Response to JBJ Review

Al Cooper

5/3/2021

This is an exceptionally helpful review, demonstrating careful and thoughtful reading. I have incorporated almost all suggestions. Detailed responses follow:

1. Page 1, Line 18: **"You call it the spectrum, but it implicit is that it is the significant contributions. I would suggest changing to: ....eddy sizes that makes the significant contributions to the flux."** -> *Changed as suggested.*

2. Page 4, line 13: **"Casual readers may not readily understand the Rosemount part number and relate it to a sensor they know. Even if you call the sensor "the unheated sensor", I suggest making an additional brief reference to add: "sometimes called the Fast Rosemount sensor", or something similar. This would also be worthwhile in the abstract and the conclusion section, assuming that some readers will only skim the paper. And the particular sensor is not mentioned on page 2, lines 29-30. A bit extra explanation would be helpful."** –> *Appropriate changes were made in all these locations.*

3. Page 1, line 4-5; page 2, line 13; page 4, line 26 (and maybe elsewhere): **'Mostly you attribute the heating to "compression", in other places to "airspeed fluctuations". They are obviously related, but a bit more explanation would again be good.;** –>*It seems to me that the explanation in the abstract, lines 5–6, covers this and explains the connection.*

4. Page 5, figure 1: **'Why does "best" with small tau1 and tau2 give worse amplitude and phase for frequencies above 1 Hz? I would have expected the opposite.'** –> *? The amplitude response is larger for "best" as expected because the signal is less attenuated for "best" as a result of the smaller value of tau1. The phase shift is smaller than for tau1, as expected because a larger time constant leads to inhibited response and a larger phase shift. I think the only anomaly in the plot is the small region just below 1 Hz where the phase shift for tau2=0.6 exceeds that for "best". However, there can be some interplay between the effects of the time constants and I didn't think that was a problem. Incidentally, "best" refers to the best fit, not necessarily the best response; these are just included to illustrate sensitivity to the coefficients.*

5. Page 5, line 4: **"The fastest GV air speeds that I can readily find result in a dynamic heating of 29.7C. Thus your statement of "exceed 20C" is technically correct, but also an underestimate for high-altitude, fast jet speeds."** –> *Right; I was just indicating that the correction is major. I have changed to "can exceed 25C".*

6. Page 6, line 9: **"Swap for McFarquhar reference 2020 in BAMS."** –> *Thanks; done. (It wasn't published back when I started this.)*

7. Page 8, Figure 2: **"Fig. 1 has amplitude ratio in the top (and phase in the bottom). It makes it difficult to quickly compare. Also, one label is amplitude, the other one is amplitude ratio."** –> *I changed both figures as suggested, and also changed Fig. 4 to have the same structure.*

8. Page 9, Figure 3: **"The reader is left with the only difference being the application of heat or not. But the sensors are also very different in construction, mass, etc. Even a brief reference to fast and slow might help."** –> *? The text does explain that these are different sensors from different manufacturers. Earlier reviewers complained that I kept repeating the model numbers, so*

I changed to "heated" and "unheated" but explained early in the text what these references meant. I have now include "fast" in the first description of the unheated sensor.

9. Page 9, line 8: **"Suggest add "(for the unheated sensor)" towards the end of the line."** –> *The line now starts with, "Fits using the three-parameter representation of the transfer function were unsatisfactory for the heated sensor, . . . "*

10. Page 10, legend to Figure 4: **"Here you use "gain" as opposed to "amplitude ratio" in past descriptions."** –> *Changed the caption to "amplitude ratio".*

11. Page 11, Table 3: **"Here is finally a reference, side-by-side, to the names of the unheated and heated sensors."** –> *? Yes, but they were each specified when first introduced.*

12. Page 12, line 6: **"Add to the 5 m/s that it is the vertical aircraft speed."** –> *Changed to ". . . when the aircraft descent rate through the top of the marine boundary layer was approximately 5 m/s."*

13. Page 13, first and second paragraphs: **"This is such an important point, that I think that you should blow your horn a bit more, e.g., by adding 'novel' or preferably something stronger."** –> *Started the 2nd paragraph with "The new approach . . . " to emphasize that this is original.*

14. Page 13, line 4: **"Change 'As' to 'as'."** –> *Done.*

15. Page 17, line 30: **"Consider reference for Daniell smoothing."** –> *This is well known smoothing for a periodogram, but for clarity I have changed line 30 to begin, "The cospectra have been smoothed using width-3 moving averages for . . . "*

16. Page 19, line 14: **'Make more clear, suggest: ". . . . but the flight speed of the GV aircraft was about 30% higher in the present case so a larger error would be expected." '** –> *I made this change: "the flight speed of the aircraft was about 30% greater than that of the aircraft they used so a larger error would be expected."*

17. Page 19, line 20: **"Do you take the 130 m/s and 1.3 − 10 Hz from the Lenscow reference or from the present manuscript figures?"** –> *From Lenschow. The text includes ". . . cospectra provided there suggest . . . " to indicate that the suggestion comes from that reference.*

18. Page 19, line 28: **"Can you give more of a reference for how you imposed the 0.3 correlation?"** –> *This seems straightforward enough to me: Generate two random time series s1 and s2, use s2 for the vertical wind, impose correlation via T=(1-0.3)s1+0.3s2. There's a lot more complicating steps here, including weighting to get -5/3 spectra and correction to find the actual measured recovery temperature. The original had much more explanation here, but the simulation results didn't seem to justify adding all that to the manuscript so the present version tries to summarize the results. An advantage of the R markdown format is that all these details will be available to anyone really interested in digging into them.*

19. Page 20, line 4: **'Consider adding quotation marks around "measured", because it is really calculated.'** –> *Changed to "The result for the cospectrum that would be measured . . . "*

20. Page 20, line 8: **"You list wavelengths here, but Figure 11 only shows frequencies."** –> *The heavy dashed-black line denotes the frequency corresponding to 2.5 km, the only wavelength mentioned. The description of that line was omitted from the caption but is now included.*

21. **"Section 4.3 is really good."**

22. Page 22, line 5: **"I would add ' . . . to new cases and to slower aircraft."'** –> *? This statement applies to the flux, not the aircraft. It was stated in terms of the aircraft used here, so I have rephrased it in terms of wavelength. The limiting frequency for a slower aircraft would be smaller, for the same wavelength limit. New statement: ". . . still higher frequencies. (At typical GV flight speed, these frequencies correspond to wavelengths smaller than about 14 m.) . . . "*

23. Page 22, after line 7: **"Again, blow your horn a bit and say that the present technique may go a long way towards closing the 20% gap between eddy-covariance measurements of heat flux from research aircraft and towers."** *–> My preference is to omit this and not deviate into discussion of the tower measurements. I hope that the presentation makes the case.*

24. **"This is a really important paper!"**