Nonlinear Processes
in Geophysics
Discussions

Open Access

NPGD

Interactive comment

Interactive comment on "Characterising regime behaviour in the stably stratified nocturnal boundary layer on the basis of stationary Markov chains" *by* Carsten Abraham and Adam Monahan

Anonymous Referee #1

Received and published: 14 November 2018

Summary: Stationary Hidden Markov models (HMM) are fitted based on long time-series obtained from meteorological tower, using Reynolds averaged meteorological state variables (wind speed, wind shear and stratification). The HMM classifies the data into two regimes, corresponding to weakly stable boundary layers (wSBL) and very stable boundary layers (vSBL). The fitted stationary models are used to obtain statistics of regime occurrences, regime transitions or time between transitions. The HMM estimation provides a transition probability matrix describing regime transitions, and the sensitivity of regime statistics to the matrix values is studied. The authors discuss limitations of the stationarity assumption in the model and acknowledge a need to account for external influences in the transition probability matrix. The dynamics of

transitions are shown not to fit the Markovianity assumption. An idea to use a state dependent Markov model, or regime transition probability matrix, in a turbulent kinetic energy budget closure in a weather or climate model is sketched as a conclusion.

General comments: The idea of including a stochastic representation of SBL regime transitions in a turbulence parameterization for weather or climate models is interesting, and suggesting ways to do so is a welcome contribution. As discussed by the authors in the introduction, models have been proposed to explain transitions from weakly stable to very stable states, but no model exist to represent a recoupling of turbulence to the surface after a decoupled state, or in other words to represent transitions from vSBL to wSBL. Numerous observational studies show events such as gravity waves, instabilities or other types of non-turbulent motions connected to a transition from vSBL to wSBL and such transitions take a rather random character. Therefore, proposing to represent such transitions as a stochastic process is an interesting direction. Yet, the presented study falls short in several aspects. The authors start by discussing HMM analyses of the considered tower data which are presented in parallel papers and which give clear signs of non-stationarity and non-Markovianity in the regime statistics. Nevertheless, the authors choose to present the statistics of regime transitions and occurrences that result from a stationary Markov model and to compare those to the observational statistics, justifying this choice by the wish to test the simplest possible approach. The comparison not surprisingly shows the need to include non-stationarity in the model of regime transitions, as was already discussed by the authors based on the HMM analyses in the cited submitted papers. I am not convinced that this is a very important additional contribution. Discussion of ways to consider non-stationarity in the model is kept to a minimum. Further, the non-stationarity is attributed to external influences, such as synoptic meteorological states (cloud cover, geostrophic wind for example, as was also described in Monahan et al. 2015, JAS). This is a very relevant and important fact, and the work should at the very least discuss methods that provide means of estimating non-stationary models of regime transitions explicitly influenced by external factors, and at best include non-stationarity in the model. Methods to in-

Interactive comment

clude explicit influence by external factors have been proposed and implemented in atmospheric applications, including to describe SBL regime transitions (eg: Horenko 2010; Metzner et al., 2012; O'Kane et al 2013; Vercauteren and Klein 2015). The need for state dependent transition probabilities, in a stochastic model that would be implemented in the turbulence closure scheme of atmospheric models, is emphasized rightfully and the authors suggest to relate it to the Richardson number. Why not test a Ri number dependence on the transition probability in this paper? That would make the analysis much stronger. The HMM framework is suggested as a foundation for a new parameterization of SBL turbulence, and discussion on how the authors would see such a turbulence parameterization could be expanded. The suggestion is to include random "kicks" of TKE in the vSBL regime, which in turn affect Ri and eventually a transition to wSBL could occur such as no extra TKE source term will be added anymore. Can the authors give ideas on how such a noise term could be defined? And how could such a parameterization fit with the conclusion of the present study, which state: 1- that a stationary Markov chain is inappropriate to represent wSBL to vSBL transition such as driven by radiative cooling. 2- it is inappropriate to represent the statistics of persistent wSBL and vSBL nights as those are impacted by external influences or large-scale synoptic forcing which induces nonstationary behaviour. 3- it could be appropriate to represent a vSBL to wSBL transition after an initial wSBL to vSBL transition. The third point fits with observational and DNS evidence of perturbations that can drive the vSBL back to a wSBL (such as the DNS of Donda et al. 2015, which are cited but not in this context). The authors could also discuss efforts that have been made to describe such "random" perturbations (eg Kang et al. 2014; 2015), which could help giving a stochastic description of the random perturbations, if not of the impact on the TKE itself.

