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Interactive comment on "Measurements of CO_2, its stable isotopes, O_2/N_2, and ^{222}Rn at Bern, Switzerland" *by* P. Sturm et al.

P. Sturm et al.

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We would not like to divide this manuscript into two separate short papers. The technical aspects raised here are an integral part and highly relevant to part of the presented data. They point out the present limitations and difficulties when dealing with such data. We agree that the discussion of some presented data is preliminary, but this is due to two reasons. First, there are gaps in most records. We don't have the resources to monitor all tracers continuously without interruption. Secondly, the location of our site in an urban area leads to very heterogeneous catchment areas in space as well as in time. Both these facts make conclusive discussions of some aspects difficult at the present state of research. Still, we think that the concept of combining different atmospheric tracers as it is presented with our results is relevant for the interpretation

of atmospheric CO2 measurements and related tracers.

Answers to the detailed comments:

1) The time resolution is different for the different species as it is explained in Section 2, so we avoid using the term “continuous” in the abstract to prevent a possible confusion. Also, because we are only discussing diurnal and seasonal effects, it is actually not crucial whether the time-resolution is seconds, minutes or hours. To further clarify the time resolution of our measurements we added in Section 2.5 that “The radon activity is reported as hourly mean values.” (This was previously only stated in Section 3.5)

2) The expression “Gas Chromatography Mass Spectrometry” is indeed misleading and for the application described here incorrect. We are not using a GC to separate N2O from CO2 and therefore a N2O correction has to be applied. We have corrected this and replaced GC/MS with “an on-line CO2 trapping system connected to a mass spectrometer”. As it is indicated in the manuscript, a detailed description of this method can be found in Leuenberger et al., 2003.

3) This has been done and reported by Andrew Manning as indicated by the reference (Manning, 2001), so temperature-dependent fractionation at tee pieces is a verified effect. This has been clarified in the introduction. Fractionation at tees is dependent on the geometry of the tee and the flow ratio at the two outlet branches of the tee. The larger the flow ratio is, the more the tee is subject to fractionation.

4) The results of Grew and Ibbs, 1952 (3.9) and Keeling et al., 2004 (3.77ś0.04), are now stated in the text explicitly to better show how well our results compare to the results of these references.

5) The referee assumes that the high CO2 concentrations in winter are mainly caused by combustion sources and that this, rather than the exchange with the biosphere, is the reason for the highly variable results. We propose this explanation on page 8484, lines 26-29. However, we didn’t favour one explanation over the other because com-

ACPD

5, S4185–S4187, 2005

bustion CO2 has a d18O of about -18L' and we observe values as low as -34L'. That's why we assume that combustion sources alone can not be used as a sole explanation. In principle, applying models to these data and considering climatic conditions would be very helpful. The difficulty here again is the limited data coverage and the inhomogeneity in our source areas in combination with the rather poorly constrained isotopic ratios of these sources. Models will be applied with more data available in the future, but this is a major task that does not seem feasible within the scope of this paper.

6) That's exactly the point: We don't have a satisfactory explanation so far. Some of our oxidation ratios are considerably smaller than theory would suggest. Therefore we also take into consideration the possibility that our data might be unreliable. Other groups have also made similar measurements, but no conclusive results are published yet. These oxidation ratios are still debated and therefore only a preliminary interpretation can be given here.

7) As we say in Section 3.4 it is very difficult to quantify these effects, because the observed variations are in the same order of magnitude as the measurement precision. Any further discussion based on these measurements would rather be speculation. We have rephrased this paragraph to clarify this.

8) Because the catchment area of our data is so heterogenous and we haven't got enough data to select for different meteorological conditions, it is difficult to compare with independent measures of CO2 emissions. We are not aware of any comparable CO2 flux studies in Bern, and eddy-flux measurements from boreal or grass land sites don't help with reducing any uncertainties, because they are not comparable. Also, because our data often are influenced on a local rather than a regional scale there is no fossil fuel inventory available that is detailed enough to be useful for an estimation of fossil fuel combustion.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8473, 2005.