Summary of revisions:

Figure 2 was revised, and redone, replotted, in part as suggested by the editor to increase the symbol size. The caption was changed accordingly, for a typo in the acceptable ranges of angles used. No significant changes to the results or discussion accrue from these revisions.

Table 1 was revised as suggested by multiple reviewer and community comments.

We added a sentence (lines 88,89) in response to reviewer and community comments, concerning the effects on our corrections if the assumption of 1.25 for the ratio were to be wrong and different ratio values were to be found more appropriate. We also added a reference to the 1.25 value (Thurtell et al., 1970) who reported a 1.25 value using a pressure sphere anemometer (lines 79,80), which had a design essentially not subject to transducer shadowing.

In response to reviewer comments, Table 3 was modified to include more standard deviations to quantify one measure of uncertainty when multiple sonics were used to calculate C_w, and minor revisions were made to the C_w values for the RMYoung to the table, figures, and in the text.

We added some sentences interspersed in the discussion and summary, to clarify some results and comparisons with literature results, that became apparent when revising and double checking references—detailed immediately below.

Line 149-150—We clarified the data gathering model for the IRGAson's and CSAT3a

Line 178 – The zeta limits were corrected, the original manuscript had a typo for the values.

Line 208 – Extra space typo deleted

Line 215 – Corrected a typo in the lower value of the correction factors

Figure 3 caption typo corrected to upper case "s"

Line 264 and following – A minor change in the correction factor for the RM Young was discovered in the text (was 1.23 in the original manuscript, but should have been 1.28), and in addition it was in the calculations using the RM Young corrections for the intersonic comparisons. This resulted in small changes in the regression coefficients, the text discussing the regressions, the points plotted in Figures 4 and 5, and the corresponding figure captions.

Line 338-339—" Horst et al. (2015) reported a σ_{wm}/u^* value of 1.17 for uncorrected CSAT3 data, which using our method would yield a C_w of 1.14, well within our range of CSAT3 C_w results."

Line 339-341 "They also reported a similar σ_{wm}/u^* value (1.16) for the shadow correction that can be implemented in models like the CSAT3a or IRGAson, so it appears that implementing the built-in shadow correction option would not affect our method or results."

Line 347 – 23% typo corrected to 28%

Line 352-355 – "We present a method to estimate vertical velocity and flux corrections for sonic anemometers, using commonly reported turbulent statistics from a single anemometer, instead of comparisons that require the test anemometer and reference orthogonal sonic anemometers to be side by side, laboratory or numerical methods, or methods requiring raw high frequency data"

Line 367 – 1.23 typo corrected to 1.28

Line 374-376 – "This form of analysis could be tested in the future for usage over taller roughness landscapes, such as crops, orchards and forests, given enough fetch for measurement heights over the roughness layer."

Line 387-389 – "…and Dr. Nicolas Jorgensen-Bambach from the United States Department of Agriculture Agricultural Research Service in Davis CA for his informal questions regarding the manuscript after the formal comment period closed."

Below are the posted responses with small edits in some cases to reflect how we have revised the manuscript. Highlighted text in green correspond to comments resulting in revisions made in the manuscript, while orange color font indicates posted responses that did not result in manuscript revisions, because we felt our responses alone were adequate to address the reviewer and community comments.

We thank the reviewer for their helpful comments, our responses are in bold type.

Reviewer 1: The manuscript presents an interesting investigation to derive a simple, yet effective, method to correct instrumental errors from sonic anemometers in the measurement of the vertical velocity component. The manuscript is well-structured and written, but I have some major queries related to the methodology and to which extent this can be applied. Please find them below.

Table 1: It is worth reporting the method used to derive the correction factor in the notes column, dividing sensor-to-sensor comparisons from sensors-to-model, wind tunnel from outdoor experiments, and specifying which conditions have led to different estimations of the correcting factor for the same anemometer.

> **We can add the methods used in the revision, but we cannot add which conditions led to different estimations of the correcting factors since this is unknown.**

The determination of the near-neutral stability range should account also for the horizontal wind components and not only the vertical ones. Also, if the horizontal wind components are not distorted as claimed, they should identify the near-neutral range more precisely. I was also expecting the near-neutral stability range across $z/L=0$, like having $|L|<500$ or $|z/L|<0.05$ as it is mostly observed in the literature, but that seems not the case. Is this a characteristic of the sites? Does it affect your correction method or limit the applicability to this specific location?

