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Earth System Dynamics
Discussions

Interactive comment on "Economic impacts of a glacial period: a thought experiment. Assessing the disconnect between econometrics and climate sciences" *by* Marie-Noëlle Woillez et al.

Marie-Noëlle Woillez et al.

woillezmn@afd.fr

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One of the issues concerning potential future climate change is that there is no analogue in the "recent" past of our planet of such a change. There are similar changes in magnitude, but not in rate. As pointed out by Nolan et al. (2018) in their study, "under the RCP8.5 scenario the rate of warming will be on the order of 65 times as high as the average warming during the last deglaciation". Therefore, to illustrate the disconnect between climate sciences and econometrics, two options can be considered:

1) Carefully list all the expected climate and environmental changes according to climate models at the end of the century (including extreme events, sea level rise. . .etc)

that are not accounted for by statistical damage functions and show that they would have an impact far above the projections from these functions. This was the approach of DeFries et al. (2019). This option relies on current Earth system models, and there are still many uncertainties.

2) Apply these functions to a different, but rather well-known, climate change that has occurred in the past. We chose the LGM because it is the most recent past period representing a climate change of the same magnitude than what may be our future (RCP8.5). This period is actually often used in climate communication to illustrate the fact that a difference of 4°C in the global mean temperature is by no mean a small change but corresponds to a completely different world. As mentioned previously, the comparison between the LGM and the RCP8.5 has already been made by Nolan et al. (2018) to assess ecosystem changes.

On the one hand, going back to exactly the same climate state than the LGM would require following exactly the same path than for the last glacial period, with the same forcings. By definition, this is not possible (even the different glacial periods of the Pleistocene have their own climate and ice-sheet patterns). In this regard, our scenario is implausible.

But on the other hand, the glacial climate state itself is not physically implausible, since it has already occurred in the past. We excluded the ice sheets from our equilibrium assumption, because considering that the LGM extent and thickness of the Laurentide and Fennoscandian ice sheets is reached would be like assuming more than 20 m of sea level rise in 2100 for the RCP8.5, by comparison with the mid-Pliocene estimates. Looking at the surface mass balance over the Laurentide and Fennoscandian ice sheets at the LGM, as simulated with the IPSL_CM4 climate model, it appears that the balance is positive over most of the ice sheets (except on the edges), with values above 40 cm/year on the southern edges (outside the ablation zone) and 10-20 cm/year more in the center (Woillez et al., 2012). But the spatial resolution of this simulation is rather coarse. Other simulations from Ullman et al. (2015) for the Laurentide

ice sheet actually show that the accumulation rate is above 50 cm/year on the edges, but very low in the center, as you suggested it could be because of the drier glacial climate.

Based on these elements, I consider that, in our thought experiment, snow accumulation on the regions corresponding to the edges of the LGM northern ice sheets would be of a few tens of cm/year in the last decades of the scenario, which would lead to an accumulation of a few meters of snow. The total thickness would of course depend on when the threshold for accumulation is crossed, depending on temperature and precipitation evolution. In the more central regions, it is difficult to provide an estimate based only on published surface-balance maps for the LGM. Yet, for these regions, the decrease of the mean annual temperature is greater than 20°C, which makes them rather unsuitable for significant human activities anyway.

To summarize, a better assessment of what snow accumulation would be at the end of the century would require performing ad hoc simulations using our hypothetical climate scenario as an input for a surface model including a snow model. But we can consider that on the areas corresponding to the LGM ice sheets there would be either a permanent snow layer of a few meters and/or that the climate would be much too cold for human activities. In both cases, we cannot expect the current level of economic activities to be maintained, even less so to grow. Therefore, we do not think that this issue of ice sheets invalidates our demonstration, but we suggest that 1) the above arguments should be added in the manuscript; 2) given the uncertainties on the snow accumulation rate without new simulations we cannot constrain the rate of sea level drop and whether it would be fast enough to really have a significant economic impact, therefore it might be better not to consider these impacts. Moreover, the inconsistency of the GDP projections for southern countries remain: if the damage functions of Burke et al. (2015) manage to simulate a collapse of northern countries because of cold temperatures, the results obtained for southern countries (large positive impacts on GDP) appears unrealistic compared to the climate and ecosystem changes (sahelian

countries for instance).

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