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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 #2

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The MS of Godderis et al. describes the influences of changed environmental factors on the processes and their rates relevant to understand the dynamics of CO2-consmuption by loess weathering.

The manuscript is well written, the design of the analysis clear and the results are discussed considering the state of the art. The complexity of the model framework is high and most parts of the methods are explained in different publications.

Some of the interesting outcomes are that the development of drainage rates are analysed in detail together with other relevant parameters affecting chemical weathering. For example the rate of CO2 diffusing back to the atmosphere is increasing with in-

creasing temperatures. This might be expected, but the approach of Godderis et al. allows studying this process in detail, considering the spatial scale and differences in climate evolution. Altogether, results are relevant for understanding changes of CO2-consumption due climate change, and the MS provides important new insights into the dynamic of the processes.

I would like to ask the authors to consider some questions/considerations, which may improve the manuscript.

While in the introduction the global context is established (e.g. global rates of CO2-consumption rates are mentioned, which is one of the highest one considering recent publications), is it possible to set the results a bit more into the context of global change and the Earth system? I am not sure if this can be done easily. Thus this comment should be seen as a suggestion.

You state that continental weathering is besides other factors a function of physical erosion. However, the reference West et al., shows this for felsic lithologies. In the MS carbonate dissolution seems to be in the focus. Has physical erosion an impact on your results? How well are surface hydrological processes covered by the model framework? See comments below.

It is stated that the multi-parameter dependence of continental weathering makes it difficult to assess the response of continental weathering to climate change. Could you explain why? For example some researchers use simple functions depending on the named parameters to estimate exactly this. Simple functions are also used in the Geoclim Earth system model, if I am not wrong? So it might be useful, and this would shed more light onto the innovations used here, to explain what gaps in knowledge your presented approach closes. For which scales (time and space) are the results relevant or can the outcome of the models be of relevance?

Loess minerals often have "fresh" unweathered surfaces (P10850, L3-4). However, the loess considered here has weathered already a few thousand years. It may be appro-

priate to discuss why the term fresh is relevant for this study, or better, why carbonates are less affected by aging than igneous felsic minerals?

Model settings (P10851, L9 to 18): Are geomorphological settings considered, or terrain characteristics? I guess the loess-areas are relatively flat; therefore surface runoff should be of no significance? What would be the role of slope influencing the percolation patterns assumed here?

Could a more, steep environment bias the general findings outlined here, or a do changes in strong rain fall events (strength or frequency) lead to a significant change of the proportion of surface runoff or a decreased proportion of water percolating to the relevant weathering zones discussed?

The CO2 consumption for dolomite is reported for the south to be about 1 mol m-2 a-1. This is about six times the world average CO2-consumption rate. Are there regional studies to compare these results with? What is the regional CO2-consumption for the continent or carbonates in this area on average? It would be useful to compare the outcome with some reported values.

P10862, L 15: Can the finding, CO2-diffusion increases with temperature, be generalized to other soil-rock systems? I think this is an important point.

Conlusions: Considering the high complexity of the analysis approach, how close to reality is this model set up now? Should it be improved, and if yes how? Or in other words, how, if, can it be applied as a setup for global applications? This type of information would be useful for scientists following comparable or different approaches.

Interactive comment on Biogeosciences Discuss., 9, 10847, 2012.