> **Thanks for these comments. We chose to focus on the vertical velocity, because that has the feature of being predicted to rise at a 1/3 power as local free convection is reached, providing a clear contrast to transition to near neutral conditions, and to provide a screening for near-neutrality. Similarly, for stable conditions, we expected a more direct effect on the vertical velocity, for the screening for near neutral. We also note that the vertical velocity would be the most affected by sonic anemometer design form factors, for most designs there are major mounting structures that interfere more with vertical flows, while the horizontal flows are somewhat less obstructed and the structure is more open for those flows. Most sonic anemometer models have also undergone some level of wind tunnel testing for characterization and correction of wind velocity measurement errors, but because this work is typically performed in laminar flow with the anemometer oriented vertically, it does not account for errors in the vertical velocity. Furthermore, horizontal components like σ_u/u^* tend to have more scatter in scaling to u* (Hicks 1981; Lee and Meyers 2023). When we did some preliminary checks, the scatter in σ_u/u^* was somewhat**

larger at near neutral stability, when compared to σ_w/u^*, in a sample of HATS data.

We specifically used the range for which the $\sigma_w/u^*$was apparently constant. We generally excluded z/L >0 because most of the different sonics showed an apparent shift to a higher σ_w/u^* for near neutral, z/L >0 but close to 0, and this was unexpected. This shift for stable conditions is consistent with Lee and Meyers (2023)'s analysis. We advise using a flexible, empirical definition of near-neutral, based on the objective of any particular study, which is in our case, based on σ_w/u^*values. We did not use the L > |500| (note our sign change, we assume this is what the reviewer is specifying) for the reasons above also. We note, if we had used the L > |500| value, the z/L thresholds would have ranged from 0.002 to 0.014 for the heights of the experiments, so our thresholds were within this range. We believe previous work's definitions of near-neutrality are closely related to the research piece's objectives for identifying near-neutrality, as exact stability limits are a little subjective, and therefore not universal. We don't believe this correction method is limited to a specific location, as for several of the sensors, for example, the CSAT3 family of sonics, they were in experiments at different sites ranging from Southern to Northern California (Kettleman CA for HATS; Davis CA; Roberts Island,CA; Courtland CA), and at different times (differing by up to approximately a quarter century), but yield similar results, and if all sensors we used are considered, in two different continents (North America and Europe).

Given the different site's data are collected from, did you apply a single pre-processing technique (like for despiking, computation of fluctuations and covariances, etc.)? Does this technique involve EddyPro like in the case of the CSAT+IRGAson eddy covariance station? Do you expect the preprocessing differences, if any, can be responsible for the success or unsuccess of the correction method? What about the different sampling rates?

We did not apply a single pre-processing technique, but relied on the different field campaign's processed data from their varied data analysis techniques. We rely on a similar philosophy as Merry and

Panofsky (1976) for examining the σ_w/u^* ratio for multiple field sites and datasets without using a common turbulence data analysis method for all sites. It is from this paper and the others cited in our manuscript, also a reference we hadn't yet cited, Thurtell et al. 1970, that we obtained the "ideal value" of 1.25. For the RM Young and CSAT3's at the Delta sites, and the CSAT3's at the Davis site, custom UC Davis turbulence processing was done, with the main QA/QC described in the paper. The custom UC Davis processing was compared with EddyPro processing for some other datasets and no significant differences were found. Therefore we don't believe the differing processing had any influence on the corrections.

No despiking was done. Only the CSAT3a and IRGAson involved the EddyPro technique. Different sampling rates, as noted in the manuscript's citation to a study on sampling rates would generally result in slightly different uncertainty in the averaging of the statistical properties, but not any mean bias which should approximate the central limit theorem, if applied under the limitations for assessing such statistical properties (Bosveld and Beljaars 2001).

For the LIAISE data, we used a processing code similar to that of the custom UC Davis processing methodology. This method yielded similar results to the processing done by the UK Met Office. Like the other sites, we did not despike the data before processing. We did a double rotation method to correct the velocity field.

One of the assumptions of the similarity theory requires the flow to be in a steady state, at least statistically. How did you ensure that? Can unsteadiness be an additional source of error for the vertical velocity?

