

***Interactive comment on “The impact of a future
H₂-based road transportation sector on the
composition and chemistry of the atmosphere –
Part 1: Tropospheric composition and air quality”
by D. Wang et al.***

Anonymous Referee #3

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The paper describes the impact mitigating to a hydrogen based economy would have on tropospheric composition/air quality in a future climate (as given by an emission scenario, somehow ignoring climate change). The subject is generally suitable for publication in ACP, but I have realised that the authors have a companion paper focusing on the stratosphere as well (which I have not been asked to review). I doubt that the amount of new material in the paper merits a two paper approach and would ask the authors to consider merging both papers. This would also help a more ‘holistic’ approach that becomes more and more the norm in atmospheric composition research.

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The paper distinguishes itself by talking about the changes in the future (2050), whereas previous papers just looked on the effect on a hydrogen economy under present day conditions. Unfortunately, even though the authors stress the ‘future’ aspect of their study the integrations are performed with ‘past’ meteorology, with the hand waving argument that only the emissions matter. This obviously negates the earlier assumption that we should think about future climate change in this context. Admittedly the authors consider future emissions scenarios, but I am not entirely sure that this aspect helps the clarity of their paper. Basically the paper compares fuel cell and hydrogen combustion systems in their effect on the atmosphere, and I would encourage the authors to work on carving out the differences between those alternatives more. I would suggest a very clear table summarising the experiments and a map showing the ‘replaced’ emissions. Direct comparisons to the base case are certainly valuable, but the level of detail in listing percentage changes goes far beyond the desirable. I think better metrics describing the ozone change as a function of NO_x reduction would be much nicer (only highlight distinct regional differences). If you wish provide a table of percentage changes at the end or as an appendix.

Very little is gained by the use of the air quality model (but I am not objecting to including the results) and focusing on the troposphere only in the latitude/height cross sections seems a waist of figure space real estate (given the companion paper). Again, I would encourage merging both papers and simplifying the story line strengthening the differences between the key question: What does the hydrogen cell achieve above hydrogen combustion (or not)? Very little thought is given to uncertainties of projections, in particular given that we do not know what a realistic hydrogen leakage rate will be. Instead of highlighting the differences of emissions scenarios in a constant climate I would have been far more interested to learn if it would make sense to think about lower leakage rates.

Aside: I do not share the authors’ classification about non-polluting methods of electricity generation; please just explain what you mean and do not list sources/providers

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of electricity.

Request: Please specify in more detail what averaging of the lowest level(s!) (~ 100 m) mean.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19371, 2012.