

Interactive comment on “A 15 year record of high-frequency, in situ measurements of hydrogen at Mace Head, Ireland” by A. Grant et al.

Anonymous Referee #2

Received and published: 10 December 2009

The paper reports an interesting analysis of a 15 year record of continuous measurements of molecular Hydrogen at the European baseline station of Mace Head.

The dataset has then been analysed in order to derive atmospheric trends in background concentration as well as the influence of European and Southerly air masses on the recorded concentration. Moreover, an interesting discussion on the H₂ to CO ratio behaviour in air masses from different primary sources is reported.

The value of such record lies in that it represents the longest continuous record in the Northern Hemisphere. This is important especially in light of the possible impact of a possible shift to a H₂ economy on the atmospheric budget of molecular hydrogen.

The general comment on the paper is that it deserves to be published on ACP since it

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



provides a great deal of information from a unique data set. The results are important in order to better understand processes governing the atmospheric budget of molecular hydrogen. However, my opinion is that Section 3 sometimes lacks in clarity. In fact, frequently the authors makes statements without too much explanation or supporting evidence.

In the following, some specific comments are reported in which those parts that in my opinion would need a more detailed explanation are identified.

Section 3.1 Baseline Air Masses

At page 6, line 10, the authors state that in Figure 3 de-trended monthly mean values are reported. However, they don't specify how the data are de-trended.

At page 6, line 30, the authors state that no significant trend over 15 years is observed, as from Figure 2. However, once again no details on the statistical procedure used to assess the trend are given.

Section 3.1.1 Biomass burning

At page 7, line 4, is mentioned Figure 4, in which monthly growth rates are reported. This Figure needs to be redrawn because in the x axis there are no tick marks, so that it is difficult to assign years to the peaks and troughs.

At page 7, line 7, the authors should clarify what they mean by "smoother correlative behaviour".

At page 7, line 22, how can the authors be confident that the deviation observed in 1996 can be attributed to long range transport events rather than measurement uncertainties?

Section 3.2 European Pollution Events

At page 8, line 3, authors state that a strong correlation is observed between H₂ and CO peaks, as shown in Figure 6. However, it would be useful to clarify if they refer to

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

non local pollution episodes only or to the whole period shown in Figure 6?

At page 8, line 12, the authors make reference to a procedure to calculate deposition velocity from a manuscript in preparation. It would be useful if they could describe, even briefly, how they calculated deposition velocities.

At page 8, line 14, the authors states that in order to derive deposition velocities they need calm still conditions. However, it is not clear how during summer months it is possible to have the long term records of stable boundary layer conditions that are necessary to calculate deposition velocity.

Section 3.3 Observed H₂ to CO ratios

In general, the discussion of the winter and summer correlation is not very clear. Moreover, at page 9, line 20, the authors state that winter data show a higher correlation coefficient, that is not shown. How much is it higher than the summer one?

Section 3.4 Modelled H₂ to CO ratios

At page 11, in the second paragraph, from line 9 to 18, the approach used to model H₂ to CO ratio is briefly described. However, being this the “heart” of the method, the authors must explain more clearly the procedure used.

At page 12, line 2, the authors use conversion scenarios of 0.47 for both transport and non transport emissions. Could they explain why they use the 0.47 ratio for both?

At page 12, line 9, when authors mention uncertainty in modelling, do they refer to the modelling in general or to the modelling of the ratio?

At page 12, line 12, authors state that this “observation based value is the ratio calculated prior to correction for soil deposition”. Could they explain what does they mean by a correction of observations for soil deposition?

At page 13, line 2, authors discuss the relative production of CO and H₂ for each VOC carbon atom. In the light of the following discussion, it would be useful to dis-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



cuss/consider also the relative chemical loss rates of CO and H₂.

Section 3.5 Southerly Transport Events

At page 13, line 22, Figure 9 is discussed. Concerning this Figure, the authors should add error bars.

At page 13, line 29, the authors state that varying synoptic conditions increase boundary layer ventilation. A deeper low would suggest an enhanced vertical mixing of the boundary layer more than a meridional mixing. Could authors better explain why base-line air would contain a larger southerly component?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20195, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper