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Interactive comment on "Impacts of grazing on vegetation dynamics in a sediment transport complex model" *by* Phillipe Gauvin-Bourdon et al.

Anonymous Referee #1

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The authors present a coupled grazing-vegetation growth-aeolian transport model based on ViSTA and an agent-based grazing model (GrAM). The coupled model is described and a set of simulations used to demonstrate the model sensitivity and vegetation responses to rainfall and grazing scenarios. The premise for the work is that new integrated models are needed to help understand vegetation dynamics in drylands that are influenced by interactions between grazing pressure, rainfall, and aeolian sediment transport processes. In this respect, the work is important and would be of interest to a wide audience across research and land management fields. However, I have concerns about how the work is framed and justified, the level of detail and rigor in the model simulations, and whether the authors' claims are actually demonstrated of the model providing an improved representation of feedback between grazers and

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Discussion paper

vegetation, reinforcing our knowledge, and representing an important improvement for managing arid landscapes over previously available tools. Overall, my assessment is that, while the work is timely, the manuscript has a number of shortcomings that bring to question the general utility of the model and whether the conclusions are adequately supported by the approach. I think that my concerns could be addressed through a major revision of the manuscript.

Major concerns include:

1) The Introduction oversimplifies the context and justification for the work and undersells the value of an integrated vegetation-grazing-sediment transport model. The first paragraph of the introduction hints at but does not describe the complexity of the interactions among grazing, vegetation and aeolian processes. The treatment of vegetation responses, in particular, is overly simple and I think this weakens the authors' case for what could be important work. The problem description and justification could be strengthened if the authors incorporated discussion of vegetation dynamics and the nature of interactions with/among grazing and aeolian processes. See for example: Okin et al. (2006) in Journal of Arid Environments; Ravi et al. (2011) in Reviews of Geophysics; Bestelmeyer et al. (2018) in BioScience; Webb and Pierre (2018) in Earth's Future. In many areas, the dynamics are likely to be non-linear. Framing the complexity in terms of multi-equilibrial models (e.g., state-and-transition models) and drawing on that literature may help to convey the complexity and need to consider the interactions and feedbacks. See Zhang (2020) in Acta Ecologica Sinica for an example of a coupled ecological-wind erosion model that establishes its roots in the vegetation dynamics.

2) The model simulations are not sufficiently connected to an established real context such that, while some environments may produce the model responses, the reader will be left wondering "which environment?" and possibly rejecting the results as they are contradictory to many semi-arid systems (e.g., in North America, Australia, Mongolia, Kalahari).

3) Beyond presenting the model, the purpose of the manuscript and its contribution aren't clear. The authors state that the objective is to identify the response of a semi-arid landscape to climate and grazing variabilities, but which landscape? What is the utility of the model? Who is the intended audience and what are the intended applications? What can we learn from the model that we don't already know, perhaps using more accurate methods? What kinds of questions can it be used to addressed? How does the model provide an improvement for managing arid landscapes over previously available tools? These aspects should be addressed in the Introduction and expanded upon in the Discussion and Conclusions.

4) Structure - I think that Section 2 on Context could be integrated with the Introduction to help establish a single foundation for the work. This would reduce repetition between sections and enable the authors to expand on the nature of vegetation changes and feedbacks that make their approach useful.

5) How do we know the model responses are realistic? Relative to which specific environment, location, soils, vegetation communities? The simulations appear to be more hypothetical than grounded in a particular system. How well does the model work the system for which it is parameterized? How well do the authors anticipate the model to work in other systems with other dynamics? Some level of validation is needed for reader to have confidence that the authors' claims of the model working well for its intended purpose are justified.

6) I think inadequate information is provided for the reader to understand the sensitivity of the model to grazing and rainfall as only total annual rainfall is described, and inadequate information about the stocking strategy is provided. Arid and semi-arid systems will respond differently to grazing and rainfall depending on their timing, intensity and duration. Were these aspects considered in the rainfall regimes? How were these aspects represented in the grazing simulations?

More specific comments:

Line 33: I think the authors need to define what they mean by vegetation degradation and vegetation health.

Line 39: I don't think that the challenges of understanding these complex interactions can (or should) be reduced to a data collection issue. There are multiple examples of where there are sufficient data to address these interactions (e.g., see those described by Webb et al., 2017; Webb et al., 2020; Sasaki et al., 2018). I also do not think that this is a necessary justification for the authors' approach which also arguably has limited representativeness (to the simulations). The authors might have more reach by expanding on the need for integrative assessments - for which there are multiple approaches - and here present one...

