

Interactive comment on “Response of dust emissions in southwestern North America to 21st century trends in climate, CO₂ fertilization, and land use: Implications for air quality” by Yang Li et al.

Anonymous Referee #1

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This manuscript describes a coupled modeling study to investigate the role of climate change on dust emissions in southwestern North America (SW). The role of dust emissions and transport in the SW is important for air quality impacts in the region and has been suggested by other research that dust concentrations and associated impacts will likely worsen. The authors incorporate a dynamic vegetation model and a chemical transport model with two different future emission scenarios to examine the effects of land use change and CO₂ fertilization on dust emissions in the SW. They found that under the most extreme future warming scenario used (RCP8.5), the absence of

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CO₂ fertilization provides an upper bound on increased dust emissions across the SW, but especially in SE New Mexico (NM) and the border between NM and Mexico. It is important to consider various causal impacts in order to design appropriate mitigation strategies, so the types of analyses described in this paper are important and worthwhile. However, a significant weakness of the paper is the discussion and accounting for the role of drought impacts on dust emissions. The authors don't reference this very important impact to the region and how it might impact CO₂ fertilization and the competing impacts on plant growth through water stress. The paper would also benefit from additional organization and clarification. I recommend a major revision to deal with some of these issues- see detailed in the comments below.

Line 14: How is surface air quality defined here? Do the authors mean only particulate matter?

Line 16: Perhaps refer to the “spring time” earlier, it's not clear whether decreasing trends were observed year round and then increasing trends were only in spring? (how is spring defined?)

Line 20: Perhaps refer to the fact that only these two drivers were investigated- the role of drought is very important in this region and does not seem to be addressed in this work. (e.g., see Archer and Predick, 2008; MacDonald, 2010; Prein et al., 2016; Stahle, 2020; William et al., 2020)

Line 22: Instead of RCP8.5, just use “most extreme future warming scenario” like was used in line 15. Or define RCP8.5.

Line 24: Above some reference value?

Line 25: It would be helpful to the reader if the authors leave them with a motivation for this study. Why should the reader care? Future mitigation strategies? Also, some reference to the fact that drought was not studied because many readers will be familiar with the role of drought in this area and wonder if/how/why it was accounted for in this

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study.

Line 32: And soil moisture? Drought? (for example, see references listed above)

Line 39: Here and throughout the paper it is unclear what the authors define as “climate change” and it is important to define it here. Do they just mean increased emissions? Or increased temperatures? Increased drought?

Line 39-42: These sentences read like they should come towards the end of the Introduction.

Line 44-49: Where did these studies occur? It also seems that the estimates would depend largely on the particular region of study, since different regions may have different controlling factors. Line 60: This study indicates the importance of drought, but again, it is not clear whether the impacts of increased drought is included in this study?

Line 69: If “climate-induced changes” includes the role of drought, it should be described here because it is unclear. If it is not, the authors need to address why this very important role was not considered.

Line 75: Like the abstract, the end of the Introduction would benefit from an implications statement, or some description of what these results could inform in terms of public policy or future studies.

Line 77: An overall statement about the Methods section. It is difficult to follow, descriptions of some of the models and methods are scattered throughout the sections. The entire section would benefit from a streamlining and overall organization. It seems like the authors are giving a brief overview of the method at the beginning, which is fine, but in its present form it includes some details that leave the reader looking around for descriptions that aren't included until later. Perhaps leave the overview very general and then describe each step in more detail. For example:

Line 85:86- Time periods aren't given, GISS is discussed here but then again in line 133 (maybe a separate “GISS” section, like the other models have?).

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Line 88: Again, what does “changes in climate” mean in this context?

Line 93: How is “fine dust” defined (again, this comes later).

Line 100: We are in the Methods section?

Line 102-107: This seems misplaced, perhaps it should go in a separate “GISS” section?

Line 109: What land use fields are included in the model and where do they come from? Some reference to this is included in line 237 but would be useful to know sooner. Where does the vegetation information come from? Is it representative of desert vegetation? Where does wildfire information come from and does it change over time? Do the meteorological anomalies characterize future drought?

Line 118: “Future land use scenarios applied follow CMIP5”. Can the authors expand and define CMIP5? What all types of land use scenarios are included?

Line 121-122: Some discussion here regarding how the model accounts for hydrologic feedbacks, such as whether plants react to water limitation?

Line 122: “. . .and analyze results over. . .” This sentence is redundant and unnecessary.

Line 125-128: Discussion of RCP4.5 and RCP4.8 seems out of order here.

Line 129-133: Redundant, see lines 85-87. Again, move the GISS information into a GISS section.

Line 161: How representative are these of desert plants in the Southwest?

Line 165: I assume (based on equation 3) that 7 different PFTs are included to represent stem area index? What are they?

Line 170: Are all plants represented here responsive to CO₂ fertilization? How do the effects of drought, heat, and evapotranspiration offset gains in CO₂ fertilization and can this be captured by the model? If not, it should be stated.

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Line 177: MERRA is mentioned here for the first time?

Line 202: Define “springtime”

Line 205: These boundaries are not shown on the figures and probably aren’t important to mention here.

Line 237: This description of land use change would be helpful earlier.

Line 246: How is “desertification” defined? Does this imply anything about drought?

Line 257: How are “climate stresses” defined and quantified in the model? This implies impacts from drought and water stress on plants, but as mentioned before, this doesn’t seem to be captured by the model? Should “temperature” be “temperate”?

Line 264: What is the land use type shifting towards in these regions?

Line 277-278. I am not sure I understand this sentence. Land use is the driver, but climate change makes up the bulk of the increases?

Line 279: The authors seem to be implying that winds are also involved in these differences?

Line 292: This wasn’t specifically shown in the results (shifts in land surface type).

Line 298-299: And this study doesn’t include changes in wind speed, so it’s hard to say that the differences between the Pu and Ginoux study are primarily due to the changes in vegetation.

Line 308: So that I am understanding what is presented in the Table, CO₂ fertilization would correspond to “fixed land use” but I don’t see 30% or 64% in the table?

Line 312-213: But, as stated previously, it is unclear whether future drought is accounted for, or whether the role of increased temperature and water stress on whether plants are responsive to CO₂ fertilization is addressed. This seems like an important question the authors need to address, as it could change the directions of trends in

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dust emission. The authors need to discuss how or whether this was accounted for.

Line 367: References: There appears to be formatting inconsistencies with several of the references. I encourage the authors to check their reference manager settings (e.g., line 396, 399, 417, 433, 435, etc.). In addition, “doi’s” were not included for any of the references.

Line 486: Figure 1: This is the first time land use is referred to as “anthropogenic” and would benefit from a description of what this means (in text).

Line 517: In the “a” description, include whether “2010” is the first year in the 5 year slice.

Archer and Predick, 2008, “Climate change and ecosystems of the Southwestern United States”, *Rangelands*, 30(3):23-28

MacDonald, G.M., 2010 “Water, climate change, and sustainability in the Southwest”, *PNAS*, 107(50).

Prein et al., 2016, “Running dry: The U.S. Southwest’s drift into a drier climate state”, *GRL*, 43, doi:10.1002/2015GL066727.

Stahle, D.W. 2020, “Anthropogenic megadrought”, *Science*, 368 (6488).

Williams, A. P., et al., 2020, “Large contribution from anthropogenic warming to an emerging North American megadrought”, *Science*, 368 (314-318).

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