Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2015-355-EC2, 2016

Atmospheric Measurement Techniques Discussions

Interactive comment on "Adapted ECC ozone sonde for long-duration flights aboard boundary-layer pressurized balloons" *by* François Gheusi et al.

J. Joiner (Editor)

joanna.joiner@nasa.gov

Received and published: 12 July 2016

Due to a technical issue, the second review could not be uploaded in the usual manner. I am posting the second review below on behalf of the anonymous second reviewer. I realize that the authors have already responded to the first review and apologize for this inconvenience.

Sincerely,

Joanna Joiner,

Associate Editor

This paper presents a novel approach for performing Lagrangian measurements using constant volume balloons and ozonesonde instruments. The authors perform both ground-based (controlled) tests and a few limited comparisons with "coincident" data from three summer field campaigns in which the special balloons were deployed.

Overall, the authors do a reasonably good job demonstrating the effectiveness of the approach in a paper that I found interesting and enjoyable to read. The approach described here could well be adopted by other measurement groups, so the work presented here is important to the sonde community. In fact, it may present the simplest way to sample an air parcel in a Lagrangian manner.

I had a number of questions as I read along, many of which the authors answered, but some of which remained unanswered. The approach of cycling the power on the ozone pump was at the center of my concerns/questions:

1. How is the pump efficiency affected by all of the power cycling?

2. How does the pump motor current (and indication of the pump efficiency and flow rate) vary with time?

3. What happens to the box temperature?

4. How is the "apparent" background current affected? There is some evidence that the background current may not be constant – that it goes down as the instrument runs. Your data hints that might be the case as well. Did you take a look at that? That might be something that could be checked on a running instrument.

Here were my other larger concerns:

1. Your controlled tests looked at the response of the instrument with ozone varying over a pretty tight range (35 – 45 ppb), yet your actual data vary over the range 20 – 80 ppb. How confident are you that the responses over that larger range match your controlled tests in the tighter range? And if someone else wanted to use this approach, how confident would you be in your recommendations if the ambient ozone

was closer to 120 ppb rather than 50 ppb? While the percent differences might track, the absolute differences would be quite different from one another, I think, and those absolute differences are meaningful to some in the audience that might like to employ this technique.

2. HYPSLIT: never rely upon a single trajectory to help understand air mass history. Recommendation: This paper should be published after some minor edits and with the answers to my questions.

Detailed comments:

In the abstract, how does the 1 – 2 ppb/hr compare with expectations?

Below, the comments are listed by “page number – line number”.

2-11: Do you mean to say “the lower tropospheric ozone” or do you mean what you said, “the low tropospheric ozone concentration”? Just want to be clear.

2-21: change “mean” to “method”

3-32: this info is probably relevant to my question about the abstract above.

4-17ff: I think most of this section could be replaced with a reference to Komhyr (1969) and subsequent papers. The info about the solution choice is important – the 0.5% half-buffered solution was the recommendation of JOSIE. One question I did have was whether you did the high ozone conditioning of the cathode cell or just the pump? Referring to Appendix B, it would seem you chose to bypass the cathode cell. Is that correct?

6-12: All of the 2Z En-Sci/DMT sondes I’ve used seem to have just one set of batteries – 2 9-volt batteries in series. I haven’t used the 1Z sondes – are they powered with two separate batteries?

6-15: “. . .only a few percent.”

8-5ff: I'm curious how you did this. Did you let the stopwatch run continuously and try to read it when the film crossed the 0 and 100 mL lines? How precise can those measurements be? What kinds of uncertainties are associated with your flow rate measurements. And since in Figure 7, you tie the y-axis data to the reading near 60 s, the precision of those measurements is going to affect that plot.

8-12: "...the motor was let to rest for at least..."

8-33: The 60s spin-up phase is equivalent to 2 – 3 reaction time constants. Knowing your 4.0 to 1.5 μA decay times would help identify the appropriate "spin-up" duration.

9-1: "I0 – 0.13 μA" seems very high. Do you mean "0.013 μA"? If not, our recommended operating procedure suggests not using sondes with background currents greater than 0.08 μA. Change the solutions. Change the cells themselves if necessary.

9-20: "disequilibrium"

9-26: You say "increase with time" – but as Figure 9 shows, this is good – it's getting closer to 0. It may well go past 0 and the agreement get worse again if time continues to pass. However, I recommend restating this observation, since the magnitude of the deviation is decreasing with time.

11-16: Change "uneasy" to "difficult."

11-20: Change "impossibility" to "inability."

11-28-30: You suggest that condensation caused the problem. Do you have any evidence to that effect? Dew point temperatures versus temperature? Observations of clouds? Sondes do not seem to have trouble with most clouds. Is this a problem with a low cloud or fog that would affect the ozonesonde measurements differently than a typical cloud?

