughout the text as suggested.
  - L291 and elsewhere: The term 'exponentially' refers to a mathematically derived curved for a set of data points – if it is exponential, prove it; otherwise, please avoid statements that imply something different from what the data shows.

    References to exponential growth have been removed unless mathematically proved as suggested
  - L362 and elsewhere: Aim to avoid injecting opinion or emotion – lines such as 'Since we are dealing with …' should be avoided.
- Definition of terms: In several instances, terms/acronyms were introduced but not defined. As a reader familiar with the topic, I could ascertain nearly everything, but less familiar readers may struggle. Examples include (but aren't limited to):
  - PVU (L37) Updated: PVU ($1\ PVU = 10^6 K\ m^2\ s^{-1}\ kg^{-1}$)
  - COL (L64) COL = cut-off low. First instance has been updated.
  - Reference state (L86) – be sure to define what this is and how you establish it
  - "halo" (L150) A halo is a ring of light that encircles something bright. It is our best word to describe our anomaly setup
  - MSLP (L201) – MSLP = mean sea level pressure. First instance has been updated
  - Sphere of influence (L271) - removed
  - Mid-tropospheric (L347)
  - Total atmospheric system (L478) - removed
- Figures/equations: There were several inconsistencies in the figures that could be tightened up, and equation 6 does not need to be there (it's just a re-arrangement of equation 1 and can be stated as such):
  - Figure 3 and elsewhere: Please always use panel labels (eg. A and B)

consistently. Please also include reference vectors whenever showing vectors that represent direction and magnitude.
We are not sure of the consistency the reviewer is looking for in the figure labels. We have used A and B consistently (A, B, A1, B1, etc) throughout the text

- Figure 5: This would be more clearly communicated as a table rather than a flow chart
  We have converted this figure to a Table (1) as per the reviewers suggestion.

- Figures 9, 11, 14, and 16: Please use the same axis labels amongst these four figures. This is particularly important for Figure 14, which appears to have a large MSLP response based on figures 9, 11, and 16, but in reality is only ~0.3 hPa.
  These have been updated as per the reviewers suggestion
- All captions: Be sure to include all relevant information, such as MSLP contour intervals or wind speed contour intervals.

  Additional information such as MSLP and wind velocity contours are provided as per the suggestion

"Stratospheric intrusion depth and its effect on surface cyclogenesis: An idealized PV inversion experiment"

Authors: Barnes, Ndarana, Sprenger, and Landman

Recommendation: Major Revision

Overview:

In this study, the authors perform a series of idealized experiments in which they invert QGPV anomalies of various sizes, shapes, and vertical depths for their associated horizontal circulations. These circulations are then used to identify QGPV configurations that are likely to be more influential on surface cyclogenesis. In my opinion, while the analysis does not necessarily offer any new qualitative dynamical insight beyond what has already been garnered from the application of a PV framework in prior work, the experiments performed herein do provide a nice systematic quantitative treatment of how nuances in the structure of QGPV anomalies contribute to surface cyclogenesis. This quantitative assessment is novel from my perspective, and justifies the value of this study.

That being said, there several instances within the text in which I felt the present work could be better motivated and described with improved precision. Additionally, the intensification rate of surface cyclones is an important component of the interaction between upper-level PV anomalies and the surface. The temporal evolution of surface cyclones is not considered as part of the analysis but is quantifiable using diagnostic PV tendency inversions. Last, the analysis does not necessarily consider the role that static stability plays in modulating the character of these interactions and may represent an additional experiment that the authors can consider integrating into their analyses. Given the extent of my comments below, I have recommended the manuscript be returned for Major Revisions.

Major Comments:

1. There were several instances within the text in which I found myself a bit confused regarding the interpretation of figures (see minor comments below). I believe that this confusion could be remedied with a thorough review of the text to improve the precision of the discussion and through better definition of various terms. For example, it was difficult for me to differentiate between the physical interpretation of the minimum relative vorticity and the cross-sectional minimum in relative vorticity.
Thank you for this suggestion. We have made additions to the figures, text and captions as specified in the relevant suggestion below.

2. The introduction and motivation for the present work could be made clearer. Namely, it might be effective to construct a figure that highlights the diversity of PV intrusions and how these structures are associated with different surface cyclone intensities in real data. This figure could more effectively frame the idealized experiments performed in this study. There are also several instances in the introduction where the authors emphasize that such a study has not been performed in the Southern Hemisphere. But, to my knowledge, there is no reason to expect that PV anomalies will behave in a dynamically different way compared to the

Northern Hemisphere. Therefore, I recommend this discussion should be either more strongly motivated or eliminated from the text.

