Atmos. Meas. Tech. Discuss., 3, C2473–C2477, 2011
www.atmos-meas-tech-discuss.net/3/C2473/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of three-dimensional wind fields from Doppler radar data using an efficient two-step approach" *by* C. López Carrillo and D. J. Raymond

Anonymous Referee #2

Received and published: 14 January 2011

Note: I regret the tardiness of this review. Please forgive me.

One way to consider the significance of this paper is to consider how it adds to what has already been discussed in the Gao et al (1999). The most obvious to me is in the treatment of the Doppler observations by examining the eigenvectors and eigenvalues associated with solving the Doppler projection equation alone and using them to determine if there is enough information to compute a wind solution at that grid point (as well as give an estimate of the Doppler variances at that grid point). In other ways this looks very similar to the what has already been discussed by Gao et al (1999). I also

don't see how this saves much time over (1) in Gao et al, since every grid velocity still has to be compared in some way to the all the good Doppler observations, within the radius of influence of the Doppler observations to the grid point. Once the summations of (2) of Gao et al are stored in the big solution matrix, they can be used over and over in any iterations of the solution. The added value seems to come from estimating the goodness of fit at each location, and how well conditioned the solution really is at any location.

Since the authors still show wind fields that extend beyond the data, in a way that is not only interpolation, but ALSO extrapolation, it would be good if they had shown at least example of the results of eigenvalues (perhaps the magnitude of the second largest eigenvector in the solution) to show where the wind field was well constrained by just the Doppler observations and the continuity equation.

It would also be good if the authors discuss how well they think this method will work when the analysis has a resolution closer to the that of the radar observations themselves ($\sim$1 km).

What's the point of the whole coplane discussion, since the vector normal to this plane (in which continuity will essentially be integrated) can vary considerably from grid point to grid point since the aircraft is a moving platform with changing attitude and direction of motion. There are ways this comparison to COPLAN can have value (as explanation–to explain the direction continuity is integrated since wind constrained in other 2 independent vector directions) but they are not really discussed clearly.

The authors state (quite correctly) that Doppler observations in general have a much larger error than the dropsondes. Since this is so, it would have been good if the authors discussed the difference between their wind retrieval from radar only with the dropsonde observations, by comparing the sonde winds to winds in neighboring grid cells of the radar-only wind fields. This might not be a requirement in a paper discussing the meteorological aspects of the analyses, but since this is Atmospheric Mea-

AMTD

3, C2473–C2477, 2011

suring Techniques, such an examination would be good, even if only discussed. I think a table would be good, however.

Major revisions separated from general discussion above:

1) I tend not to agree with the 1 m/s error estimate for the airborne Doppler radial velocities. I would accept this generally for a ground-based Doppler radar, but not one on a platform that is moving, may be turning, banking, etc. It would have been good if the authors discussed the difference between their wind retrieval from radar only with the dropsonde observations, by comparing the sonde winds to winds in neighboring grid cells of the radar-only wind fields. This might not be a requirement in a paper discussing the meteorological aspects of the analyses, but since this is Atmospheric Measuring Techniques there should be such an examination, even if only in discussion. I think a table or figures devoted to that examination of accuracy would be good, however. This comparison to sondes might also require a finer-resolution analysis of the winds in a region with good Doppler radial velocity observations from two sufficiently independent directions.

2) I think there should be at least one figure displaying the structure and nature of the eigenvalues in the interpolation, probably the second largest eigenvalue, since it is indicative of the quality expected of the wind analysis, or at least indicating the problem is well posed at that grid point.

3) In lines 267-289, there is not sufficient description of the ELDORA radar or the dropsonde capabilities, nor are there good references for these, even if these are simply from an NCAR tech memo. I'm sure there are at least the papers by Hock and Franklin for the dropsondes.

4) Also from Line 243-4: In step 2, how do you constrain the wind in the direction unconstrained by the dual-Doppler observations, the one which will be constrained eventually by the continuity constraint. How do you "fill" all the points with no Doppler observations in this first step, or are you not actually computing a wind at this point,

AMTD

3, C2473–C2477, 2011

and I am misunderstanding.

I have a number of more specific comments.

Line 51. It looks like you meant Gao et al (1999) and not Gao (1999).

Line 108: 'and N is the total number of radial-velocity observation influencing the grid point'

Line 201: Do you actually think the radial velocities that you incorporate in your analyses have an error less than 1 m/s. This error is true in a random sense for observations from a non-moving platform, where the attitude of the platform is known very accurately, but how accurate do you think these observations really are, even after careful quality control from NCAR? The main error comes from projection of the aircraft motion on the Doppler radial. This is another reason for some comparison of the radar-only analysis with dropsondes.

Line 203: What constitute "bad gridded velocities"? Velocities that don't have at least two good eigenvalues out of 3? Isn't there value in using the Doppler observations from just one radar, if you are going to compute a solution in grid cells without sufficient observations from two radars? They can help constrain the analysis, even if not completely. I would not say this if you were not including extrapolated winds in your displayed analyses. Perhaps you do use the first eigenvector even if you don't use the second, when constraining the filled (extrapolated and interpolated) analyses you show in the paper.

What do you do with the Doppler observations near the radar, where scanning can produce enough observations, in enough independent directions to produce a wind estimate, even without continuity, within a single grid cell? Do you keep all three eigenvectors in that case? Since you use a very coarse grid, this ought to be possible near your flight track. You might state what you do if you get 3 good eigenvectors, or do you ever get three good eigenvectors?

AMTD

3, C2473–C2477, 2011

≈Line 333 Since you have computed 'variances of the reduced velocities on the analysis grid,' it would have been good to see such an example plotted, or something said about how this number varied, and over what range you accepted the data.

Line 334 Shanno and Phua (1976) is not in the reference section. Should this be Shanno and Phua (1980)?

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4459, 2010.