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Atmospheric Measurement Techniques Discussions

Interactive comment on "Preparation and analysis of zero gases for the measurement of trace VOCs in air monitoring" *by* Jennifer Englert et al.

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The paper presents exactly what the title says, a description and characterisation of methods for the purification of air specifically designed for use in the preparation of test gases, including calibration gases, for the analysis of VOC in the atmosphere. There are numerous methods described in literature for the preparation of zero-air for exactly this and other purposes. However, as far as I am aware, these published descriptions are limited typically to brief descriptions of air-purification procedures as part of the description for various experimental set-ups and usually do not present a very detailed characterisation of the performance of the air purifier. This paper provides detailed information about the quality of zero-air for VOC analysis and efficiency of several air purification procedures and provides insight into the possibilities, limitations

and problems in the preparation of zero-air for VOV analysis. In essence the paper is a "Technical Note" of very good quality than a scientific paper. In my opinion a (substantially shortened, see below) version merits publication in AMT. My largest concern is the length of the paper. Although overall the results are clearly presented, there are substantial parts of the paper which are unnecessary and only loosely connected to the subject of the paper. I have to admit that after reading the paper I was positively impressed by the solid work that had been done, but frustrated by the fact that I had to go through 15 pages of text, figures and tables to extract some rather straightforward information that probably could be presented in a paper less than 7 or 8 pages long and a supplement. Specific suggestions: 1. Introduction: The part describing the principle of several methods for generation of hydrocarbon free air should be removed. The presented methods are not complete. For example, "pressure swing" methods and use of clean oxygen and nitrogen to prepare clean air are not mentioned, charcoal is by far not the only adsorbent used for air purification. Furthermore, the information provided is essentially textbook level and only vaguely connected to the methods tested here and no information about the performance of the different methods is provided, which greatly reduces the usefulness of this part for the reader. The explanations about the importance of clean gases in general should be removed, the paper contains no information about purifying gases other than removing non-methane VOC from air. 2. The description of steps [1] to [4] (beginning of 2.2) should be clarified. A clear description (and distinction) of "what was done" and "what was determined" at this point will allow to shorten the later (often indirect) explanations of how data were evaluated and what was found. For example, it is later explained that (as far as I understand) step one included measurements using different volumes of zero air. This needs to be explained right away (including the volumes used, after all this is the experiment chapter). 3. The procedures used to generate (and as to determine the quality, see comment 7) "in house zero air" has to be given in the experimental section. 4. Subchapters 3.1 and 3.2 should be moved to a supplement. The typical reader of such a paper will not be interested in the details of peak evaluation and DL determination and knowledge of these

details is not necessary to understand the results presented here. 5. Table 1 should be removed. Its content is only very indirectly connected to the subject of the paper and the information presented has already been discussed in numerous publications and textbooks. 6. Table 2 should be moved to a supplement. The detection limits (as far as they are relevant) are obvious from Table 3 (I assume the <. . . indicates a concentration below the lower detection limit, a footnote explaining this should be added). If concentrations are above the DL the DL has little relevance for the findings presented here. 7. Table 3 should be separated into 3 tables (NMHC, Terpenes, OVOCS) which will avoid the many "empty" boxes. The "saved space" should be used to present the residual VOC concentrations for the use of "in-house zero gas" as feed for the gas purifier as well as the VOC level in the "in-house" zero air without gas purifier. In my opinion, this information is of high interest for potential readers. What are the residual levels of VOC when using a "standard" combination of clean air supply and a given gas purifier. A detail for Table 3 (and some other places in the paper), the number of significant digits presented should be consistent with the accuracy of the given data. 8. The finding that catalysts have to be "cleaned" by running for some time is not new, this part should be moved to a supplement. A useful information (if a available) would be the time constant (if available) at which the different contaminants are removed from the catalysts, which would be relevant for justifying the two hour conditioning time used here. 9. Figure 6 and the detailed discussion of the results of Figure 6 should be moved to the supplement. Breakthrough as well as memory effects and dependence of the efficiency of adsorbents on humidity are nothing mew. Moreover, it seems from Figure 7 that (even when averaging) giving a value for efficiency for the adsorbent is arbitrary since the result will (least for some of the < C5) depend on the duration of exposure to a feed with a given VOC level as well as the history of exposure to feeds with different VOC levels. The low efficiency for most <C5 HC and the high variability of efficiency for most C4 HC is evident from Table 3. 10. All chromatograms should be moved to a supplement. From the chromatograms I could not gain any important insight which is not already evident from Table 3. 11. In the supplement the authors should provide

linear regression information for the results obtained for step [1] in chapter 2.2 (sampling of different volumes) for compounds where the peak areas are not below the DL. This will allow readers interested in details to distinguish between "system blanks" and signals depending on sampled volume. 12. Conclusions: The first paragraph is mostly a summary of the introduction. It also contains statements that cannot be derived from the results presented her (e.g. the importance of monitoring blanks). This paragraph should be removed.

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