Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-367-RC1, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Modeling the contributions of global air temperature, synoptic-scale phenomena and soil moisture to near-surface static energy variability using artificial neural networks" *by* Sara C. Pryor et al.

Anonymous Referee #2

Received and published: 30 July 2017

The following is a review of the article entitled "Modeling the contributions of global air temperature, synoptic-scale phenomena and soil moisture to near-surface static energy variability over the eastern U.S.A" by Pryor et al., submitted to Atmospheric Chemistry and Physics. The article details the application of five candidate ANN models of varying complexity in the prediction of daily equivalent potential temperature over the eastern U.S. during the summer (JJA). The ANNs each use inputs of daily global T2m air temperature and a daily regional synoptic principal component score. Three of the models use soil moisture 90-day running mean soil moistureâĂŤtwo integrated

throughout the total soil column and one for the surface (0.05m) layer-only. With their chosen ensemble, the authors isolate separately the role of soil moisture and hidden layer count in ANN performance. The documented exercise and discussion serves to elevate the importance of considering the coupled (T and Q) thermodynamic variables in warming and heat wave analyses. The main findings of the paper are that (1) more complex, 3-layer ANN models offer superior performance related to linear (MLR) and less-complex, 1-layer ANN models and (2) soil moisture inputs are critical to predicting equivalent potential maximums, especially in semi-arid regions of strong land-atmosphere coupling or 'warming-holes'. The ANN models presented could be used in a forecast mode to transfer forecasted synoptic patterns (T850, Q850, H500) and soil moisture into near-surface equivalent potential temperature forecasts, but one wonders if the T2, Q2, and Psurf would not be available in the forecast to compute equivalent potential directly, especially if these fields are available at 850hPa. The article is generally well written and I appreciate the perspective on equivalent potential temperature being offered, which is that of the appropriate measure for heat severity and static energy variability. This paper demonstrates the predictability of equivalent potential, however, makes no strong linkages to standard measure of human heat indices.

I am providing below a number of minor issues that should be resolved prior to publication.

Abstract: "…measure of static energy…are more strongly linked to excess human mortality and morbidity than air temperature alone"- while this statement provides sound motivation for the study, the two are only weakly connected in the article's discussion and not at all connected in the results. Connecting this idea back in for the Results and Conclusion would be desirable.

Ln 16 model[s]

Ln17 the "drivers", should be explicitly named in the abstract: global T2m, synoptic T, Q at 850 and 500 hPa geopotential, and SMâĂŤall taken from MERRA2.

Ln 25 "Over the [eastern U.S.], the ANN. . .alarm rate [is] ∼0.08."

Main Body Pg1ln30 "elaborated [on] the drivers...health [and socioeconomic impacts](Sanderson. . ."

Pg2ln1 "associated with annual [increased] welfare losses of . . ."- clarify that $57 billion is annual increase due to warming, not annual total.

Pg2ln3 suggest deleting "Many"

Eq1. There should be references to Bolton, 1980; Bryan, 2008; and/or Davies-Jones, 2009. Theta-e should be defined in words here, as well. e.g., "potential temperature plus the temperature increase that would be caused by latent heat of saturation of water contained in air". Suggest starting from potential temperature definition first, then moving to equivalent potential temperature.

Pg2ln14-15 units for each variable need to be added here.

Pg2ln16 should read [g/g], not g/kg in Q

Pg2ln18 isn't T responsive to advection-driven forcing, as well?

Pg2ln22 "[surface broadband] albedo". What is the meaning of "counter-radiated"? Is it "surface downward"?

Pg2ln27 "although [heat extremes] such as. . .(Garcia-Herrara et al., 2010). . .(Vanos et al., 2015)". Other references are required here to strengthen assertion being made. Vanos et al., 2015 covers Midwest only. References list needs to be expanded to cover SE, NE U.S. cases, as well.

Pg2ln31 is Peterson et al., 2011 reference for 2m static energy? Please clarify level to which "lower atmosphere" refers.

Pg3ln2 Davey et al., 2006 reference expected after Eq. 1.

Pg3ln3 statistical models are ill-suited for pursuits of physical, process-level understanding

Pg3ln6 suggest using the more common equivalent term of "summary"

Pg3ln27 "extreme [warm] air T"

Pg3ln34 "extreme [high] Td"

Pg3ln36 "High Plains to [] the upper Great Lakes". Restricted vertical mixing = subsidence?

Pg4lns2-4 scale length for (b) and (c) need also to be specified as it is for (a) (i.e., global).

Pg4ln11 technically, Canada should be masked out of Figure 1a. Domain lat-lon extents should be provided here, as on pg5ln16

Pg4ln12 "trends in Te" please specify over what period these trends were computed.

Pg4ln15 could you provide an estimate of the affected population residing in the eastern U.S. domain?