We did not attempt to obtain data from exactly steady state flow, but we point out that field turbulence data is rarely steady state, although similarity equations are frequently applied to such non-steady state data. For example, Moncrief et al. (2005) notes, "These motions are part of a continuous spectrum of atmospheric

fluctuations with time scales from seconds to seasons and length scales from meters to kilometers and beyond. On any practical time scale these series are intrinsically non-stationary." We don't believe that some degree of unsteady flow created any substantial additional error for vertical velocity, although we do believe such unsteadiness would potentially increase the uncertainty in applying relatively steady state laboratory flow derived sonic anemometer corrections to sonic anemometer data gathered in the field.

Because many of the datasets used in this study are ½ hour turbulent variable summaries carried out by different researchers, a unified screening for steady state conditions was not apparent. We note that there isn't a single agreed-upon method for quantifying the degree of non stationarity. While Foken (2016, p175) suggests a partitioning based screening technique, there are several formal statistical methods such as the Augmented Dickey-Fuller test, the Kwiatkowski-Phillips-Schmidt-Shin test, or the Check Autocorrelation Function. They involve subjectively deciding probability or other thresholds, that are not universally agreed upon for field turbulence data.

We note also that the assumption of the 1.25 value for the ratio of the σ_w/u^* was obtained from a variety of experiments, including those summarized in Merry and Panofsky (1976), for which the degree of non-stationarity or related data processing the turbulent velocity data were not clear (Haugen et al. 1971; Wyngaard et al. 1971; Panofsky 1960).

Nevertheless, we did a quick of check of some of the HATS CSAT3 data, filtering the data for the same near neutrality definition used in the preprint draft, and used a check on the change in σ_w with time (a weak stationarity check). Data greater than a 0.67% change/min were omitted and the ratio compared to the ratio from all data. The difference in the σ_w/u^* ratio was less than 0.6%, with a resulting loss of around 70% of the near neutral data. We believe it is not necessary to carry out the stationarity/steady state filter on all the

varied datasets, based on this check that showed only a minor difference coupled with a potential increase in statistical uncertainty because of a substantial decrease in available data.

Given the results in Fig. 5, how robust is your correction to different averaging periods?

We believe the Fig. 5 results are related to the correlation of horizontal and vertical fluctuation distortions or internal data processing for the two different sonic anemometer types, so the correction method doesn't fully make the corrected u* from the two different types match perfectly, compared to the sensible heat, for the same ½ hour datasets. We're not sure how the results of Fig. 5 point to issues of different averaging periods. Generally, we do not expect a great effect of different averaging periods, because the near-neutral flow should be driven by mechanical shear production, separate from buoyant production related large scale boundary layer eddies that are more dominating under more unstable conditions and may require longer averaging times

References:

Bosveld F.C. and Beljaars, A.C.M. 2001. The impact of sampling rate of eddy-covariance flux estmates. Agricult. Forest Meteorol. 109:39-45.

Foken, T. 2016. Micrometeorology. 362pp. Springer Verlag, Berlin, Germany.

Hicks, B.B. 1981. An examination of turbulent statistics in the surface boundary layer. Boundary-Layer Meteorol. 21:389-402.

Lee, T.R. and Meyers, T. 2023. New parameterizations of turbulence statistics for the atmospheric surface layer. Monthly Weather Rev. DOI:10.1175/MWR-D-22-0071.1

Moncrieff, J. Clement, R, Finnigan, J., and Meyers, T. 2005. Averaging, detrending and filtering of eddy-covariance time series. p 7-32. Handbook of Micrometeorology. Eds. Lee, X., Massman, W., Law, B., Kluwer Publishers, Dordrect, the Netherlands.

Panofsky, H. A. and McCormick, R.A. 1960. The spectrum of vertical velocity near the surface. Quart. J. Roy. Meteorol. Soc. 86:495-503.

Thurtell, G.W., Tanner, C.B., and Wesely, M.W. 1970. Three-dimension pressure-sphere anemometer system. J. Applied Meteorol. 9:379-385.

Reviewer 2:

General comments: As other recent papers have tried to quantify the bias in the vertical wind component of non-orthogonal sonic anemometer configurations this paper, using empirical evidence on the nature and structure of turbulence statistics (i.e. **σ**w/u*) demonstrates how correction factors for various sonic anemometer types can be derived. Certain assumptions have to be made regarding representativeness and homogeneity of the landcover where these observations were obtained, as well as the normal suite of conditions that are satisfied in order to obtain the results presented here.

