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Additional reviewer comment (RC3)

Stephen Burt

Author comment on "Measurements of natural airflow within a Stevenson screen and its influence on air temperature and humidity records" by Stephen Burt, Geosci. Instrum. Method. Data Syst. Discuss., <https://doi.org/10.5194/gi-2021-31-AC4>, 2022

Dr Stephanie Bell, NPL, provided her reviewer comments directly to me on 6 Feb 2022 as she was unable to post them on the Copernicus website. The comments are included below with my line-by-line responses indicated by bullet points.

Overall

This is an interesting and important piece of work. I have made some comments intended to help clarity and impact for the readership.

As I read the manuscript, I wondered whether this work would be better presented as two papers: one focusing on the experimental work, and one discussing the influences of airflow on temperature error and of temperature error on wet-bulb temperature (i.e. mostly section 4.3)?

- It did in fact start off that way, but in preparing the material it appeared that presenting the first topic on its own would be likely to invite the question 'Tell me why this is important?', while similarly the second question could be parried by 'But when and why would this occur?', and therefore that such a division would be incomplete.

Title

I am not sure the title as it stands is enough to convey the full significance of the work. If the manuscript remains one item, the existing title "Measurements of natural airflow within a Stevenson screen" might usefully continue ", and the impact on airflow-sensitive measurements of temperature" or something similar. (This also points to how the manuscript might be divided.)

- A good suggestion. I have revised the paper's title along the lines suggested, but I would prefer not to divide it for the reasons given above.

Abstract

This is clear, and it conveys the broader context and implications of the measured values of airflow.

Manuscript details

Line 58: Although the text says the anemometer was visually centred in the Stevenson screen, it appears visibly off-centre in Figure 2.

- In fact the sensor was positioned as close to the centre of the screen volume as possible, but I agree Fig 2 makes it appear slightly off-centre. The difference is only a few centimetres, however. I have amended the text appropriately.

Line 64: It is good to see the statement about calibration. However, it would be good to know how current that calibration was. If not recent, it would also be desirable to note what the expected level of calibration drift might have been (if any) for this anemometer type. Was that uncertainty 2 % of reading or a fixed uncertainty of 2 % of full scale? Was there any lower limit of range where the uncertainty rose above 2 %? In addition, it would be desirable to give the coverage probability and coverage factor for the 2 %. Finally, it would be good to know whether the uncertainty in using the anemometer is predominantly only that of calibration, or somewhat larger, as is the case in many types of measurement. Overall, the resulting uncertainty in the rather small windspeeds measured would depend on these things.

- Reliable and accurate calibration is always important – of course – but calibration uncertainty simply isn't the main factor here. The conclusions set out in the paper are insensitive to even fairly large uncertainties in the sensor's low speed calibration: even if the calibration at 0.2 m s^{-1} (the mean in-screen speed logged during this experiment) were out by +20%, which is 10x manufacturer spec, this would change the ratio of interior:exterior wind speed only slightly (from 10% to 12% for 2 m, and similarly from 7% to 9% for 10 m). While more uncertainty attaches to the lowest speeds, this is largely irrelevant to the outcome as the stopping speed of the external Vector anemometers meant that reliable comparisons below U_2 or $U_{10} < 0.4\text{-}0.5 \text{ m s}^{-1}$ could not be obtained in any case. But the point is a fair one, and accordingly I have added an extra note in the paper to set out details of the calibration of both sets of instruments.

Line 135: among the reasons for selecting the cup anemometer, was it also because they were available and maintained?

- Yes

Was there, or would there be an opportunity to compare the two anemometers directly at relevant airspeeds, as a confirmation of consistency between the two?

- The 2 m and 10 m anemometers in the observatory are operational instruments and could not be easily removed for comparison without disrupting other programmes. However, the Sonic anemometer in use here had previously been compared side-by-side with an identical pattern of Vector Instruments anemometer of known calibration over a 4 week period for exactly this purpose, and again afterwards for several months, and the two instruments agreed within 2% over a wide range of observed speeds, except at low speeds owing to the 0.3 m s^{-1} stopping speed of the Vector anemometer.

Line 158: U_2 (at 2m height) is "not shown", but this feels a little disappointing, given that a relationship for this is derived at line 165. (Also, should these questions be numbered and referred to from the text?)

- The scatterplot for U_2 speeds was prepared but not included as it seemed unnecessary. However, at the referee's suggestion I have included it in the same format as Fig. 3.

Figure 4 caption appears to be missing.

- This appears to be a glitch in the publisher's PDF creation as the caption is included in my MS. I will check it appears in the updated file.

For the graphs in Figs 4, 5 and 7, the title above the graph can be removed.

- Agreed, these are for my reference only and would be annotated for removal at proof stage if not before.

Line 187: where the text says "lower than for winds ≥ 1 m/s" does this really mean "lower than then the data would suggest"?

- I have reworded to '... the ratio of U_{screen} to U_2 and U_{10} for wind speeds < 1 m s⁻¹ is probably little different to that for winds ≥ 1 m s⁻¹.'

Line 207: all observations or all means?

- I have reworded to '... all 2423 hourly means'.

Fig 7: It is a little hard to see how the percentages of winds relate to the values in table 1, especially for values in the range 0 to 0.05 m/s.

- Perhaps I misunderstand the point being made here, but Table 1 refers to 10 m wind speed classes (for which the lowest bin is 0-0.5 m s⁻¹), whereas Fig 7 relates to in-screen ventilation speeds, with lower bin 0-0.05 m s⁻¹.

