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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of historical climate and land cover changes on tropospheric ozone air quality and public health in East Asia over 1980–2010" *by* Y. Fu and A. P. K. Tai

Anonymous Referee #2

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This work by Fu and Tai presents a modeling work investigating the impact of land cover changes between 1980 and 2010 on air quality, and especially on ozone, and public health, in East Asia. The changes in vegetation distribution and leaf area index (LAI), and possible impacts on surface-atmosphere interactions (dry deposition, emissions of volatile organic compounds from vegetation and of nitrogen oxides from soils) are especially considered.

The paper is clear and well written and details an original study, with very interesting results and keys for discussion. I have several, generally minor, comments, mostly with the objective to clarify some aspects of the methodology or the limitations associated

with the results presented in this study, which I warmly recommend for publication in ACP.

General comments:

The term "historical", used in several places of the paper, especially in the title, is misleading, since it usually refers to long-term changes, suggesting that long-term simulations (in this case simulations from 1980 to 2010) are performed, which is not the case here. I would recommend to change the text accordingly, in the title but also in the abstract and other parts of the manuscript, to make it clearer that changes BETWEEN 1980 and 2010 are investigated, and not OVER the whole period.

Section 2.1, NOx emission calculation: when estimating NOx emissions from soils, was a change in fertilizer use and quantity actually considered between 1980 and 2010? If not, how was the consistency between crop location, where most of the fertilizers would be used, and NOx distribution, insured when changing the vegetation distribution? This could affect strongly emission levels, and therefore affect ozone concentrations as well, which should be discussed in the text and especially in the conclusion-discussion section.

Section 2.1, dry deposition scheme: Resistances are used in the Wesely scheme to calculate dry deposition of chemical gases over surfaces. Resistances related to vegetation (stomatal, cuticular, mesophyll) can be significantly variable from one plant species to another. Was the change in those resistances values considered in the model when changing the vegetation distribution and if not, could the authors precise the limitation they would expect, as dry deposition is shown in this study to be a key driver of ozone change?

Values for LAI are derived from satellite observations, and therefore do not integrate the variation from one vegetation types to another. However the distribution of LAI between high and low emitters of biogenic VOCs could have an impact on the eventual emissions. Could the author clarify and give a bit more details on how the LAI was

considered in the model (was only one LAI actually indeed considered for the whole grid or was a species-distribution taken into account) and the possible uncertainties related?

Section 2.1, page 14116, line 22-24: Please explain “but as implemented by Barkley et al. (2011)”. What does this imply specifically for the model integration and BVOC emission calculations or the model generally? Are GEOS-Chem and MEGAN coupled or is MEGAN actually embedded in GEOS-Chem, or running separately and calculated emissions therefore used as forcings?

Section 3: Atmospheric CO2, vegetation, and biogenic emissions: For a better understanding of possible impact of changes in atmospheric CO2 concentrations on LAI, please specify the CO2 levels in 1980 and 2010. Regarding BVOCs, changes in atmospheric CO2 concentrations have been demonstrated to be potentially a strong driver of plant emission capacity in the case of isoprene (Possell et al. 2005 and Wilkinson et al. 2009 for instance), with plant capacity decreasing when atmospheric CO2 increases. Has this inhibition effect been considered in this study when calculating isoprene emissions and if not, what would be the related uncertainty? This really need to be addressed in this section, and discussed in the conclusion as well.

Section 4, lines 8-18, BVOC emissions and crops: The fact that cropland expansion is estimated to benefit to public health, through changes in BVOC emissions, is strongly dependent on emission factors prescribed. Results could be significantly different in a biofuel-type scenario for instance, for which high emitters (oil palm) can be selected. This is a strong limitation of BVOC emission estimates, and of their potential role in the atmospheric chemical composition change, that is not discussed in this study, and for which some elements should really be added in the text, and in the conclusion-discussion section as well.

Specific comments:

Abstract, page 14112, lines 23-24: add “on” in “is more dependent on dry deposition

than ON isoprene emissions”

Introduction, page 14113, lines 1-2: change “public health concerns facing us today” to “public health concerns that we have to face today”

Introduction, page 14113, line 15: remove “ ‘s “ in “Earth’s climate”

Figures: For quicker and clearer analysis of the figures, please add titles on the plots, on top of having them described in the legend, as done in the figure 1 for instance, increasing the font size for better reading.

Figure legends: when dry deposition is illustrated I understand it is related to ozone, please add the information in the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14111, 2015.