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Atmospheric Measurement Techniques Discussions

Interactive comment on "Note on the application of planar-fit rotation for non-omnidirectional sonic anemometers" *by* M. Li et al.

M. Li et al.

wolfgang.babel@uni-bayreuth.de

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We thank anonymous referee #1 for his comments and criticism, which helps us to improve our manuscript. We start our response with a more detailed discussion of the planar-fit method, and then reply to the specific comments.

General comments

Referee #1 begins his review with a brief summary of our manuscript. He highlights the role of planar-fit as one method amongst others to remove a tilt angle. Further, he disagrees with our method mainly because planar-fit could only properly rotate plane streamlines and therefore should not be used to correct more complicated wind fields

as caused by obstacles or sonic probe elements. According to his first specific comment, he believes, that the planar-fit method is only applicable to "sloping but otherwise flat terrain" and therefore not applicable to any heterogeneous landscape, where this is not fulfilled.

The planar-fit method (PF) has come to the community´s attention since the paper by Wilczak et al. (2001), although it has been developed earlier and was already in use by NCAR (Oncley, pers. comm.). Before that, the double rotation (DR), described e.g. by Kaimal and Finnigan (1994), was predominantly used to remove mean vertical wind components in EC data processing (e.g. Finnigan, 2004). In homogeneous flow conditions, DR is a very efficient method and it can be used for online flux calculations (Rebmann et al., 2012). Especially in complex landscapes and over tall vegetation, however, the vertical wind velocity may not always be zero for 30 min averages, which has to be taken into account (Lee, 1998; Paw U et al., 2000; Finnigan et al., 2003). In such situations, the DR method exhibits several drawbacks: Wilczak et al. (2001) state, that tilt angle estimation with the DR method is more sensitive to errors in mean vertical velocity than the PF method, and therefore introduces random noise to the alongwind momentum flux estimate. The DR method is limited by possible overrotation, which can be caused by electronic offset in vertical velocity measurements but occurs also for low wind speed conditions (Rebmann et al., 2012). Furthermore, the DR method is problematic for budget estimations on seasonal or annual timescale, as it does not provide a consistent reference surface.

The PF method overcame the problems related to the DR method, specifically potential overrotation (Rebmann et al., 2012). Residual vertical velocities can be examined and give insight into the structure of the flow field for a respective period (e.g. Göckede et al., 2008). The most important disadvantage of the PF method is that suitable rotation periods have to be found, which depend on possible seasonal changes in the flow field. Moreover, a new rotation period must be started, whenever the instrument tilt, canopy structure or instrumental offset in vertical velocity changes (Foken et al., 2004; Rebmann et al., 2012). Therefore the PF method is not straightforward to apply for operational use.

In recent years the research focus of eddy-covariance measurements has shifted from homogeneous to more and more heterogeneous and complex landscapes (Baldocchi et al., 2001). At the same time the PF method was introduced, and evolved into the nowadays preferred rotation method. Nevertheless, the DR method is still applied, especially in homogeneous areas over short vegetation, where its problems are less significant. In many cases, however, the terrain structure is too complex to be levelled on a plane. Finnigan et al. (2003) mention a dependence of the rotation angle on wind direction for such cases, which can be realized, e.g., by a sectorwise PF. This has been proposed by Foken et al. (2004) and already adopted, e.g. by Ono et al. (2008); Yuan et al. (2011); Siebicke et al. (2012).

We agree with referee #1, that air flow around instrument structures does not follow a linear slope. In cases of the DAT 600, the disturbed sector is very large, and of the CSAT3, with the wind coming from behind, the flow field is modulated too much to be handled simply by the PF method. It was not our intention to claim otherwise, but we suggested to treat this sectors as low quality data (p.7329, ll10-15). However, in case of the CSAT3 front sector, where only a small wind sector is affected ($\pm 30^\circ$), and the probe supports lay behind the measuring path, a plane may constitute a feasible approximation. There is a large difference in the degree of disturbance for both CSAT3 sectors affected by sensor structure. It is recognized in flux processing to exclude the data from non-omnidirectional sensors for sectors, where the structures shadow the measuring paths (Foken et al., 2004). In contrast, the CSAT3 front sector is usually not considered as disturbed in flux processing. Siebicke and Serafimovich (2007) can confirm a strong effect on wind velocities in the back sector of a CSAT3, but also a weak effect of the front sector in a wind tunnel study. More sophisticated correction terms have been derived in such wind tunnel studies (e.g. van der Molen et al., 2004; Nakai et al., 2006), but it is questionable, whether these results could be transferred to turbu-

lent field conditions (Högström and Smedman, 2004). Nakai and Shimoyama (2012) carefully investigated angle-of-attack corrections for Gill windmaster ultrasonic in the field and conclude that the corrections differ from wind tunnel experiments. Although the angle-of-attack problem has a different perspective, we can transfer their findings to our investigated problem of flow distortion due to instrument structures. From their results, it is not clear, if sensor specific corrections could be derived, which are applicable at any site.