The reviewer makes a very valid suggestion. The work was inspired by two papers preceding this work – one of a case study of PV induced COLs and surface lows and another of a climatology of COL extensions associated with PV intrusion depths. Both are cited in this work. It is felt that this serves as justification for the diversity of PV intrusion scenarios tested in this study. Although real world cases could additionally be shown here, this may take away from the idealized feel of the work and make it rather lengthy. We appreciate this suggestion, but have opted to not include at this stage.

3. The authors do not perform a temporal diagnosis of the evolution/development of cyclonic circulations at the surface in their idealized experiments, but such an analysis can be performed in a diagnostic sense using either QGPV (e.g., Breeden and Martin 2018) or Ertel PV (e.g., Davis and Emanuel 1991). I wonder whether the application of this diagnostic framework for examining the instantaneous intensification rate of surface cyclones would bolster the analysis. It is also not clear in the text why QGPV is adopted over Ertel PV. At the very least, this choice should be justified in the context of the proposed applications.
The reviewer proposes a very interesting question regarding temporal diagnoses. The idealized setup reflects instantaneous changes of the parameters that are induced by the various PV anomalies. Although we did consider including temporal aspects visa case studies as was performed in Breeden and Martin (2018), it was decided that this would confuse the direction of the work. We therefore decided to the work limited to instantons changes and analysis. We hope that future work will entail the utilization of a dynamical core etc which we can then use to analyze temporal changes such as those proposed by the reviewer.

4. One element that is not explicitly considered in the idealized experiments is the role of static stability. Namely, those PV anomalies that are situated in a less stable environment are able to more effectively induce cyclogenesis. Could an experiment be run that considers varying the static stability of the environment? Additionally, the inversion of QGPV requires the specification of a reference atmosphere. It is not clear from the text what the authors have selected as their reference atmosphere, unless I may have missed it.
The reference atmosphere is described in the experimental setup section. The analysis of varying static stability environments was not considered here. We would however like to do this in future more intensely by looking at various aspects of the stability of the environments including low-level inversions etc. It believed that this could be best achieved by use of a full dynamical core which would also include temporal aspects of the stability of the environment (for eg. How is a low-level inversion degraded by PV anomaly?)

5. It is somewhat difficult to compare the various experiments because the summary figures (e.g., Fig. 9) feature different values along their y-axes. To better enable a comparison between experiments, I'd recommend standardizing these y-axes across all similar plots.
Thank you for this suggestion. We have standardized all of the y-axes in the plots specified.

Minor, Specific, and Typographical Comments:

Abstract

L15–16: I am a bit confused by the discussion in these lines. Namely, L15 states that horizontal extent is more important, whereas the next line states that vertical depth is important in dictating the strength of the circulation – both of which can be used to characterize cyclone intensity. Could these lines be clarified to better describe the respective influences of the vertical and horizontal extent of the stratospheric PV anomalies?

L17–18: This relationship in this sentence has to be true by definition, and I wonder whether it could be deleted to make more room to better clarify the nature of the relationships described in L15–16.
The abstract has been reconfigured to clarify the above as suggested. In essence, wider anomalies result in a greater decrease whilst relative vorticity on increases marginal whilst the reverse is true for surface relative vorticity changes.

1. Introduction
L41: The specification that "high PV" corresponds to negative values should occur earlier in the manuscript when this terminology is first used. I also believe it could be made clearer earlier in the introduction or abstract that the focus of the manuscript will be on Southern Hemispheric anomalies to avoid any potential confusion imparted on a reader.
The abstract has been amended in order to introduce the idea that the present study takes place in the Southern Hemisphere and introduce the idea that high-PV are negative anomalies in this study.

L44–45: The vertical depth of the circulation induced by the PV anomaly in Fig. 1 is also a function of the static stability. Namely, the lower static stability in the troposphere compared to the stratosphere allows the circulation induced by the PV anomaly to penetrate deeper towards the surface. Some reference to the thermodynamic environment in which the PV anomaly is embedded could benefit the discussion at this juncture in the text.
The static stability settings used within this study are added into the experimental setup section to reference the thermodynamic environment used within this study

L51: In the context of this study, cyclogenesis is described as a near-surface phenomenon. Consequently, I found the reference to cyclogenesis occurring throughout the stratosphere to be a bit confusing. Could this line be revised for improved clarity?