12-5: "...Ersa surface station agrees fairly well by the end..."

12-15: I wouldn't use the word "proved." At best, your observations "demonstrate an ability to provide ambient ozone mole fractions. . .with an accuracy of about 10%."

12-21ff: You compare your observations with a campaign in the North Atlantic off New York City (Mao et al., 2006). Would you expect these to have similar results? How do the currents, water temperatures, winds, convection, etc. compare between these two sites, and how do those factors influence the results? In other words, why should we expect the Mediterranean to look like the Atlantic?

13-4: Have you looked at potential temperature in addition to specific humidity?

13-27: ". . .to conclude in situ ozone production."

13-31-32: "This is presumably the first time that ozone production is evidenced in the free troposphere from direct observation." See the paper from NASA-TC4 data by Morris et al. (2010) on ozone production from a dissipating tropical convective cell in which they report 6 – 12 ppb/hr in the lower free troposphere. While the mechanism in that paper is lightning NOx production, it does provide a quasi-Langrangian analysis using an ozonesonde that oscillated between 2-5 km over a 90-minute period. Your study would be the first with an intentionally designed instrument to sample in a Lagrangian fashion.

14-5: ". . .allows us to clearly distinguish. . ."

14-12-14: This discussion about humidity and ozone-humidity relationships reads as though it is universally true. In particular, the comment that "higher ozone concentrations are therefore expected in the free troposphere than in the boundary layer," certainly relates to the local conditions for this flight and is not true generally. Just rephrase these statements to clarify your intended meaning.

14-13: I would avoid the word "global" when you are talking about a general characteristic of your particular data set.

14-14: ". . .section 1, and no chemical evolution. . ."

14-18 and 21: delete the word “here”

14-27: clarify that this is a positive temperature change: “+40C”

15-3: “. . .likely owing to water condensation weighting the balloon.” The Morris et al. (2010) paper makes a case for this mechanism, too. But if you do, what is your evidence? Again, having the temperature and dew point temperature data should tell you whether this is indeed the case. If you can’t demonstrate that explanation, I think you should just delete the sentence.

15-6: I would mark this discussion of the model results as a new section of “Model results.” One question I had at the end of this section, does the model predict/show the 4 different air regimes you’ve identified in Figure 14?

15-26: I think “photochemical production” is better.

16-1ff: It is inadvisable to use HYSPLIT in a single parcel mode to evaluate air parcel histories. You should initialize a matrix around the starting lat/lon and run at several altitudes to verify that the trajectories hold together. The spread of the matrix will give you some indication of the reliability of the trajectory calculations and sensitivity to the initial conditions.

16-14: Delete “in contrast.”

16-19: I think “photochemical production” is better.

17-17-18: “The few other data sets available from. . .measurements) suitable for in-flight validation all show reasonable agreement. . .”

17-32: Same comment about the first observation of ozone photochemistry in the free troposphere – see Morris et al. (2010).

Appendices: While nice, I think most of Appendix A information could be subsumed into a reference (or series of references). The background current info is interesting, though. 22-22-23: The fact that the I0 value was adjusted so that the sonde matched the ozone analyzer might not have been the right thing to do. How different were these values, typically, before the adjustment?

28-Table 3: I know that there are different recommendations that depend also on the solution type. Not sure about Z sondes. Just asking the authors to verify they are using the best correction factors for the sonde type and solution type. I realize that the solution type should have nothing to do with the pump efficiency, but the WMO group noted that there's some "offsetting" of errors that happens, so these pump corrections do more than just correct the pump efficiency.

Figure 5: Difficult to see the reference sonde data. Change the scale? Plot differences between reference and experimental sonde vs. UV analyzer?

Figure 7: Perhaps show the raw measurements, too, rather than % variation?

Figure 8: A -3.3 ppbv bias suggests to me that the background current used is too high. Thoughts?

Figure 9: Looking at this figure got me thinking to ask how does the pump motor current change with time? How does that affect the flow rate and hence, the agreement of the measurement?

Figure 14: I think this comes across in the text, but looking at just this figure, region 1 is above the nocturnal boundary layer, region 2 is in the boundary layer, region 3 I sin the lower free troposphere, and region 4 is in the boundary layer again. Right?

Figure 15c: Might you also so this as a scatter plot of model versus measured ozone? That could be interesting to see how well the model picks up the variability...

Reference:

Morris, G. A., A. M. Thompson, K. E. Pickering, S. Chen, E. J. Bucsela, and P. A. Kucera (2010), Observations of ozone production in a dissipating tropical convective cell during TC4, Atmos. Chem. Phys., 10(22), 11189-11208, doi:10.5194/acp-10-

11189-2010.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-355, 2016.