Response To Short Comment #1

Table 1 of the paper quotes CH_4/CO "emission ratios" (ER) from fires >4. This is more than 20 times larger than the normal range for this value (0.02 - 0.2). Further, the decrease in CH_4/CO with time is hundreds of times faster than is possible if controlled by the known rate constant of OH + CH4. There are at least two possible reasons for this anomalous data. 1) A typo: if the CH_4/CO ER (0-24 h value) was supposed to have a 10^{-2} exponent, that would "fix" the ER, but not the "implied lifetime." 2) A mixing effect entraining air between levels (i.e. boundary layer or stratosphere mixing with free troposphere) and controlling the values of the ratios. In this case the chemistry is not the controlling factor, which appears to be the author's hypothesis. The mechanism for generating the values in Table 1 may have generated other data (in particular excess values for O3) that are flawed or need further explanation.

The error associated with the CH_4 data is not arising from any of the data processing calculations made in this work. It appears that the problem may be stemming from the quality of the ACE-FTS Ver. 3.0 CH_4 data product itself. This may explain why no correlations for CH_4/CO were observed in Tereszchuk et al. (2011) for any of the biomass fuel types studied, when they should be expected. Until the source of the error is determined, the CH_4/CO enhancement ratios will be omitted from Table 1.