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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Response of dust emissions in southwestern North America to 21st century trends in climate, CO_2 fertilization, and land use: Implications for air quality" *by* Yang Li et al.

Anonymous Referee #2

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The authors present a study of how dust emissions across southwestern US states could respond to projected climate changes, elevated atmospheric CO2 and land use change. Projected climate changes are assessed for two Representative Concentration Pathways (RCP 4.5 and 8.5) representing moderate and continued increases in greenhouse gas concentrations through the 21st century. The effects of the climate projections on surface erodibility are represented through a dynamic vegetation model that is linked to a dust emission scheme and the GEOS-Chem chemical transport model. The general subject matter of the manuscript and approach taken is consistent

with regional dust modelling approaches today. Linking a dynamic vegetation model to a dust model to investigate projected climate changes is novel, not straightforward, and has potential to provide new insights into the effects (and interactions) of dust emission under changing land uses and climate.

Overall, my assessment is that, while the subject matter is timely, the manuscript has a number of shortcomings that reduce the relevance of the work and confidence that the conclusions are adequately supported by the approach. These include:

1) While the first paragraph of the Introduction seeks to establish the relevance of the study, this is done only at a very high level and specific research and management impetus are not provided. This high-level treatment of the rationale for the work is carried throughout the manuscript, with the text rarely going deeper than general drivers and responses to justify why the work is important, how it can have impact, who it may have impact for, or how any of the processes and interactions between vegetation, land use and climate actually work and may influence future dust emissions. The superficial treatment of these important elements reduces the impact of the work. Adding detail to these elements would give the work more weight and enable the authors to show exactly what the new insights are that they provide, how they are relevant, and where key uncertainties are.

2) A focus of the manuscript is establishing how future vegetation and land use changes may influence dust emissions. However, the authors have not grounded the manuscript in the present situation – What types of vegetation communities are there across the study area? What types of land use changes are occurring today? How important is land use versus land management? How do these present changes relate to the modeled vegetation and land use change scenarios? How are the vegetation communities changing today? What are the implications of vegetation change trajectories today for future responses to elevated CO2, climate change, and land use? How are these changes related to and influence aeolian processes? By not addressing these questions, the work presents as a typical dust modelling study and/but detached from reality.

Expanding the Introduction and Discussion sections is needed to ground the work 'in the real world' and could help the authors demonstrate the relevance and contribution of the study (point #1 above).

3) The modeled vegetation changes appear unconnected to vegetation changes occurring across southwestern US landscapes today and are not adequately represented in the dust model. As described in Sections 2.2 and 2.3, the DEAD model is used to estimate dust emissions, with vegetation effects represented through a linear adjustment term Av that is calculated from VAI that is the sum of leaf and stem area indices. This approach makes two assumptions that are inconsistent with the physics of aeolian transport and drag partition theory: 1) fractional vegetation cover adequately represents lateral surface aerodynamic sheltering – ergo structural changes in surface roughness due to changing vegetation were not represented while they are likely to have a greater influence on dust emissions than fractional ground cover (Av), and 2) adjustments to the fractional vegetation cover can be made through a dynamic vegetation model (to represent vegetation change) that are separate to the dust model drag partition scheme and its use of aerodynamic roughness lengths (z0) – creating a functional disparity in how vegetation is represented in different parts of the model. I identify these issues in full recognition of the difficulty of accurately representing future vegetation change in a dust model. However, these two assumptions also potentially undermine the validity of the model experiments and so need to be addressed transparently. Further, what are the implications of the model parameterization for the rigor of the results? How much confidence can we have in the outcomes of the study? Where are the gaps that need to be addressed? Turning this challenge into a positive – what insights does this work provide for how future research can address interactions among climate change, vegetation change, land use and dust emissions?

4) Literature cited is constrained to dust modelling studies and a few supporting studies related to the vegetation and climate modelling. In addressing my concerns above, the authors could draw on the rich and diverse literature addressing vegetation and land

use changes, and their interactions with aeolian processes, across the southwestern US.

Some specific concerns are as follows:

Line 65: Given the focus of the manuscript on land use and vegetation change as a driver of changing dust emissions, the introduction would benefit from inclusion of a review paragraph/synthesis of the types of vegetation and the trajectories of these ecosystems across the southwest today. This is likely to have important implications for trends in dustiness, with pervasive vegetation changes influencing surface aerodynamics and wind erosivity. The authors might also comment on the likely sensitivity of these vegetation communities to elevated CO_2. See for example references within:

Bestelmeyer et al., 2018. The Grassland-Shrubland Regime Shift in the Southwestern United States: Misconceptions and Their Implications for Management. Bioscience 68, 678-690.

Edwards et al., 2019. Climate change impacts on wind and water erosion on US rangelands. Journal of Soil and Water Conservation. Vol. 74, 405-418. doi:10.2489/jswc.74.4.405.

Line 110: How important is fire in the study area, if at all for the changes under investigation? Supporting references would help.

Line 125: It would be helpful if the authors can define what they mean by vegetation structure. Is this purely geometric (e.g., height, width of plants), or does this include spatial patterns in landscapes?

Line 157: The authors use an estimate of fractional vegetation cover to linearly account for vegetation effects which are predominantly lateral and non-linear for saltation flux and dust emission. While working within the constraints of the DEAD model, the authors should recognize the limitations of this approach and implications for the sensitivity of the model to vegetation change and accuracy of its representation of dust emission responses.

Line 161: How representative are these classes of vegetation communities across the southwest? How do they relate to actual patterns of vegetation? For reference, the authors might look at NRCS ecological site descriptions across the study area.

Line 166: Although, during the first half of spring in the desert southwest, C3 shrubs (e.g., Prosopis glandulosa) may not have leaves such that the main aerodynamic effect is provided by branches and stems. It would be instructive to link actual plant phenology in the study area to what is/is not represented in the vegetation model.

Line 174: How did the authors parameterize the drag partition scheme and represent land use change effects in the dust model? In DEAD, these are represented through the MB95 drag partition scheme, with aerodynamic roughness lengths (z0) assigned to land cover classes. As dust emission is a lateral process, z0 and the drag partition should have a larger effect on dust emission than fractional cover via VAI. If z0 was not changed consistently with the fractional cover of vegetation, the model would represent an inconsistent vegetation effect and would likely not capture the nature of dust emission responses to the examined scenarios.

Line 180: Do the authors mean saltation, or dust emission? Although a general term, dust shouldn't be saltating.

Line 192: Can the authors describe the implications of not changing wind speed? Would you anticipate wind speed changes in response to regional vegetation (roughness) change and changes in synoptic meteorology?

Line 201: Discussion point - what about changes in seasonality due to changes in plant phenological changes due to species change and change in the timing of warming and precipitation? This is partially addressed in the results, but would benefit from further discussion linked to actual plant communities.

Line 232: How do these modeled changes relate to the vegetation communities in

these locations in reality and their dynamics? Line 235: The effect of vegetation on dust emission shouldn't be reduced to growth as it is the kinds and proportions of vegetation in the landscape that influence surface aerodynamic roughness and spatial patterns of dust emission. These changes aren't represented in the model, but do need to be addressed by the authors.

Line 246: Can the authors define what they mean by desertification, and how this differs to the vegetation changes (grass-shrub transitions) that have already occurred over much of this region? e.g., for reference see Bestelmeyer, B.T., Okin, G.S., Duniway, M.C., Archer, S.R., Sayre, N.F., Williamson, J.C., Herrick, J.E., 2015. Desertification, land use, and the transformation of global drylands. Frontiers in Ecology and the Environment 13, 28-36.

Line 269: What conditions would make CO2 of limited importance? Can the authors explain and expand on this in the Discussion? Will CO2 be the main driver of vegetation change, or are other factors likely to be more important/have been important in the past that are likely to influence future trends? (e.g., vegetation state transitions driven in part by land management, not just land use)

Line 278: It would help for the authors to expand on this point about wind as my understanding is that wind speeds were not adjusted for climate changes in the scenarios/simulations.

Line 280: Again, it would be good if the authors can be specific about both vegetation change and land use change. For example, what is the changing land use in west Texas in this scenario?

Line 298: I agree with this statement about the importance of robust representation of both future vegetation changes and the sensitivity of dust emissions to these changes. However, I question whether this need has actually been addressed in the present study. See my major concerns above relating to: 1) description of changes lacking detail and grounding in actual vegetation and land use changes occurring across the southwest, and 2) physical representation of vegetation in the dust model ignores the major effect of vegetation on dust emission (lateral process) and the interactions with vegetation changes that are likely to occur.

Line 312: I think the emphasis on CO2 perhaps oversimplifies the controls. These dryland systems are largely water, not nutrient, limited. But not only cover - this will also be C3 vs C4 dominance and so the proportions and kinds of vegetation on these landscapes will influence responses to elevated CO2. Vegetation state changes today and into the future (influenced to some degree by CO2) are likely to have a far greater effect on the structure and cover of protective roughness.

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