The manuscript "Characterising regime behaviour in the stably stratified nocturnal boundary layer on the basis of stationary Markov chains" discusses how well hidden Markov model (HMM) for two distinct regimes (weakly and very stable boundary layers) matches with the observational regime statistic, and how these comparisons help in development of SBL turbulence parameterisations. The authors present a very thorough set of comparisons that lead to detailed conclusions, effectively summarized in the abstract. My understanding is that the stationary HMM is not quite compatible with the observational data, which in itself is a useful conclusion, since it indicates that modified or other types of models are required. Overall, the paper is well-written and the results seem highly relevant, so it could be potentially considered for publication in NPG, except for the point raised in the paragraph below. Other (hopefully) minor comments are listed below as well.

The main problem I had with this paper was that many results and discussions are quite difficult to follow for someone not familiar with AM18a, AM18b, AM18c (which not all available freely on the internet either to refer to - at least I could not locate them), since the authors refer to these three papers quite a lot. It also makes it difficult to assess the novelty of this paper compared to these three papers. (Of course, one natural question is why this paper is not called "Part IV" of the same series and submitted to J.Atmos.Sci. - presumably then the same referees will get to see all the four papers, and better able to judge the quality and novelty of results.)

Other minor comments: (Please note that I have not used the bold-faced symbols $\mathbf{Q}, \mathbf{X}$ etc. in the description below - hopefully it does not cause any confusion.)

(1) Equation (1) should not have $P(x_t = j)$ on the right, and should read:

$$P(x_{t+1} = i | x_t = j, x_{t-1} = k, \ldots, x_0 = n) = Q_{ij}$$

(2) Since equation (2) gives the observational likelihood, conditioned on the states X and the parameters λ, it should simply involve the product of likelihoods at different times as follows:

$$P(Y|X, \Lambda) = \prod_{t=1}^{T} p(y_t | x_t = i_t, \lambda_{i_t})$$

There is also a problem with the notation in the same paragraph: the K-dimensional vector of parameters λ_i for each $i = 1, 2$ needs to have an additional separate index, e.g., $\lambda_{i,\alpha}$ with $\alpha = 1, \ldots, K$.

The other possibility is that the authors really wanted to write $P(Y, X|\Lambda)$ (without conditioning on the states), in which case the equation is "morally" correct but needs a lot more notational changes:

$$\begin{aligned} P(Y, X = \{i_1, i_2, \ldots, i_T\}|\Lambda) &= P(Y|X, \Lambda) P(X|\Lambda) \\ &= P(Y|X, \Lambda) P(X) \\ &= \prod_{t=1}^{T} p(y_t | x_t = i_t, \lambda_{i_t}) p(x_1 = i_1) \prod_{t=2}^{T} Q_{i_t, i_{t-1}} \\ &= \pi_{i_1} p(y_1 | x_1 = i_1, \lambda_{i_1}) \prod_{t=2}^{T} p(y_t | x_t = i_t, \lambda_{i_t}) Q_{i_t, i_{t-1}} \end{aligned}$$

Not pretty, but I just cannot think of any easier way of writing this correctly.

In order to obtain $P(\Lambda|Y)$, of course the above equation needs to be now summed over all possible states, which is a sum over 2^T terms (so I am not sure how the authors deal with this problem).

I hope that for the numerical calculations, the authors did indeed use the correct forms of these equations.

The discussion in the appendix also suffers from similar problems with notation, making them difficult to read, in my opinion.

(3) Since the matrix Q in equations (1-2) and λ in equation (2) (the "corrected" version) do not have the time index, it is automatically clear that they are time-independent, so comment three is redundant (or another way to put it is that these equations are only valid under the assumption of stationarity). So the authors may want to rephrase that comment.

(4) I am not an expert in the field, but the terms "diel cycle" or "diel nonstationarities" were confusing: even a quick internet search does not bring up anything consistent (the first search result is "Diel vertical migration"!). Are the authors simply using it as a replacement to "diurnal" (which is much more common) or some other technical meaning?