Don Lenschow comments, 29 May:

1. (9) can be simplified to: Q = T_r(rm^2\gamma T/7)(1 + T_r(rm^2\gamma T/7). With this formulation, you don't need to introduce C_v.

I wanted to quote the Bange et al. (2013) reference because that is widely available and authoritative. For an ideal gas containing no water vapor that form reduces to

$$Q = T_r \frac{\alpha_r M^2/5}{1 + \alpha_r M^2/5}$$

so maybe for simplicity I could add that. (That doesn't quite reduce to your form, but I think the above equation is right.) However, I prefer the equation as given by (9) because, with appropriate adjustment of the gas constant and the specific heats, it is valid for moist air. I don't make a point of this, but I do use humidity-adjusted values and your simplification requires dry air. All the measurements I use are in the humid marine boundary layer,

2. Is the time constant for dynamic heating the same as for temperature fluctuations?

I think it must be because the sensor is responding to the same thing, dynamically heated air (the recovery temperature). That is why I make such a point of treating the recovery temperature when characterizing the transfer function or correction procedures. If the proposed differential equations are applicable, they are linear so the response to the recovery temperature is the sum of the responses to the air temperature and dynamic heating.

3. Why not show the coherence function so the reader can gauge the significance vs. frequency of the correlation between T and u??

I did look at that but ended up omitting it from the paper while trying to shorten to a reasonable length. The problem is that there is high correlation between T and Q but it is mostly false because it is produced by incorrect treatment of Q. It seemed to me that the transfer-function plot made the points more clearly without having to explain the source of that erroneous correlation, which is discussed at a later point in the paper. The coherence between Q and the recovery temperature might be more relevant; here it is:
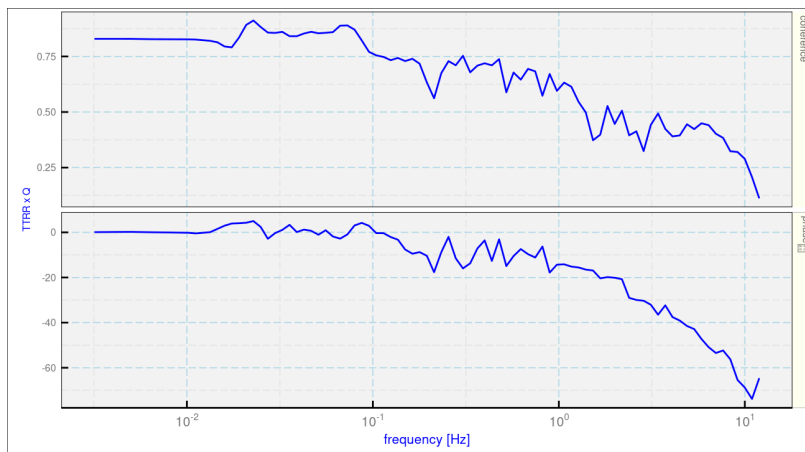
4. You seem to assume that T spectra have more energy at low frequency than u spectra. Why not show plots to see if that's true? (p. 8, 2nd last para.)

I did check before making that statement. Here is an example:
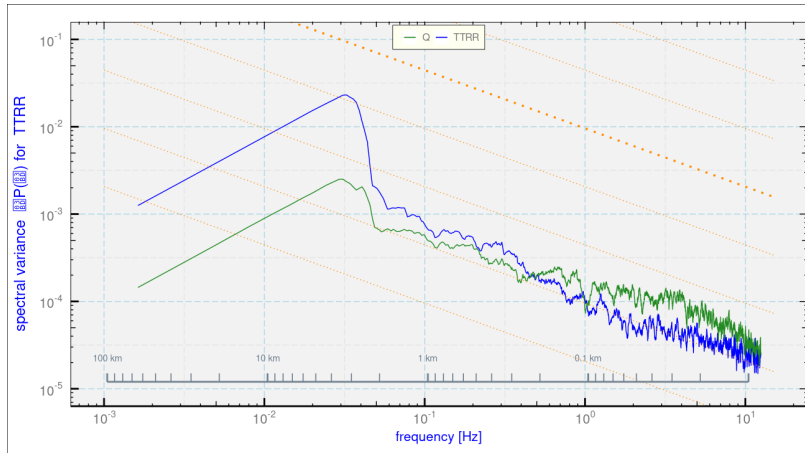
I'll change that statement to reflect that I know it to be the case, rather than sayng "it appears likely", which I said because I hadn't checked all situations exhaustively, only those I used.