Pg4ln19 sentence beginning "Therefore,.." is confusing and should be reworded.

Pg4ln21 it is confusing to bundle land management and SM rates of change in the same sentence while they occur on very different time scales. Clarify relative rate of change and period of change.

Pg4ln23 specify the "Parts of the region" that are being referenced. Is this the southern Great Plains? There is no reason to be non-specific here. The GLACE hotspot and two regions of reduced Tmax should be demarcated on Fig 1a, which could be enlarged.

Pg4ln26 Guo et al., 2006 and Dirmeyer and Halder (2017) should be added to reference list.

Pg4ln27 "The study region". The preceding three bullets referred to the study region as “it”. Why the change here? Also regarding: “maximum T during parts or all of the twentieth century”, it is unclear whether “parts” in this context alludes to seasons, years, consecutive years, etc. This should be clarified.

Pg4ln29 “lack of warming” The warming hole label was included in the abstract and this is probably the appropriate place to introduce the term in the article.

Pg4ln31 suggest new sentence begins with “. . .Ellenburg et al., 2016). In the case of Mississippi. . .(AL), [up to]. . .”

Pg4ln32 specify whether “summer temperature” is JJA T, JJA Tmax, JJA Tmin, etc. This section should be more carefully worded and details added to improve clarity of meaning.

Pg5ln7 consider adding a note that MERRA-2 uses bias-corrected P to drive the land surface model, which lends strong confidence to the SM estimates. The skill of MERRA-2 lower-atmospheric fields used in the synoptic airflow classification is yet to be well established.

Pg5ln16 suggest adding 12 EDT /2 CDT after 20 UTC.

Pg5ln30 “strong [southerly] low-level advection of [high] T and Q into the region”?

Pg5ln36 “[Due to its spatial heterogeneity,] soil moisture is . . .”

Pg6ln3 SM plays much less of a role in the radiation-limited temperate NE forests.

Pg6ln6 I would not agree that MERRA-2 SM has been “extensively” evaluated- it is a very new product and only one (biased) reference is provided.

Pg6ln8 it should be specified that validation occurs only over non-forested sites, where in-situ SM is available.

Pg6ln18 was the time difference (2PM EDT, 1PM CDT) accounted for in the comparison? If so, how?

Pg6ln19 is the GPLLJ the only source of moisture for MO and IA? Sentence is currently misleading.

Pg6ln21 “and the presence of abundant SM”. Intent of statement is unclear. SM is abundant in the eastern states, as well.

Pg6ln21 suggest list item (5) should be added, beginning from “There are also important. . .”

Pg6ln25-30 suggest moving these stats out of the data section and into Results.

Pg6ln30 “over [the] southern. . .”

Pg7ln8 please include commentary on how these predictors were selected. What was the logic or metrics employed in the selection process?

Pg7ln24 “readily available to [soil evaporation, as opposed to integrated soil moisture profile that constitutes the water availability to evapotranspiration]”.

Pg7ln25 suggest “Table 1 summarizes the acronyms used herein for each of the five models considered. A schematic of the model architecture and data flows is provided in Fig. 3.

Pg7ln27 suggest including statement that the 70-15-15 sample subsets were identical for all model architectures.

Pg8ln17 this statement appears at odds with Table 1, which lists ANN-HL3 as having the worst model statistics.

Pg9ln2 is there any statistical significance to this difference?

Pg9ln6 “where [modeled] land-atmosphere. . .and where strong [longitudinal] gradients of SM”

Ph9ln13 “such [as] dry lines. . .”

Pg9ln24 I question “evaporation from the Great Lakes”. Is Great Lakes evaporation a strong predictor of theta-e in the eastern U.S.? Evaporation over the Great Lakes is much more substantial in the winter months and drives tremendous lake-effect snow bands, but the affected region is isolated.

Pg9ln30 how is water management signal being linked to the present analysis? MERRA2 has no accounting of water management.

Pg9ln32 suggest DeAngelis et al. 2010 in addition to Pryor et al., 2016 reference for warming hole

Pg10ln5 HL3-SM seems to perform the best; 3 hidden layers as opposed to 1.

Pg10ln7 "...when all [eastern U.S.] grid cells are considered..."

Pg10ln12 please clarify that "test period" here is synonymous with "independent sample", or 15% of all JJA days

Pg10lns5-36. Why can't these stats on HR and FAR be added to an expanded Table 1 for each model?

Pg10ln22 statement on CLM (Buzan et al., 2015) does not appear directly relevant to MERRA-specific results being discussed here. It is out of place, given non-MERRA results are not specifically called out elsewhere in the paper. I suggest removing this statement.

Pg10ln26 To include MO, IA and IL is a stretch. The central U.S. hotspot of GLACE-1 was squarely contained within 27-41N; 106-97W.

Pg10ln32 please quantify "greatly" Is this statistically significant?