We thank the reviewer for this nice summary of the paper, and some of its limitations regarding assumptions and applicability.

Specific Comments:

In the filtering of the ratio, were there any mean wind speed thresholds or filters that were used in the determination of the correction factors? Typically, the lower the wind speed, the more variable the wind direction can become and the determination of u* can become more noisy.

We thank the reviewer for this point. We examined the effect of different wind speed thresholds, and although the scatter of the σ_w/u^* is indeed greater for lower wind speeds, but also that for wind speed thresholds up to around 2 m s^{-1} the median (that we used in our analysis) is relatively constant. Other filters were noted in the manuscript, such as for the HATS data where two heights were used for multiple sonics, a check for an approximately constant u*.

Is there some sense of the uncertainty of 1.25 as the ratio? I have seen reported values ranging from 1.21 to 1.3 in various papers. What is the sensitivity of the magnitude of this ratio in the determination of the correction factors? Also, once the correction factor was determined, this should then be used to correct the raw value of u*, which of course will affect the determination of the stability parameter z/L since L is a function of u*3. For example, a 10% error in u* will affect the magnitude of the Monin-Obukhov Length by over 30%.

For the value we took of 1.25, we used the consensus of several references, such as Merry and Panofsky (1976) and references cited in the paper and above, but we also noticed a range of values in the literature (Merry and Panofsky, 1976 report around a 10% uncertainty). The correction factors would become greater for assumed "correct" σ_w/u * values greater than 1.25 (such as assumed 1.3 would result in a correction factor of ~8% greater), and less for values less than 1.25 (such as 1.21 would result in a correction factor of ~6% lower).

We added in lines 88-90 the following statement in the revision:

"We also note that if the true σ_w/u* value were to be assumed equal to 1.2 or 1.3 instead of 1.25, the correction factors we report would need adjustment to be approximately 8% lower or 8% higher, respectively."

The reviewer is right that the u* value can be corrected with the correction factor itself, that is how the method is developed, as shown in the equations that are used to determine the correction factor. This could have an effect on the stability factor z/L, as mentioned by the reviewer, but we note that since we're screening for near neutral conditions, the correction on z/L is not a serious issue as we're using the σ_w/u * values to screen for near-neutrality and not a specific relationship to z/L such as a phi function. We're already choosing the low magnitude near-neutral z/L (uncorrected) values to obtain a minimum approximately constant value of σ_w/u *, and since we could adjust the z/L (corrected) values to still show the minimum value at near-neutrality, the correction factor would still be the same. Also, we note that the correction factor is to the power of 3/2 for u*, and since z/L also has the correction factor appear in the numerator through the kinematic sensible heat wT, z/L is changed by $C_w^{-1/2}$. For example, a 10% increment of vertical velocity (C_w =1.1) would decrease the magnitude of z/L by 5%. Our threshold of -0.10 would change to around -0.095 in this case, which would be

negligible from inspection of the ratio of sigma w/u* in Figure 1, so we did not iteratively change the near neutrality screen and redo the calculations.

In Table 3, it would be very helpful to add statistical uncertainty values or confidence limits for the various correction factors.

This is a good suggestion, we will implement this in the revision, when we have multiple sonic anemometers of the same type, to arrive at standard deviations for the calculated correction factors, to provide one form of uncertainty measure.

Community Comment 1: Gerhard Peters

In Table 1 there are 4 entries on Metek sonics (USA-1 and uSonic-3) with contradicting correction factors. Small correction factors were found by Mauder and Zeeman, 2017 (M), and Beyrich et al. 2002 (B). Large correction factors were allegedly found by Horst et al., 2015 (H), and Pena et al., 2019 (P). While M and B evaluated the standard products provided by the sonic, the last two citations are misleading:

We are sorry if we were misunderstood in our draft manuscript Table. We do not believe the citations are misleading, but misunderstood by the commenter. Nevertheless, to reduce the misunderstanding, we will be clarifying the cited numbers with a brief explanation within the table.

1. H investigated the implemented flow distortion algorithm theoretically and confirmed it basically. The cited numbers in Table 1 (22 – 32%) are not "vertical velocity correction factors" but the expected maximum shadow for the case of flow along a sound path. This is quite a different object.

   We'll be editing the table to clarify the number. For wind velocities along the maximum shadow, the number cited would be what the cited publication reported.