5. Another way to calibrate the time constants might be to provide a heating function to the sensor. For example, apply a voltage step function to the sensing bridge large enough to generate a ttmperature increase, then reduce the voltage. Or possibly also drive the bridge with a sinusoidal voltage large enough to generate a sinusoidal temperature rise and vary the frequency of the driving voltage.

Josh did just this to find the response of the thermistor we were investigating, but that doesn't help determine the effect of the support or the relative importance of heat transfer to the support. The way I have done this leads to separation of these parameters with low associated uncertainty.

6. The T'w' covariance has an associated integral scale that may not be incorporated by your parameterization. In the literature, the cospectrum of w'T' has a -7/3 slope. The integral scale of the heat flux is \lambda_w\theta = 0.16 z/z_i^1/3, where z_i is the CBL depth (Lenschow & Stankov, 1986)

I'm sorry, I don't understand what problem you are identifying. My parameterization relates to the time constants, not the covariance; I plot the covariance without any parameterization, as a function of frequency.

Here is the autocovariance function for $T'w'$, which is based on a flight segment of about 115 km. It isn't particularly well behaved, but I suppose it indicates a length scale smaller than 1 km, with first zero crossing at <250 m. That would suggest that the flux estimate is based on a flight segment more than 100–500 times the integral scale. That might be marginal but should be enough for the argument being made, which is not related to the flux but to the magnitude of the correction. The flight altitude was about 150 m and the depth to the inversion was 1500 m, so $0.16(0.1^{1/3})z_i$ predicts about 100 m. However, see plots below that show this is a very stable layer, not well mixed.
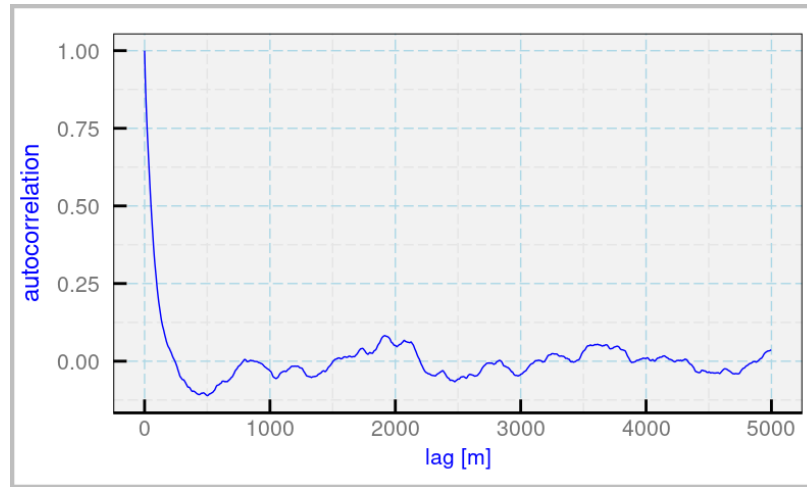
As Fig. 11 shows, the cospectrum is much flatter than a -7/3 slope (which would be -4/3 in these weighted plots). The high-frequency slope is about -1/3 in weighted plots or about -4/3. However, the conditions are stably stratified, with evidence of anisotropy, so they may not conform to inertial-subrange expectations despite the -5/3 slope of the wind-component variance spectra. Here are some sample plots from the CSET case that show the structure of the layer below the inversion at about 1500 m.
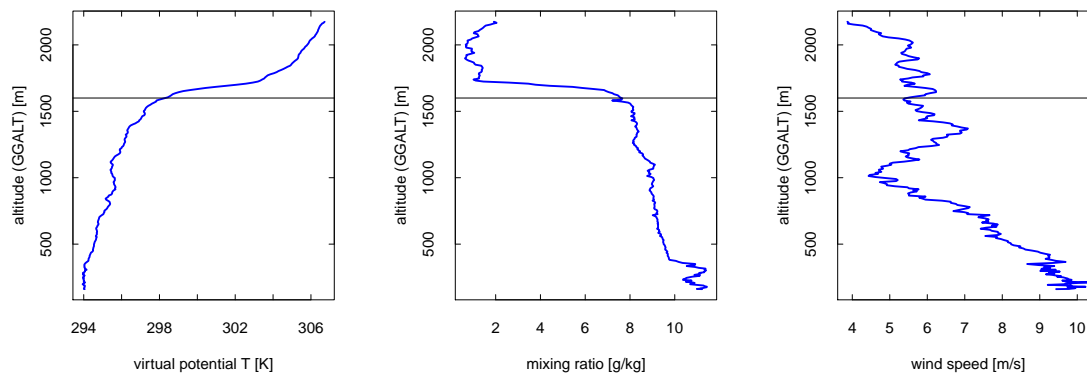
The mixing responsible for the flux appears to be driven by wind shear and the along-shear component exceeds the cross-shear component even with consideration of the expected 4:3 ratio.

I did add a reference to Lenschow and Stankov, 1986, to indicate that the length of the flight segments used here is just marginal to meet the 10%-uncertainty criterion you proposed: "\citet{lenschow1986length} suggested that, for 10\% uncertainty in a measurement of scalar flux, an averaging distance of 100--500 times the boundary-layer height is needed. The flight segments in these two cases span about 80--150 times the boundary-layer height, so they are marginal by this criterion, but the measurements still serve to illustrate the effect of the proposed correction."

7. In the discussion in #2 of the procedure, you discuss the Fourier representation of the air temperature (which is complex) being multiplied by the complex conjugate of the Fourier representation

of the updraft, but you don't point out that a significant contribution to the error of the heat flux estimate using the air temperature uncorrected for its limited response is by the phase shift in the uncorrected air temperature, although it is obvious in Figure 1.

Right, but I did point that out in the discussion of Fig. 1. I added an additional comment to that effect in the section where the correction procedure is discussed.

You also don't point out that the temperature and "updraft" signals are assumed to be in phase (or anti-phase). This seems to be the case, but I think it might be worth pointing out.

I use the cospectrum, so this doesn't make any assumption about the signals being in phase but isolates the in-phase component, whether they are in phase or not. I hope my added comment about the phase helps here also.

I also wonder why you use the term "updraft" rather than "vertical air velocity" since "downdrafts", which you don't mention, also contribute to the heat flux.

You are right, I prefer "updraft"; "downdraft" is just a negative updraft. "Updraft" is just more compact. "Velocity" is a vector, so the proper term would be "the vertical component of the air velocity" which is just too cumbersome.

8. It would be possible to be more quantitative as to what the cospectrum looks like in a convective boundary layer. First, in the "inertial subrange" the cospectrum is expected (and has been observed) to decay with f^(-7/3) rather than the f^(-5/3) slope that characterize the component spectra.

Again, the cospectra I show are far from a -7/3 slope (or from -4/3 in my weighted plots), as is evident from Fig. 11. As I commented above, I don't understand what might be wrong with them. Any ideas? When plotted using a linear scale and normalized as in Fig. 5.5 in your reference, the cospectrum looks rather different, with more contribution from high frequency and a peak well above $\alpha = 0.1$. I suspect the cause is the stable stratification of the layer?

Second, the shape of the cospectrum over the entire region significantly contributing to the heat flux can be quantified to some extent in a schematic way at least in the approximately lower half of the CBL, as shown in the attached page. I have also included the entire reference where it is found. In the upper half of the CBL, the cospectrum can have negative contributions due to entrainment from the overlying free troposphere that become progressively more dominant with height, so the cospectrum becomes more messy and less well defined. For that reason, a completely general normalized formulation of w'T' cospectrum throughout the CBL seems not to have been published.