The use of cyclogenesis for the theoretical experimental framework and results have been removed which should clarify the lines referenced by the reviewer

L61: Why is it expected that the influence of PV anomalies will be different in the Southern Hemisphere compared to the Northern Hemisphere? I believe this claim may require stronger motivation/explanation.
The study in reference here is in relation to a climatology of PV intrusions in relation to COLs. The hemisphere in which it is performed is therefore highly relevant to the distribution of these phenomena etc.
The mathemathics and theories of the influence of PV anomalies is hemispheric independent and so for the purpose of this study may not be critical to the result obtained, as the reviewer suggests. We have therefore trimmed our references to it being a southern hemispheric study extensively. However, we have left some reference to this study being from a southern hemispheric perspective as we believe that this type of study in a SH context is rare. We hope

that the fact this study is performed from a SH point of view can help to stimulate further work in our region.

L62–63: The term "stratospheric tropopause" is not accurate, since the tropopause represents the interface between the troposphere and the stratosphere.
The typo has been removed and reference to "tropopause" only is made.

L64: The acronym, "COL", has not yet been defined in the manuscript.
The acronym COL has now been defined

L64–66: It is not clear to me how this conclusion follows from the previous sentences in the paragraph. Consider a revision to improve the clarity of the discussion.

The COL extension climatology by Barnes et al. (2021a) was based on real-case reanalysis data. As reanalysis data, climatological averages and composites were used, the isolated effect that the PV intrusions studied by Barnes et al. (2021a) had on surface cyclogenesis was not considered.

L72: As in L61, it is not clear why the physical influence of cyclonic PV anomalies will differ between the Northern and Southern Hemisphere simply because the sign of PV is negative in the Southern Hemisphere.
The mathemathics and theories of the influence of PV anomalies is hemispheric independent and so for the purpose of this study may not be critical to the result obtained, as the reviewer suggests. We have therefore trimmed our references to it being a southern hemispheric study extensively. However, we have left some reference to this study being from a southern hemispheric perspective as we believe that this type of study in a SH context is rare. We hope that the fact this study is performed from a SH point of view can help to stimulate further work in our region.

L72–73: This sentence is somewhat redundant with the statement that ends in L70. Consider whether it could be deleted.
We agree! The sentence has been deleted and incorporated into L70.

2. Methodology
L89: The Davis (1992) study focuses on the inversion of Ertel PV rather than QGPV, for which the system of equations for performing the inversion features nonlinear terms. For QGPV, the differential operator is linear, which does return a unique solution using successive over-relaxation.

L114–119: I found this discussion to be a bit confusing. Could you clarify more as to why the tropopause is defined differently within the inversion algorithm?
We have tried to reword this section in order to clarify this.

L121: Is the "specified DT" the height of the tropopause above ground level needed for the algorithm or the –1.5 PVU isosurface?
The specified DT is the height of the tropopause needed for the algorithm

L127: Is the intent to refer to the left panel of Fig. 3 in conjunction with this discussion? I ask

because the right panel of Fig. 3 does not show any pressure contours.
The reference to “(right)” has been removed. The intention was to discuss Fig. 3 holistically

L205–210: I found this discussion concerning vorticity thresholds to be a bit confusing. Are you basically looking for areas near the surface that feature vorticity with the magnitude described in the text? Or are you looking for areas where the circulation induced by the upper-level PV anomalies features vorticity of a particular magnitude at the surface.
L213–215: To what extent are the results sensitive to the selection of these vorticity thresholds?

The discussion concerning these cyclogenesis thresholds has been removed. The aim was to try and quantify the strength of cyclogenetic forcing but reviewers and readers found this comparison to cyclogenesis (with a temporal component implied) confusing. We have therefore removed this section

3. Results

L244: If referring to a line of constant pressure here and elsewhere in the manuscript, "isobar" is more descriptive than "isohypse".

Description of the surface pressure in terms of isobars has been removed as a result of our effort to separate the induced response and cyclogenesis type arguments.

L297–298: This particular sentence, as currently written, is a bit confusing. Would it be possible to rewrite it for further clarity? Similarly, I found L300–301 to also be confusing, which may require an edit for further clarity.

This description has changed as a result of our effort to separate the induced response and cyclogenesis type arguments.

L359–365: Could a plot of static stability be produced to help illustrate this effect more clearly?