Pg11ln1 "HR and FAR are comparable to (or better than) seasonal re-forecasts of summertime T at 2-m..." Please clarify were the HR and FAR of T or theta-e-max,min compared against the HR and FAR of T2m from ECMWF (ERA-I?)?

Pg11ln15 the comparison between HL3-SM and HL3-TOP needs to be included and

thoroughly discussed, esp. for theta-e-max.

Pg11ln32 "from the [MERRA-2]..."

Pg11ln33 suggest "important differences in the magnitude of derived equivalent temperature (Te)[,as well as in strength of land-atmosphere coupling between the reanalysis products (e.g., Ferguson et al., 2012; Schoof et al., 2017)]"

Pg12ln9 sentence beginning "Correlation coefficients exceed..." needs to be reworded and probably split into two or more sentences.

Pg12ln11 sentence beginning "This is true for the simulation..." is confusing. Does this imply that the prior sentence holds exactly for max theta e?

Pg12ln15 the statement that "min theta-e exhibits a stronger dependence on the precise prevailing synoptic scale conditions" is unsupported by the analysis and accompanying discussion. Where has the link between min theta-e predictability and PC number been established and probed?

Pg12ln28 what are the alternative hypotheses in literature for the 'warming hole'? references here or previously upon the introduction of the term would be useful.

Pg12ln30 I disagree that the statistical modeling exercise documented herein has "enhance[d] mechanistic understanding of the causes of variability and change in theta-e". This claim should be deleted.

Pg12ln35. The authors should comment on lessons learned and insight gleaned. For example, the weighting for the global T was stated to be negligible. Then, is their recommendation to forgo global T requirements in future ANN pursuits? Similarly, is the total integrated column soil moisture necessary or merely the surface layer? Does the fact that MERRA2 uses bias-corrected P make any allusions of similar success in full forecast models (with biased, model P) unfair and misleading? In addition to those predictors tested here, which other predictors would be meaningful to explore/consider?

Table 1. Caption should specify over JJA and 1980-2014; “coefficient [(r)]”, “the presence [or] absence”; the total number of grids (1962) should be specified OR the stats should be presented as percentages to be consistent with the text. Column with r>0.8 and RMSE <5k: what about the number of grids that satisfy BOTH criteria?

Fig 1. For (c-g) it is unclear from the caption whether 1980-2014 is still the averaging period; “standard deviation of daily [JJA] (e)...”; Mean [JJA daily] soil moisture...”; explained computation would be more appropriate to include in the main text of article. Is SM also averaged over 1980-2014? ; “total profile [0-1m? profile total thickness]”; the variable names and units should be displayed on each subpanel. Is the 90-day running JJA just June1-Aug31 average or on June 1 the ∼March1-June1 average and so-on? Please clarify.

Fig3. The 5 models from Table 1 should be alluded to here by their acronyms in that table; the domain of actions on the right hand side should be specified as for the predictors. From the flowchart is appears that z-scores are fed into the ANN. From the text I understood that daily z-scores are used to compute daily PC’s which are passed to the ANN. This Figure could be eliminated by adding a data table, which demarcates the predictors and predictands, and describing the rest in text. Currently the flowchart is a bit unclear.

Fig 4. ANN-HL3-TOP is missing and needs to be included. The labels and units for rows 2 and 3 should be added to the far right side.

References (Bolton 1980; Bryan 2008; Davies-Jones 2009; DeAngelis et al. 2010; Dirmeyer and Halder 2017; Ferguson et al. 2012; Guo et al. 2006)

Bolton, D., 1980: The Computation of Equivalent Potential Temperature. Mon Weather Rev, 108, 1046-1053.

Bryan, G. H., 2008: On the Computation of Pseudoadiabatic Entropy and Equivalent Potential Temperature. Mon Weather Rev, 136, 5239-5245.

Davies-Jones, R., 2009: On Formulas for Equivalent Potential Temperature. Mon Weather Rev, 137, 3137-3148.

DeAngelis, A., F. Dominguez, Y. Fan, A. Robock, M. D. Kustu, and D. Robinson, 2010: Evidence of enhanced precipitation due to irrigation over the Great Plains of the United States. J Geophys Res-Atmos, 115.

Dirmeyer, P. A., and S. Halder, 2017: Application of the Land–Atmosphere Coupling Paradigm to the Operational Coupled Forecast System, Version 2 (CFSv2). J Hydrometeorol, 18, 85-108.

Ferguson, C. R., E. F. Wood, and R. K. Vinukollu, 2012: A Global Intercomparison of Modeled and Observed Land-Atmosphere Coupling. J Hydrometeorol, 13, 749-784.

Guo, Z. C., and Coauthors, 2006: GLACE: The Global Land-Atmosphere Coupling Experiment. Part II: Analysis. J Hydrometeorol, 7, 611-625.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-367, 2017.