This is very helpful; I studied that chapter and learned a lot. I added a reference where I have discussed possible contributions from higher frequencies than are measured. and a reference to the chapter you provided, regarding expected errors in the measurement. I hope the section added to the end of (new) Sect. 4.2 represents this result properly. Are you suggesting I do something else? I have tried to maintain a focus on the temperature measurement and how it should be corrected, not on the nature of the cospectrum or the flux itself, but if there is something wrong with how I have presented that please help me fix it. The paper focuses on the temperature sensor, not the measurement of sensible-heat itself, so it seems to me that extensive discussion of the expected

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shape and other aspects of the boundary-layer structure would just distract from that, especially because the measured normalized cospectrum doesn't conform to that expected shape very well:
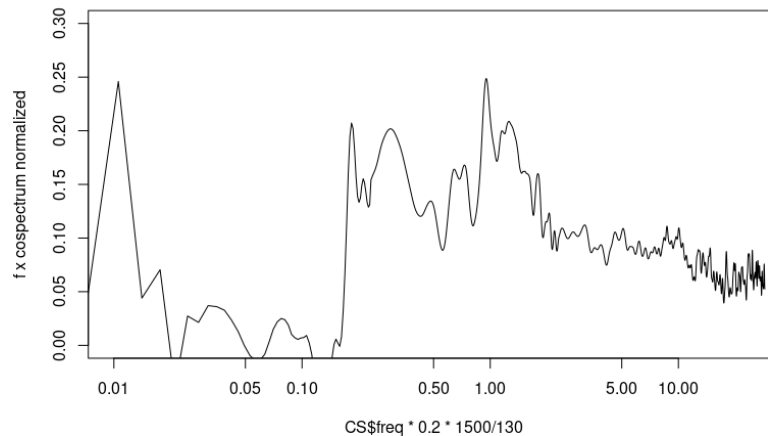
The peak is at higher frequency than expected and high frequencies contribute more than expected.

9. As I remember, at one time we (RAF) did correct for the limited response of the Rosemount unheated probe using a filtering scheme devised by Bob Gabel, who was a consultant for us many year ago. He was a professor at (I think) CU-Denver at the time, and he developed several filtering schemes for RAF and wrote a report (or reports?) on what he did. I don't seem to have an accessible copy, although I may still have it filed away somewhere. Possibly Dick Friesen would have a copy. I'm sure he remembers what Bob Gabel did for us.

I searched for this but didn't come up with anything. Was he trying to correct the measurement or the dynamic-heating term?

10. There is a significant correlation between the wind component fluctuations along the mean wind direction and temperature fluctuations in the atmospheric surface layer; that is <u'T'> is significant, and the ratio of (horizontal heat flux)/(vertical heat flux) is positive for unstable conditions and negative for stable conditions. This is well-documented in a paper by Wyngaard, Cote, and Izumi JAS 1971. Is it relevant above the surface layer? Perhaps not. Does this affect your analysis? I'm not sure.

I did observe that correlation and worried about it because it could arise from incorrect subtraction of dynamic-heating, which would produce a false correlation. Also, maybe a contribution to the flux would arise because fluctuations in u contribute, as we calculate w, to fluctuations in w, but that seems to be a real contribution to the flux.

11. On p. 13 you mention that Stickney et al. found a Mach number dependency for \tau_1 of M^{-0.6}. In my Buffalo tech note I used the Collis & Williams (JFM, 1956) analysis for Reynolds number Re 44 < \tau_1 < 140 (4-15 on p. 27) and estimated \tau_1 from the relation: dT/dt = 4.74x10^-4 U^(1/2) d^(-3/2) (T-T) = 1/\tau(T T_t). Thus, I assumed that \tau_1 had a velocity dependence of power -0.5. I wonder if Stickney used any published Re dependency documentation

or whether this was purely empirical? I don't have the Collis & Williams paper (nor the Stickney et al. report), but I found a more recent reference that seems to update these results and gives formulations for a wider range of Re number. Thus, it seems possible to base this Mach number dependency on published data, and not rely on the Rosemount report.