L392: Arguably, this claim may be best reserved until after the final few experiments have been introduced (i.e., anomaly intensity has not been considered yet). Unless the goal here is to refer to the vertical extent of the anomaly. If so, a revision may be necessary to make that point clearer.

The intention was, as you refer, to refer to the vertical extent of the anomaly. We have tried to clarify this point with some minor wording tweaks.

The results in Experiments 1 and 2 imply that the proximity of a stratospheric intrusion to the surface has a larger impact on inducing deeper and enhanced cyclonic circulation at the surface than the vertical extent or size of the intrusion itself.

L406: Would it be possible to expand further on how this result may be an artifact of the basic state?

L421–427: I'm having a bit of a difficult time verifying some of these values against those plotted in Fig. 14. Could the text be revised to more clearly reference where these results are drawn from.

We have revised the text as suggested. In essence, there are minor differences in the meridional mid-tropospheric velocities but none really at the surface.

"Minimum cross-sectional relative vorticity decreases by 3x10-5s-1 from the low to high anomaly amplitude scenarios, whilst the maximum meridional velocity decreases by 1m.s-1 around the anomaly. Anomalies of all magnitudes tested induce similar cyclogenetic forcing upon the surface. Both the induced surface pressure and relative vorticity are comparable throughout the scenarios tested."

L436–440: Could a figure be produced that shows the characteristic PV structure associated with these categories in real cases. This may help to visually motivate the forthcoming experiment.

L445–446: It is not clear what this particular statement is referring to.

For some reason our reference was left out of this sentence. This should clear up this issue.

L459–460: This statement is a bit difficult to verify. Namely, Fig. 16 suggests that the magnitude of the relative vorticity decreases with increasing width of the anomaly, which is counter to the discussion in this section. I think my confusion here may stem from difficulty understanding how the cross-sectional relative vorticity is calculated/defined.

We have tried to improve our arguments with respect to the lines indicated
"The intrusion width changes result in a change in the geometry of the resultant jet core which appears thinner and shorter the thinner anomaly, whilst the broader anomaly results in a visibly broader and longer jet core, affecting almost the entire cross-sectional domain. Although stronger velocities are observed in the troposphere as a result of broader intrusions, the mid-tropospheric relative vorticity increases sharply for broader intrusions (Figure 16). The larger magnitude relative vorticities induced by thinner intrusions are the result of the circulation, even though with lower velocity, being confined to a smaller horizontal region around the anomaly."

L462: For greater specificity, it may be worth stating that this 1 hPa corresponds to a pressure *perturbation* – as a "lower central pressure" would typically correspond to a stronger storm.
The line specified has been updated as per the suggestion in order to indicate that we are referring to a surface pressure anomaly

4. Discussion and Conclusion
L520–525: These few lines may be a bit redundant with the discussion in the previous paragraph. Consider whether these lines could be deleted without any loss of content.
In this paragraph we state that it is the height of the intrusion AGL that is more critical than the intrusion size itself. The previous paragraph talks to the depth experiments taking into account a similar tropopause height.

Figures and Tables:
Fig. 2: Would it be possible to specify the contour interval for PV in the caption?
Added as per reviewer suggestion

Fig. 5: It is not clear to me from the flow chart why all the experiments connect with the basic state box. It may be clearer to put the basic state box at the top of the image and then have all experiments flow beneath it.
As a result of this suggestion and that of the other reviewers we have converted this flow chart to a Table in order to improve how the variation of these experiments reads.

Fig. 6: Could the contour interval for the meridional wind be included as part of the caption?

Fig. 9: It is not clear to me how the cross-sectional minimum relative vorticity is different from the minimum relative vorticity value. Could this be explained a bit more clearly in the text?

Fig. 9/11/13: Would it be possible to make the limits along the y-axes the same in all of these plots so as to allow for better comparison between experiments?

References:

Breeden, M., and J. E. Martin, 2018: Analysis of the initiation of an extreme North Pacific jet retraction using piecewise tendency diagnosis. *Quart. J. Roy. Meteor. Soc.*, **144**, 1895–1913, https://doi.org/10.1002/qj.3388.

Davis, C. A., and K. A. Emanuel, 1991: Potential vorticity diagnostics of cyclogenesis. *Mon. Wea. Rev.*, **119**, 1929–1953, https://doi.org/10.1175/1520-0493(1991)119<1929:PVDOC>2.0.CO;2.