The submitted manuscript by Stocker and Joos investigates differences in anthropogenic land use and land-cover change (LULCC) emissions (eLUC) arising from different methodologies in the literature and presents a case with stand-alone DGVM and a coupled model. The study is a step in the right direction and will help to resolve some existing confusion in the LU literature, but it still needs a sharper focus and clarity. It attempts to cover both a methodological discussion (i.e. different definitions) and an analysis of differences in eLUC estimates in stand-alone and fully coupled models. The current conceptual scope of the former is too idealized and it does not help to understand in depth the latter. I would recommend to expand the analysis of the simulations and to downscale the discussion of the flux definitions to only those relevant to that analysis.

My *first criticism* is that the manuscript needs a cleaner presentation of the mathematical formalisms relevant to analysis of the experiments (may be in an appendix):

- It does not present mathematical equations used to produce figures 1-3 and table 3, just conceptual definitions. How such definitions are used for the cases with heterogeneous and time-varying LULCC? Can they be as easily linearized?
- It also would help to state from the beginning if the formalisms refers to cumulative or net fluxes. It appears that figures show the net fluxes but the methods section states that the equations 5, 6, and 7 compute cumulative CO2 emissions from land use change as a difference in terrestrial C storages.
- Furthermore, it would be useful to include a list of all mathematical terms and what experimental setups they represent. There are a number of *F*s with different sub- and super-scripts and it's hard to follow the equations without having all notations in one place.
- The methods used also make a critical assumption that the environmental effects from LUC and FF combine linearly. I think the validity of this assumption needs to be demonstrated and discussed, both for local and global scales.

Second, manuscript does not discuss implication of unforced climate variability for the eLUC in the coupled and stand-alone simulations. I don't think the SM08 and GC13 approach takes care of natural climate variability; it would be good to include that aspect into consideration as well. Furthermore, I am not sure if it's actually possible for many current DGVMs and ESMs to compute the difference between sources on agricultural and natural lands (i.e delta *f*s) in the same experiment, because most models cannot separately compute physical and biogeochemical soils under agricultural and natural lands. Perhaps the authors could provide figures illustrating how delta *f*s compare to one another in their model, which would be fairly novel illustrations.

Third, if models compute spatial fluxes why does analysis focuses only on global totals and ignores spatial details? It will be useful to go beyond global net flux trajectories, such as in figures 1 and 2, and show maps of LULCC effects. Unlike the global effect of CO2 on climate, the effect of LULCC on carbon is not global but local, and is highly heterogeneous and time varying. If the Bern model is able to compute delta f values separately for agricultural and natural lands in their simulations, they can actually clarify how changes in the C fluxes on different kinds of lands (at the core of the used formalism) relate to differences in total fluxes. Furthermore,.

Fourth, the used definition of the bookkeeping flux as a difference between two experiments is incorrect. The original bookkeeping approach of Houghton 83 and all subsequent Houghton's estimates compute LULCC emissions (i.e. $eLUC_{D1}$ in the manuscript) only for the lands affected by LULCC in the same simulation (there is no F^0_0), not as a difference between fluxes in two experiments as presented in equation 5. The difference equation 5 was introduced in stand-alone models and EMICs studies.

I personally believe that the differencing approach, even if it's the most widely used in the literature, is not a good strategy for characterizing emissions from lands affected by LULCC. The difference in total land fluxes under the method in equation 1, 6 and 7 is caused by LULCC but *it does not represent emissions* from lands affected by LULCC, it's a different metric. Perhaps the authors can clarify this in the results sections. Most models have to invoke differencing approach because of their technical limitations: stand-alone land models and ESMs do not keep track of belowground soil BGC pools separately on lands affected by LULCC and natural lands; as a result, cannot compute soil respiration on natural and agricultural lands. A few models do (e.g. JSBACH and MPI-ESM, GFDL's LM3 and ESMs, as well as some EMICs). In ESMs the fluxes on natural land are not the same in F^0_{LUC} and F^0_0 because of climate variability and a biophysical feedback on climate (the two are of about the same magnitude).

While it's beyond the scope of this manuscript, generally it would be much more productive to analyze how LULCCs affect stored carbon in vegetation and soils where such LULCC are taken place – not just differences in fluxes between a simulation X and simulation Y.