

## ***Interactive comment on “Global modelling of H<sub>2</sub> mixing ratios and isotopic compositions with the TM5 model” by G. Pieterse et al.***

**Anonymous Referee #2**

Received and published: 5 May 2011

The paper in question is a new effort to tighten up the atmospheric molecular hydrogen budget with a high resolution global chemistry and transport model, TM5. The paper does suffer from the fact that it reads like a mixture of carefully detailed photochemical model development and arbitrary changes in various source and sink strengths/fractionations in order to adjust model results to match observations. Nevertheless, it is a welcome piece of work that is worthy of publication.

Page 5813, line 9. Should also reference Jacobson, either Science or GRL.

Page 5813, line 21. References needed. The 1.4 year lifetime comes from Rhee et al., a piece of work that this paper apparently discredits and the large variation in current estimates of H<sub>2</sub> lifetime should not be emphasized simply in order to validate the current work.

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Page 5821, lines 12-16. If the discrepancy between model and measurements were due to measurements being made in clean air, then the difference between the two should be greatest for highly populated northern latitudes and should trend toward zero at high latitudes but this is not the case. If anything the data appear to show the opposite.

Page 5824, lines 19 - 20. This could also be explained by an increase in production from tropospheric methane which results in isotopically enriched H<sub>2</sub>, an increase that would be expected during peak production of OH in the SH summer.

Page 5830, lines 20-30. The large proposed mid-latitude strat/trop exchange is somewhat troubling. Some mass balance considerations comparing the mid-lat to high-lat exchange would be welcome. The authors even admit on the next page that TM5 does a poor job with downward transport.

Page 5533, lines 5-8. The authors state that an alpha of 0.9 for soil uptake is out of the range of reported values. At least one of the authors however has seen evidence at least once that there is some evidence that alpha for soil uptake actually is in some cases as great as 0.9 (Rahn, AGU fall meeting 2005). Although not in the peer reviewed literature, the authors reference at least one non-peer reviewed internal report so I see no reason not to either reference the detailed AGU abstract or at least contact the author for permission to use personal communication. It is a rather important parameter in their model and to dismiss the possibility that soil uptake may be solely responsible for the modeled/measured delta based on the meager data available in the literature does a disservice to the reader.

Finally I cannot help commenting that the current work repudiates in several aspects the earlier work of some members of this team (Rhee et al, 2006) and it would be fitting for the authors to take the opportunity to address the discrepancies between the two pieces of work. The work of Rhee et al was submitted to several journals before being accepted by ACP and they continued to ignore the reviewer comments until they

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successfully got it published without addressing legitimate concerns; concerns that this current work show to have been valid (i.e. prediction of a much shorter lifetime, an illogically predicted alteration of the projected soil sink, and a cycle of dD in the SH that is nearly exactly opposite to that reported here). Having published literature that is far ranging in conclusions allows future authors the opportunity to pick and choose the work that they reference to advance their own cause. It also provides the lay community with the opportunity to ridicule the scientific community on the state of their agreement on issues of critical import. Finally, journals request reviewers to spend a significant fraction of their time reviewing papers and proposals and when their efforts are ignored, it does not make them predisposed to accept review requests in the future.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5811, 2011.