What is the recent reference to which you refer? It would be very useful to update this. Stickney didn't give a power law and didn't present a theoretical relationship, but showed wind-tunnel measurements; that is just my rough fit to the data in his plot. Invertarity, looking at the same plot, cited a different slope ($Z^{-0.688}$), so it would be good to do this better. The Collis and Williams paper and one by Khan et al (2005) both indicate that the Nusselt number should vary as $\mathrm{Re}^{0.5}$ for a cylindrical wire, suggesting that τ_1 should vary as $\mathrm{Re}^{-0.5}$ as you suggest. Stickney et al. argued for a dependence on $Z = \frac{\rho}{\rho_0}M$ and presented data indicating a slope of about -0.68 (from a more careful measuring from his figure; I had -0.6 before). The uncertainty range in that figure is large, but a slope of -0.5 seems inconsistent with their figure:
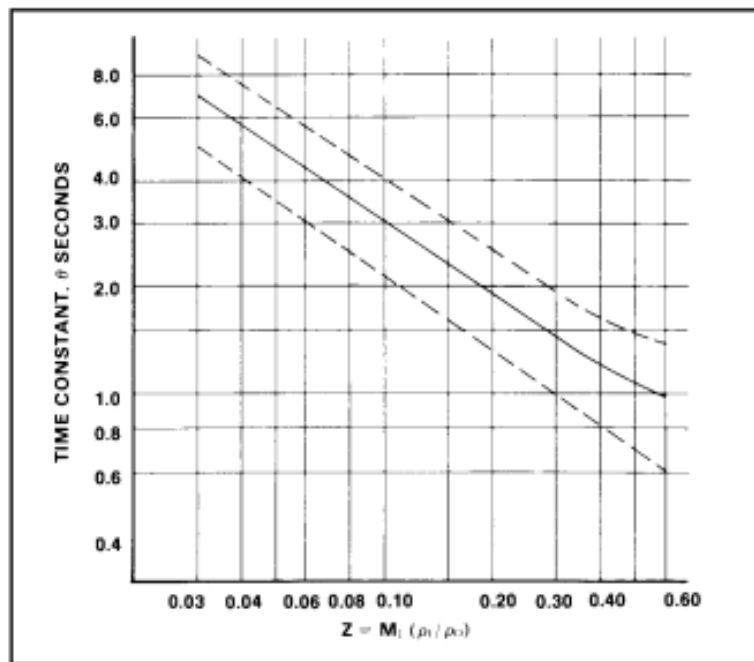

Figure 12: Wind Tunnel Data; Time Constants for Model 102 Sensors

Here is my attempt to interpret this in terms of Reynolds number. It would seem appropriate to use Re at the sensor wire rather than the free-stream Re – i.e., use the airspeed, air density, and dynamic viscosity μ evaluated for recovery conditions: $V_p = \sqrt{1-\alpha_r}V$, $p_p = p+q \approx p(1+M^2/5)^{-2/7}$, $\rho_p = p_p/(R(273.15+T_r))$, leading to

$$\mathrm{Re}^* = \rho_p V_p d/\mu$$

where the temperature dependence of μ is approximately proportional to $\sqrt{273.15+T_r}$. Because of this, the Stickney et al. parameter Z has almost the same temperature dependence, because $Z = M\rho/\rho_0 = V\rho/(\rho_o\sqrt{\gamma R T_K})$ with $T_k = T+273.15$.

It is then possible to compare the ratios of Re^* and Z for different flight conditions. For $Z = 0.3$, Re^* is about 75, so it is useful to normalize both to those values and then consider how the time constant would vary with either $(Z/0.3)^{-0.68}$ or $(Re^*/75)^{-0.5}$. The best test I could find was based on DC3 flight 11, a flight segment at about 11.5 km (21:00:00 to 22:00:00). My estimate of τ_1 from that flight leg was $\tau_1 = 0.037$ s. Z predicts 0.036 and Re^* about 0.033, so the results aren't very different but they seem closer to the Stickney representation. Therefore, it seems better to stay with the Stickney representation, which was based on wind-tunnel measurements with the sensor rather than idealized-cylinder results. (It was interesting that there did not seem to be a similar variation in τ_2; if anything, the results favored a smaller value. Using the expected dependence for the sensor is probably not appropriate for the support, so maybe the best representation is to leave τ_2 unchanged with Z. I have changed to that description in the draft text.)

minor comment: I can't see the carat in the definition given on p. 23.

Thanks, I think I have fixed that.