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Interactive comment on "Relative impacts of land use and climate change on summer precipitation in the Netherlands" *by* E. Daniels et al.

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Thanks for your positive words and suggestions for improvement. Regarding your suggested revisions: 1. We will add the 3-5% increase resulting from conversion of historic to present and 2-5% reduction following present to future land use to the abstract. 2. 19 cases were initially selected, but only 18 were used in the analysis. We will check the paper for clarity on this. 3. The model is run for 48 hours, including 12 hours of spin-up from 12 to 00 UTC, 24 hours of simulation and 12 additional hours to be able to compare to both radar data (00-00UTC) and station data (8-8UTC). We will make this more clear. The model is indeed run for 19 cases for each land-use/temperature perturbation. Although 19 cases might still not be enough, it is substantially more than a single case study. In addition, we thought the selection procedure for the cases would assist in drawing relatively strong conclusions, but the results were more heterogeneous than

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hoped for. The initial conditions might have affect the results, though a sensitivity analysis was performed for some of the cases by starting the runs up to 3 hours earlier or later and this had relatively effect and WRF seems pretty robust in its predictions. 4. The linearity in adding the responses is indeed not intuitive, though something we have encountered with WRF before. In Daniels et al. 2015a -referred to in the reference list- a similar linearity is found for changes in the latent heat flux and related parameters. We are unclear on the implications, but thought it is worth drawing the attention of the reader to. 5. The model inadequacy in representing the current climate precipitation can be discussed in the beginning of the results section. What the implications are for the study is unclear. WRF is a commonly used model that does not do worse on predictions than similar mesoscale or alike climate models. The usefulness of alike studies can be sought in understanding the processes governing the changes -in precipitation in our case- not in the numeric outcomes of the models per se. We hope to further such understanding through section 3.2 explaining the atmospheric processes involved. More in-depth discussion on the Trusilova et al. studies will be added. 6.We can add some discussion on natural variability and initial conditions. From previous work, the soil moisture initialisation seems to be one of the most important in general. In the Netherlands those conditions are generally at field capacity however due to the frequent rain and high ground water table and can therefore be expected to have limited influence. 7. Land use change is considered to have a larger effect on convective precipitation, which mostly occurs in summer. 8. We will make this distinction more clear. Neither GCMs nor WRF are unfortunately able to adequately simulate precipitation intensities. 9. This could very well be a sampling effect. The sentence on extreme precipitation is certainly correct. In this case the FUT+1 case is meant, where land use changes seem to counter the increase in extreme precipitation that is observed in REF+1. 10. This comment touches upon our main concern while preparing the paper. The analysis was repeated twice for a different set of data, but led to similar results. This gives us confidence in the response of the model, but whether this response is realistic cannot be inferred. Nevertheless, we hope to add to the knowledge on land

atmosphere processes by describing the surface and atmospheric conditions and processes in detail. Sharing successful and less successful methodologies – in this case the selection procedure (clustering approach)- is in our opinion at least as important as the numeric conclusions that can be drawn from the simulations, which should be treated with caution due to sampling and model issues.

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