In contrast, we believe, that our approach for the front sector can be deployed more generally and we want to propose it as a feasible approximation. Nevertheless, we see the objections of both referees, that comparisons in the undisturbed sector should be as strict as possible. Therefore, we will generally exclude the front sector of the CSAT3 from the undisturbed sector for all comparative analysis in the revised manuscript, and Table 2 will display both the results including and excluding the front sector.

The focus of our manuscript is to investigate the consequences, when the PF method is applied for all wind directions, which is often the case in practice. We show, that computations of momentum flux are affected, when (routinely) using disturbed data for estimating PF coefficients, and simply recommend not to do so. It is not our intention to provide better correction algorithms for sensor specific flow distortion. The very homogeneous terrain around BJ site (see also specific comments) enables us to investigate the effects of the sensor and the measuring tower only. This study continues the work done in our group on the PF method, namely its relation to data quality control (Foken et al., 2004, 2012), to the measurement footprint (Göckede et al., 2008) and its influence on CO_2 advection estimates (Siebicke et al., 2012).

Specific comments

p.2 L2f: The planar-fit method assumes that the mean streamlines from all wind directions define a single plane. This is true for sloping but otherwise flat terrain. But there are infinitely many "heterogeneous landscapes" where this is not true, and then the *planar-fit method is not appropriate.*

The PF method has several advantages, especially when dealing with heterogeneous landscapes. In more complex situations it could be applied sectorwise, see general comment. On the other hand, strongly distorted flow fields, where large flux divergence exists in several directions, eddy-covariance flux results are questionable, regardless of the rotation method applied (see Finnigan, 2004).

p.2 L8: I disagree that the planar-fit method "replaced" the double-rotation method. The latter is still in widespread use, and for good reasons, because the planar-fit method works properly only where the tilt angle has a sinusoidal dependence on wind direction.

It is true, that the PF method yields larger residual vertical wind velocities, the more the tilt angle deviates from its ideal behaviour. But following the arguments from Lee (1998); Paw U et al. (2000); Finnigan et al. (2003); Finnigan (2004), summarised in Rebmann et al. (2012), the choice of DR for such situations would be even worse. See also general comment. We agree, however, that there still can be reasons to use the DR, Therefore our statement "replaced" is extreme and we will change it accordingly

p.4 L4: "incline $< 2^\circ$ ": it may be useful to provide a topographical map of the surroundings, to see whether topography (which would be the justification for applying a planar fit) is likely to explain any features in Fig. 2.

The terrain around BJ site is extremely flat as shown in Fig. 1d. Gentle hills occur in NNW - NE of the measurement location, the shortest distance to the hill slope is 900m in NNE (In the view of Fig. 1c these hills appear closer in the picture than in reality, which is caused by the shot position in 20m height and the clear sky on the Tibetan Plateau). Other hills and mountains in sectors E,S and W are at least 10 km away from the EC setup. According to our experience, the features in Fig. 2 cannot be attributed to the terrain. We will include information about topography in the revised manuscript.

Planar fit angles for the respective data sets and instruments

Data set	Device	Rotation sector	Sector size [°]	α (pitch)[°]	β (roll)[°]
A	DAT600	whole	360	0.0	4.0
		open	80	-2.0	4.1
		disturbed	280	0.5	4.0
B	DAT600	whole	360	-1.5	-0.3
		open	80	-2.3	-0.4
		disturbed	280	-1.3	-0.3
	CSAT3	whole	360	-1.3	-0.1
		open	240	-0.7	0.0
		front	60	-2.1	0.6
		disturbed (back)	60	-1.7	-1.6

p.4 L17ff "Furthermore, the front sector..." Better state clearly earlier in this paragraph that for the CSAT-3, three and not two sectors were distinguished, and why.

We will revise this paragraph accordingly.

p.4, Results section. Can the planar-fit rotation angles be given (alpha and beta in Wilczak et al.)? How (in-) consistent are they between sectors and between sonics?

PF angles (see Table) α (roll) and β (pitch) show generally low values, which is to be expected for planar fit angles. The largest values are approx. 4° (Data Set 1), which can be easily explained by misalignment of the sensor. They are consistent, i.e., angles for a certain sector can deviate from the respective value of the whole planar fit, but these deviation is "leveled out" by a deviation in opposite direction in the other sector. Angles for smaller sectors deviate more from the whole PF than for larger sectors, which is also not surprising. We will add additional information to the revised manuscript.

p4 L.27f, "Singular and negative... of 140°": looks like 130° in Figs. 2 and 3.

Thanks for the correction, it must be 130-135°

p.5 top paragraph, discussion of Figs. 2 and 3. While Fig. 2d suggests a sinusoid as the main trend in the tilt vs direction dependence (tilt = arctan (w/u), why not use that variable on the vertical axis?), Figs 2e and f do not. As a consequence, the success of the planar-fit rotation is limited, and Figs 3b, c, e, f all show significant structure which is clearly not sinusoidal.

We agree, that Fig. 2e and f do not show a sinusoidal dependence for the whole sector. This is exactly, why we argue to use a PF for the undisturbed sector, as the data in Fig. 2 indicates the existance of a sinusoidal dependence in this sector. Thus, the sectorwise rotation (3d, e, f) yields reasonable residual vertical wind velocities within the undisturbed sector. Some problems, however, remain in the sectorwise rotation of the CSAT3 (3f), which is discussed in the next answer.

