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Discussions

Interactive comment on "Rates of consumption of atmospheric CO_2 through the weathering of loess during the next 100 yr of climate change" *by* Y. Goddéris et al.

Anonymous Referee #1

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The paper, "Rates of consumption of atmospheric CO2 through the weathering of loess during the next 100 yr of climate change," by Godderis et al., is a well-written presentation of modeling results projecting future chemical weathering of loess deposits in central North America, under changing future climate. These kind of changes to fluxes associated with land surface weathering under variable future climate have received relatively little attention, even though weathering plays a significant role in the global carbon cycle. In most (though not all) previous considerations, variation in weathering has been seen as important over geologic timescales (i.e. >10ˆ4 years) but has not been carefully considered over shorter time scales. This present manuscript follows another recent paper, also co-authored by Godderis, that highlights the sensitivity of

weathering to climate change on centennial to millennial time scales. In the present contribution, the focus is on weathering of loess and particularly the differing effects of changing climate on carbonate (dolomite) and silicate (albite, K-feldspar) weathering rates. The results indicate that accurately capturing changing weathering of carbonate is vital to quantifying net changes in weathering-driven CO2 consumption, and one of the most interesting and exciting aspects of the paper is the idea that variation in carbonate weathering on land may lead to a 'terrestrial lysocline' that has an effect on the marine lysocline by supplying alkalinity to the oceans. The model presented in this study is unfortunately not able to conclusively quantify the effect of changes in carbonate weathering, given the complexity of the system. Nonetheless, altogether the conceptual novelty makes this a valuable contribution. The comments that follow are intended for discussion, and to potentially help the authors to clarify aspects of what is overall an interesting study.

One overarching concern that is not addressed by the authors is the accuracy of the models that they are using for this region, particularly the climate and biospheric models. In previous work, the authors have established that the WITCH model reliably predicts weathering of North American loess over the past 10 kyr, but, as the authors acknowledge in Section 4, the results of their present study are highly dependent on the climatic and ecological predictions, and these come from the ARPEGE and CARAIB models, respectively. It is not clear how well these capture changes in central North America. The reference for the CARAIB model is related to forests in Europe, and the reference for ARPEGE the Mediterranean. Climate models in particular often capture variability in some regions much better than in others. Given the importance of variation in temperature, hydrology, and productivity for predicted weathering, it seems important to have some validation of the predictions that are being used to drive the weathering model for North American loess in this study. Alternatively, in the absence of such validation, it would be beneficial for the authors to acknowledge carefully in the manuscript that the current study is using one set of predictions as a "typical example" of how weathering might be expected to change under changing climate, rather than as definitive predictions.

On a related note, the authors could usefully provide a bit more information about uncertainty in their results. It may be difficult to provide quantitative uncertainty estimates, but this deserves at least some consideration. For example, is the change in dolomite weathering rate in the southern pedon, from 1.0 mols/m2/yr to 0.9 mols/m2/yr actually a significant decrease? Or are these values for all intents and purposes the same? It would surprise me if, given the number of different factors that influence weathering, this 10% change is actually meaningful. Some treatment of this kind of question in the text would be helpful. One way to do this might be to run more thorough sensitivity tests than are presented in Section 4, where a comparative simulation is presented that involves no change in air temperature. This is certainly a valuable sensitivity test, and the results are useful, but it might be beneficial to have a wider range of such sensitivity tests in order to better assess the meaning of the projections.

I also have a few comments of more specific and technical nature:

The methods could usefully be described in slightly more detail. In Section 2.1, the authors describe the model run by saying they project weathering into the future. To me, this implies starting somewhere around 2012, but the results they show start in 1950. Some clarification on this – and perhaps an explanation of why they decided to start in 1950 – might be helpful. Moreover, I wonder whether there is there any interesting information to be gained from comparing the modeled values for the present day to any observed present-day weathering fluxes for this region (if these exist)? In terms of methodology, it would also be interesting to know why the authors chose 43% porosity, and what the basis is for assuming that all heterotrophic respiration and 1/3 of autotrophic respiration occur below ground (seems reasonable to me, but why 1/3?).

Are there any potential artifacts introduced to the comparison in Section 5 by fixing the initial depth of the dolomite front at 2.8 m? This seems like a reasonable initial assumption, but it would be interesting to know if the results would be significantly

different if the initial depth were at 2.0 or 3.5 m, for example, especially since there are poor empirical constraints on the present-day depth to dolomite. The authors argue that this should not be relevant, but without clear evidence. Sensitivity tests might be useful for this.

In Section 3.2.2, it was not clear to me increasing occurrence of dry events in the south should lead to increasing Na export (end of the first paragraph). It makes sense that the overall greater drainage should increase elemental fluxes, including for Na (end of third paragraph), but the way the first paragraph is written, it sounds like higher Na export is due to the dry periods when albite may stop dissolving. This seems confusing, though I may be missing something here. Either way, it might help to clarify the text.

At the end of Section 3.2.3, the authors comment that the changing weathering under changing climate may also change the chemistry of soils and soil solutions, so plants have access to different nutrients. Is this considered in the CARAIB biospheric model, or is the information only passed "one way," i.e. from the biospheric model to the weathering model. This might be worth a sentence to clarify. There could be an interesting associated feedback.

There is an "and" missing from the first sentence, 2nd p

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