2. P examined the flow distortion algorithm implemented in USA-1 sonics in field experiments with the result of satisfying quality. The cited number is related to the impact of the

algorithm. By the way, they used similar symmetry properties of turbulence parameters as in this paper, in a well-defined restricted regime, to check the soundness of the standard sonic outputs.

We'll clarify in the text the cited number, as noted above. However, we do not understand several issues in the commenter paragraph, and they would need clarification. 1) What is "satisfying quality?"

We also do not fully understand the statement, "…By the way, they used similar symmetry properties of turbulence parameters as in this paper, in a well-defined restricted regime, to check the soundness of the standard sonic outputs." The Pena et al. paper is not closely similar to our method, although the wording of the comment implies strong analogies to our method. This is discussed further in the discussion response to one of the co-authors of the Pena et al. paper. We also do not understand what is specifically meant by, "in a well-defined restricted regime."

We point out that this comment has no bearing on the methodology introduced in this paper, but rather are just part of the literature review concerning what has been done in the past.

2

A flow distortion correction should be conceptually superior to a gross correction of sigma_w using turbulence symmetry properties, because the applicability of these symmetries depend on quite restrictive assumptions.

The fundamental background leading to this statement is not clear to us. We disagree with the concept that somehow there is a "flow distortion correction" that is "conceptually superior." The literature review, and the methodology in this paper, shows that in all cases quantifying the flow distortion and it's effects on fluxes involve multiple assumptions for practical application to corrections for field measurements. We don't see sufficient theoretical justification to claim "conceptually" superiority of previous attempts to assess or model corrections, because of the high frequency unsteadiness in velocity (direction and magnitude), and dynamically varying scales (temporal and spatial), and lack of a

universally accepted primary standard for sonic anemometers. These methods include numerical, analytical, wind tunnel or other lab flows, each with many sets of restrictive assumptions to apply to real world usage of sonic anemometers. For example, a steady state (at least assessed at the larger scale turbulent sizes typical of atmospheric turbulence) wind tunnel (or CFD simulation) study for different attack angles is not obviously accurately applicable to rapidly changing velocity vector change caused flow distortion as the velocity temporally varies from less than to greater than the specific attack angle considered in the laboratory study. Fluid mechanics theory and observations for transient boundary layers clearly show differences from steady state boundary layers, appropriate to considerations of some of the driving processes involved in flow distortion and shadowing. In addition, steady state studies have a limited number of angles and speeds that can be considered, and therefore angular correction factors are interpolated between the tested values.

We can't address comments on "turbulent symmetry properties, " because we don't understand the comment's meaning when invoking the term "symmetry." Similarly, we are not sure what exactly is being referred to in the statement, "...these symmetries depend on quite restrictive assumptions." What are the restrictive assumptions that the commenter is referring to, and how do they differ from the other methods that have been used?

3

The basic assumption that mainly the vertical wind component is subject to transducer shadowing while the horizontal components are more or less unaffected may be challenged. Whether or not this assumption is valid depends on the nature of the wind vector variation.

We thank the commenter for agreeing with multiple locations in the paper. There, we specifically discuss that the horizontal components may be affected and this could influence the accuracy of the corrections factors derived from our method. In fact, we have a substantial discussion of the IRGAson where there appears to be some horizontal component distortions occurring. The assumption of less flow distortion for the

horizontal component is based on the general sonic anemometer design patterns, which tend to leave the horizontal plane more clear of obstacles, while having the a vertical structural pattern to hold the ultrasonic/sonic transducers resulting in more obstruction for vertical velocity components.

For clarity we may consider two extreme mechanisms:

1. The wind inclination varies, while the speed is constant. In this case an angle dependent shadowing can (not necessarily) lead to a truthful variation of the horizontal component while the variation of vertical component is affected.
2. The speed varies, while the inclination is constant. In this case we expect in first order a linear shadow effect on the speed, and hence on the speed variation. In this case all components of the wind vector are affected by the same factor.

The reality is of course a mixture of 1 and 2.

We do not fully understand this characterization of the transducer shadowing and flow distortion effects, nor the extreme paradigm statements. The entire point of sonic anemometer corrections is that the geometric 3-D form factor of the sonic anemometer and their mounting systems results in complicated flow distortions, both upstream and downstream of the sonic anemometer and associated structures, propagating into the sample volume of the sonic anemometers. The two examples in the comment are not obvious in our perspective. Whether the vertical angle varies under constant speed, or the vertical angle is constant, but the speed varies, or some combination, all result in complex effects on the vertical and horizontal air flow, based on the 3-D shapes and sizes changing the flow patterns, with turbulent flows causing even more complexity to the distortion.