Line 52: I don't know why ViSTA would be englobing, or why it is compelling. I don't think these terms are needed so suggest removing. I also do not think that the paper demonstrates either descriptor.

Line 60: The authors should clarify - the impacts of grazing on what? There are many grazing studies of different kinds, most of which have not been connected to aeolian processes.

Line 63: Define what is meant by individual scale. Individual plant?

Line 66: Developed - should be implemented.

Line 69: I think the concepts in this sentence are unrelated and the critique should be broken down as the description of the pasture growth model is an oversimplification and not entirely accurate. While the model did not represent spatial patterns, it did represent vegetation dynamics in the sense of effective changes in species composition (state change) associated with increased grazing pressure. (These were represented through feedbacks to soil properties and plant growth parameters.)

Line 77-83: These sentences are long and difficult to digest. Can the authors simplify each sentence?

Line 109: It isn't clear to me what this means in real-world terms. Can the authors elaborate with an example? Surely the effect of vegetation height in preventing transport is also dependent on the gap size (spacing) between plants, such that as gap size increases the canopy height at which transport is "controlled" will also increase. e.g., short but closely spaced vegetation can be more effective in controlling transport than tall but widely spaced vegetation.

Line 130: The random distribution of grazers is a reasonable first assumption, but not necessarily consistent with grazing behavior, which is likely to be concentrated in landscapes around preferred forage species and water. While the authors start to address the issue a few lines down, this could be a point for more discussion about future work with the model (in the Discussion).

Line 198: In addition to amount, the authors should describe the rainfall characteristics - e.g., was seasonality represented? How were frequency and intensity of rainfall represented? Did these change over time (e.g., were droughts represented in the simulations)? These characteristics will have important effects on vegetation responses to grazing, and subsequent aeolian transport responses.

Line 211: Were the stocking rates implemented continuously for the simulation period? The authors should describe the stocking strategy and its implications for the vegetation and aeolian process responses relative to more dynamic (realistic) strategies over 100 years.

Line 231: What are the drivers/mechanisms producing the changes is grazing that aren't included in the simulations? I think these need to be described in more detail for the reader to understand why the model is producing these responses.

Line 234: Functionally, why would trees decrease in proportion? This suggests the shrubs are out-competing the trees (which would typically have access to deeper water...).

Lines 25-255: Again, what are the drivers and mechanisms of the changes in grass/shrub/tree dominance without grazing or other disturbance to trigger these state transitions? Were the simulations started with proportions of vegetation and cover/height/spacing that were conducive to aeolian transport (i.e., near or just beyond a structural threshold that would produce inevitable change)? I think these details need to be addressed more fully in the methods and simulation setup, then explained in the results.

Line 266: What are the mechanisms? i.e., Can the authors explain the response across the simulations relative to how the plant composition and structure were changing?

Line 310: Can the authors define what they mean by state here?

Line 317: What is meant by poor, and why is sediment transport important for grass survival? Can this be connected to a real-world situation, as usually the opposite in the case in semi-arid systems. Is the result specific to the pioneer grass?

Section 5.1: The authors need to be more specific about which system the simulations are representative of? Which species (pioneer grass) respond in the ways indicated by the model? Where are these found? How transferrable is the model to other systems where grasses and shrubs may respond in opposite ways to that shown here?

Line 343: Can the authors provide actual examples?

Line 365-370: Observed where - in which systems?

Line 370: What is meant by low rainfall regime, and why do wetter (650 mm) systems have vegetation composition with less reliance on rainfall?

Line 392: What is meant by measures? More studies?

Line 395-397: But this is a model, so surely the response is determined by the model. (i.e., It appears somewhat circular reasoning in comparing the response with Martin and Kok, 2017 and claiming that as a new result).

Line 408-409: Why - on what basis should we expect grazing effects to generally be negative for vegetation? I find this statement problematic because it certainly isn't always true and is much more nuanced depending on management.

Line 421: What do the authors mean by a change in the organization of vegetation?

Line 427: It is not clear what the authors mean by compensation in this context. My interpretation of the results in Aubault et al. (2015) is that they showed their systems were highly sensitive to grazing depending on strategy, and that sensitivity varies across soils and plant communities.

Line 431: Again, I think the authors need to specify "for the simulated system", and ideally what that system is. Further, the authors should clarify what they mean by vegetation organization - is that proportions of grass/shrub/trees or spatial arrangement?

Line 440: The lack of sensitivity of the differentiation due to stocking rates could also indicate insufficient sensitivity to stocking rate, depending on the system the model was parameterized to represent...

Figure 2 caption could remind the reader what the simulation names were, rather than just referring the reader to Table 1.

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