Is the mode (most common value) different from the mean? This would be relevant to report. (Perhaps consider whether this is relevant to mention in the abstract too?)

- All are positively skewed, as would be expected with a distribution bounded by zero:
- In-screen ventilation: mean 0.20 m s⁻¹ (Table 1), mode bin 0.15-0.20 m s⁻¹ (Fig 7), median 0.18 m s⁻¹ (from original dataset); distribution also given on Fig. 7 in original paper (now Fig. 8)
- U_2 : mean 1.96 m s⁻¹ (Table 1), mode bin 1.51-2.50 m s⁻¹ (original data), median 1.75 m s⁻¹ (from original dataset)
- U_{10} : mean 2.80 m s⁻¹ (Table 1), mode bin 1.51-2.50 m s⁻¹ (Table 1), median 2.50 m s⁻¹ (from original dataset)
- I have added median values to Table 1 and Fig 8.

Line 236: what does "preferential orientation of eddies mean" (or would most readers not need that explained)? If all air movement inside the screen is turbulent, does the mean that the anemometer measures "net wind speed" and that eddy windspeeds on a microscale might be greater, i.e. the anemometer does not have fine spatial resolution? If so, might it underestimate the micro-scale windspeeds?

- A definitive answer to this question would require a greater density of high-resolution (> 1 Hz) small sensors operating within a wind tunnel environment, coupled with CFD modelling; it is outside the scope of the paper.

Line 256: References say that warming "occasionally" amounts to 2-3 K, but it would be helpful to mention what level of warming is thought to occur "commonly".

- Half a degree is not uncommon. I have added this comment to the manuscript.

Line 279; "without any cladding" ...? Perhaps "uncovered"

- Agreed

Line 281: It is not clear why 3063 is the time required to achieve 95 % of a step change. Is there a further explanation?

- This follows from response time theory; I have added a reference

Table 2: Perhaps say here, or earlier, what is the relevance of sensor in a dry wick?

- Added a sentence to explain that the response time of the 'sleeved' sensor is compared with an otherwise identical and unsleeved sensor in the same environment – i.e. the difference in response time is down to the insulating effects of the wick/sleeve.

Line 304: "an aspirated wet bulb – if such a device could be if such a device could be developed ..." these exist and are in widespread use - for example Assmann psychrometers and many others, and even a historic design by the WMO.

- Agreed, but an Assmann psychrometer is not suitable for continuous automatic use. The issue lies not with sensors or methods of ventilation, but entirely in maintaining a constant and reliable supply of water to the wick in all circumstances (high and low humidity, and in particular temperatures below freezing, and maintaining a clean wick).

The flaws of wet-bulb measurement leave an opportunity to mention the advantages of electronic relative humidity sensors. It seems rather an omission not to.

- Agreed, and additional text has been included.

The term "wicked" is problematic as it is open to reading with another meaning (as in "wicked witch"). Once seen, this is hard to unsee and could distract readers. The WMO No8 CIMO Guide avoids this word, in favour of other terms such as wet bulb, wet-bulb sleeve, and similar.

- Ha ha! Agreed. I hadn't read it that way, but now I can't 'unsee' the connection. Amended.

Many other sources of error affect wet- and dry-bulb hygrometers – it seems an omission not to mention them, and their magnitudes, for perspective.

- It is easy to dilute the focus of the paper by delving into other issues, but I will include a short note to this effect.

Fig 8: it is not completely clear whether these are all calculated values, or not. A bit hard to follow – maybe start this description with an overview to orient the reader?

- These are all calculated values. I will reword to clarify as suggested.

Line 336 a constant relative humidity during a 5 K fall in temperature seems slightly unlikely, and this distracts slightly from the point being made.

- Agreed to a point, but at 1 K the differences are of course less obvious. It is a theoretical construct to show the point being made, but in temperate latitudes falls in temperature of 5 K in a few minutes are not uncommon, and are not uncommonly accompanied by *falls* in relative humidity (dew point falling faster than air temperature) – particularly at sharp frontal passages or in thunderstorm downdraught situations.

The "unit symbol" for "percent relative humidity" is weakly standardised and it is accepted

to use “%” or “%rh” is also widely used. In either case, there is a space between the number and the symbol.

- I have compromised and am happy to use %RH; the paper has been amended accordingly with definition at first usage

340 Is the “spot mean” a rolling mean (of 6 values here?). Is “spot” a recognised term?

- ‘Spot’ in datalogger terms is usually taken to mean ‘near-instantaneous sample’, but I have amended Table 3 wording to make this clearer.’

4.3.6 What about mentioning the accepted published values of the psychrometer coefficient (still air and aspirated with moving air \square 1 m/s) for context? A key point that the case in point is between these regimes. The values by Harrison and wood remain of interest of course and there would be scope for further study of this.

- Accepted values of the psychrometer coefficient are given in 4.3.6 and in Fig. 10 (from Harrison and Wood); I’m not sure I understand what other values are being suggested?

Table 4: What does X designate?

- Where the calculation using the parameters given in Table 4 generates an unrealistic RH (i.e. below 0%, for which dew point is not defined). This should have been made clearer in the table caption – now amended to do so

381 $A=1.1$ is very far from the accepted published value for still air.

- I would question whether there really is an accepted value of A for still air. However, 1.1 is derived from Harrison & Wood (my Fig 10), while Harrison (2014), Chapter 6, Fig 6.18 suggests values for 0 m s^{-1} between 1.0 and 1.3. Fig 10 (now Fig 11) has been updated to reflect this. I’m not quite sure follow the referee’s point here.