Specific comments:

1- P1 L20: I would suggest to replace "collapsed turbulence" by intermittent turbulence, or turbulence which does obey Monin Obukhov Similarity Theory. More discussion

about modeling difficulties for the turbulence would be appropriate to justify the need for stochastic parameterisation.

2- P2 L30: unrealistic decoupling is also connected to misrepresentation of the TKE. This point could be discussed.

3- P3 L20: The point of representing regime transitions as a stochastic process is clear, but what kind of parameterization is envisioned in each regime? L25: seasonal dependence: is it not more accurately a dependence on external influences? Such influences have been included in non-stationary regime classification schemes (see general comments).

4- P4 L30: the assumptions deserve discussion. The Markovianity assumption could be tested or relaxed, see eg. Franzke et al. 2009. The stationarity assumption is not fulfilled.

5- P5 L15: work on non-stationary statistical clustering should be discussed (see general comments and references).

6- P6 L5: the fact that the influence of seasonal changes is due to changes in the meteorological state means that explicit external influences would improve the model dramatically. Please discuss how to take those into account.

7- Section 4.1: shouldn't the comparison of observation and stationary Markov chain calculation be done for separate time periods? It could be more appropriate to compare the model results with the observational statistics by dividing the dataset in a training part and a control part.

8- L 15-25: Can the results be discussed in light of, eg., the theoretical work of wSBL to vSBL transitions (the MSHF framework by van de Wiel et al. discussed in the introduction). The importance of physical factors highlighted in this model is not included in the Markov model, again potentially calling for inclusion of external factors.

9- L30: here the fact that the stationarity assumption gives satisfactory results is prob-

ably consistent with the physical ideas of transitions being linked to random intermittent events. This could be discussed it in the context of existing work (see general comments). Fig 3: Isn't it surprising that transitions are overestimated (or underestimated) for all cases, since the Markov chain is fitted on the data? How can this systematic bias be explained?

10- P7 L5-10 and Fig 5: Do the pdfs show the probability of time spent in a state? The text and the figure caption do not seem to match, or rather, the figure caption is not informative as it is. Moreover I do not really understand the grey band. Why is the width of the distribution so dependent on time after sunset? How is the width of the band calculated? How about the seasonal dependence of the time between transitions? Since it was shown to be critical, why forget it here? Relaxation time: I believe that this could be considered by including finite memory in the Markov model (cf Franzke et al. 2009). The inclusion of explicit external influences should be discussed.

11- P8 L20-30 and Fig 7: each colour has a different number of dots, and the caption does not state what individual dots represent. The discussion actually presents results of an analysis which is different than the one presented in this paper and is already presented in the submitted paper cited as a reference. I do not believe that the results and conclusions should be repeated here. The authors could simply state the conclusions of this parallel study in the discussion.

12- P9 L5: Table 3 only shows the observed probabilities and not a comparison of theoretical and observed.

13- Figure 10: what are the grey dots in the figure?

14- P10 L30: Figure 12 does not exist.

15- P11 L5: how are non-stationarities considered in the analysis? If the stationary Markov chain is defined differently for each season, this is not stated very clearly.

16- P11 L30: "The event duration probability density functions ... display a maximum

an hour or two after sunset" I am confused here. I had understood that the figure showed the pdfs of event duration, or time between two transitions. That has nothing to do with the sunset time (And the sunset time is not mentioned in the figure caption either), but would fit with the recovery time idea which is discussed by the authors.

Horenko I 2010. On the Identification of Nonstationary Factor Models and Their Application to Atmospheric Data Analysis. J. Atmos. Sci. 67:1559–1574.

Metzner P, Putzig L, Horenko I 2012. Analysis of Persistent Nonstationary Time Series and Applications. Commun. Appl. Math. Comput. Sci. 7:175–229.

O'Kane TJ, Risbey JS, Franzke CLE, Horenko I, Monselesan DP 2013. Changes in the Metastability of the Midlatitude Southern Hemisphere Circulation and the Utility of Nonstationary Cluster Analysis and Split-Flow Blocking Indices as Diagnostic Tools. J. Atmos. Sci. 70:824–842.

Kang Y, Belušić D, Smith-Miles K 2014. Detecting and Classifying Events in Noisy Time Series. J. Atmos. Sci. 71:1090–1104.

Kang Y, Belušić D, Smith-Miles K 2015. Classes of structures in the stable atmospheric boundary layer. Q.J.R. Meteorol. Soc. 141:2057–2069.

Franzke CLE, Horenko I, Majda AJ, Klein R 2009. Systematic Metastable Atmospheric Regime Identification in an AGCM. J. Atmos. Sci. 66:1997–2012.