In our opinion, the normalised vertical wind velocity w/u the Fig. 2 is as intuitive as the tilt angle. Moreover, the shape of the figures will not change by displaying the tilt angle, because the arctan function is almost linear in the respective range ($|w/u| < 0.2$ except outliers). Therefore we will leave the axis, but will add in the caption: "A normalised vertical wind velocity $w/u = 0.1$ equals a tilt angle of 5.7°."

p.5 L 14f, "reduced ... especially in the front sector": True, but this is because the narrower a sector, the more effective is any regression method to bring the mean tilt to near-zero. There is absolutely no basis for prescribing that the tilt vs direction dependence in this sector should follow a (small segment of) a sine wave. Fig. 3f shows strong discontinuities at the sector limits, suggesting that the fit is inappropriate.

It is true, that a narrow sector can be leveled out more easily than a wide sector. But we cannot see, why this should degrade the performance of the sector-wise PF. As stated in the general comments, a separate PF for the CSAT3 front sector seems to be a good appoximation and Fig.3f supports this idea.

We agree, that there are strong discontinuities at the sector limits, This indicates, that using the standard deviation for wind direction of 20° for the definition of the disturbed sector might be too small to exclude its influence entirely. We will point this out in the revised manuscript. Furthermore, we will unify the rotation of both undisturbed sectors of the CSAT3 in the revised manuscript and update Figs. 3 and 4 as well as Table 2 and 3, accordingly.

p.5 L22, "hardly any irregular": still looks like a considerable proportion to me!

We will change the text accordingly.

p.5 L25ff. It is impossible to judge by eye whether Fig. 4e is "better" than 4b, or 4f better than 4c.

Agreed. We will extent Table 1 with data set B for a better quantification of the differences

p.6 L3, "front and side sectors should be rotated separately". I agree, but not with a planar fit, because it is the CSAT geometry and not the terrain slope (or sonic alignment) that causes the differences between the sectors.

We see, that a sensor specific correction, including the tower, where the sensor is mounted, would be physically based and therefore preferable. However this is currently not available, and it is unclear, whether it is possible to derive such a correction valid in all field conditions (see general comments). Therefore we think, a planar fit for this sector is a reasonable approximation.

p.6 L8, "friction velocity was improved": it was changed, and more often increased than decreased, but whether that is an improvement is unclear.

The sectorwise PF in Fig. 5 uses only undisturbed data to find the rotation angles, while the PF for all directions also uses disturbed data. Furthermore, we compare only data from the undisturbed sector. Although both data sets are still prone to various kind of errors, one error source is clearly eliminated for the sectorwise. Therefore, referring to the differences between both sets as "improvement" seems valid.

p.6 L12: "not significantly affected": how was significance tested?

We used the word "significantly" with its meaning as "substantially" or "considerably", without testing significance. We will revise the text accordingly

p.6 L17, "no significant effect": probably because neither method addresses the underlying physics of flow distortion correctly.

This is not the case as we compare in undisturbed sector only and the sectorwise planar-fit excludes a potential source of error, which is unaccounted for with the PF for the whole wind sector.

p.7 L4ff, "A separate..." I would agree with this sentence if the words "planar fit" were removed. An appropriate fit is required, but there is no justification for it to be planar.

The sentence is true, because the separate PF rotation indeed reduced the mean vertical velocity. But we will add the demand for more physical based rotations methods in the conclusions.

Table 2 and Fig. 5. Please note that standard linear regression is not symmetrical (except for cases of perfect correlation!). The result for the slope depends on which variable is taken as the "dependent" one. It should thus be mentioned what happens in Fig. 5a if the axes were reversed. Or if a symmetrical regression was used instead. Same applies to other regressions that are not shown but summarised in the table.

Thanks for the suggestion. We will revise the Tables and Fig. 5 using the mean geometric regression.

Fig. 1b. The $\pm 10^{\circ}$ sectors are drawn for directions into which the flow is going, not where it is coming from. This is a bit confusing. Also, the probe should be the origin for drawing the angles.

We re-drew the Figure from Friebel et al. (2009), which is originally from Campbell Scientific Inc. We will describe it more clearly in the text to avoid potential misunderstandings

Figs. 2 to 4. In my view, the main message from these figures is that the planar-fit method, in either variant applied here, is not very successful in removing tilt. Hence, run-by-run coordinate rotation (which enforces zero tilt) may not be a dumb idea. Comparing the planar-fit to the double-rotation method for momentum and heat fluxes may provide more useful insights than comparing single planar-fit to sectorial planar-fit!

The PF method cannot remove the vertical wind velocity entirely in turbulent field conditions. Nevertheless, enforcing zero tilt with the DR method leads to other problems, see the general answer to the comment. Therefore the DR cannot serve as a reference, but can be included for comparison. We will extent our manuscript with a comparison to the double rotation.

Further technical comments

We thank referee #1 for his technical comments and will incorporate them in the revised manuscript.

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