4

Multipath sonics were developed in the meantime in order to avoid the issue addressed.

We are uncertain which novel sonic anemometer designs are being referenced here, Is the comment related to the commenter’s company’s “multipath” series of uSonic’s? There will still be physical flow distortions in the uSonic design structure, which still has the concept of sonic paths between emitters and receivers. The feature that for each transmit mode

three receivers are triggered, resulting in 9 wind components, may have been thought to assist in assessing some flow distortion, the form factor is very similar to other manufacturers' sonic anemometers, so similar flow distortion will still occur, and seen in the multipath outputs.

Perhaps a microsodar like sonic anemometer could reduce the distortion issue, by accepting only time of flight reflected signals from some distance away from the microsodar transmitter and receiver assembly, but with a time of flight acceptance interval sufficiently short, resulting in a sampling volume distant from the flow distortion region, but still small enough to be useful in surface layer turbulence scales. However, the authors are not aware of any such off-the-shelf system, their non-existence probably because of the limits of vibrational and other signal to noise issues, perhaps related to the limited reflectivity coefficients from small volumes having small total numbers of particulates or density variation eddies.

Community comment 2_ebba delwik

It is really nice to see a new solid study regarding sonic anemometer accuracy, reviving the important topic of flow distortion corrections.

Thank you!

As a follow-up to the comment by Gerhard Peters, I would like to clarify that we, in our study (Pena et al 2019), suggested a different way to judge whether the sonic anemometer observations were affected by flow distortion/transducer shadowing errors without comparison to other instruments. Our idea was to study the ratio of the power spectral density in the inertial subrange for the observations. Based on theory, this ratio should be 4/3 for the w/u velocity component ratio. By testing the Metek USA1 sonic with and without the manufacturer's correction we found a strong difference; and that, by including the flow distortion correction, the ratio was close to 4/3. We also tested the CSAT sonic anemometer with and without flow distortion corrections and found that the ratios were consistently different from 4/3, which would indicate that the instrument was still in need of a correction. Different from your study, we argued that a disagreement with the 4/3 ratio could not be used directly for correcting the fluxes and variances, since the error likely is split between the horizontal and vertical components.

I think it would be interesting if you elaborated on the relative pros and cons of these methods and also commented on the different interpretations.

Best regards

Ebba Dellwik

We read the Pena et al. paper with great interest. Like our method, it can provide some idea of the correction factor using data from a single sonic, potentially from flow distortion. Data from different stabilities could also be used to examine any possible stability effects on the flow distortion. However, one difference from our method is that because the inertial subrange is used, the distortion from relatively isotropic smaller eddies are relevant for that method. Our method involves all eddy scales relevant to the ½ hour averaging period, including the scales for the main flux-bearing eddies, which could exhibit some anisotropy, and have different distortion characteristics than for smaller eddies. Another difference is featured in how we carried out our analysis, in some cases more than a decade after the actual experiment, on relatively easily available summarized turbulent statistics expressed as half hour averages. To carry out the Pena et al. analysis, raw data would be needed. Our method could potentially be used with the widely available half-hourly Fluxnet and AmeriFlux datasets, for example, without having to invoke FFT or other spectral analysis methods on more cumbersome large datasets (approximately 3 orders of magnitude more data than summarized half hour statistics).

On the issue of horizontal versus vertical velocity distortion, we discuss our own assumptions, and point out that assuming little or no horizontal distortion may not be valid in some cases. Equation 6 does bring in the possibility of horizontal distortion effects on the $\sigma_w/u*$ ratio, and it is possible if we believe the near neutral values of σ_v and σ_u reach some limit based on previous experimental data with orthogonal sonic anemometers, then the same basic method we present here could be used for horizontal velocity corrections, with some iteration because of the correction changing u* as noted in equation 6.

We did not use our method to examine Metek sonic anemometer designs, but invite our Pena et al. colleagues and/or the readers to check out our method on Metek and other designs. We only report on some literature values for some Metek designs. **We will edit the table to reflect the corrected Metek results from Pena et al, it looks like we only included an estimate of correction for the uncorrected Metek data.**