Review of "Validation of torus mapping method for dealiasing Doppler weather radar velocities", by P. Smerkol et al

This paper presents a Doppler dealiasing technique based on torus mapping, which assumes that horizontal wind components are linear. The paper is well written and clearly organized. However, there are several flaws listed below that led me to reject this paper. The two main reasons for not recommending major revisions are that I don't believe such technique can ever produce error rates sufficiently low in the most important convective situations for operational applications, especially when low Nyquist velocities are used (because it's based on a linear assumption) and the underlying idea is the same as already-published work based on the VAD technique.

1. The torus method presented in this paper is identical to the idea of dealiasing Doppler velocity using VAD winds (Browning and Wexler 1968). Besides, the implementation using first-order derivatives of radial velocity is identical to the work presented in Tabary et al. (2001). These relevant papers are not cited anywhere in the manuscript.
2. The most challenging meteorological situations for dealiasing are associated with convective systems, especially in high shear environments and when small Nyquist velocities are used. Those are also the most important conditions operationally, where Doppler velocities need to be accurately dealiased. In these conditions, the horizontal wind components are far from linear, so the main assumption made in the torus / VAD analyses is not satisfied. This major problem is not even mentioned in the presentation of the method. Depending on the Nyquist velocity used, linear winds are expected to perform very poorly for dealiasing purposes (I have tested that myself). This is not evaluated at all in the paper using simulated winds with various degrees of non-linearity. Probably because the main finding will be that the technique cannot work in these convective situations.
3. The introduction is missing most major recent developments in dealiasing techniques (e.g., Helmus and Collis 2016; Feldmann et al. 2020; Louf et al. 2020) and some important older ones (James and Houze 2001).
4. Figure 3 shows that most of the precipitation data are not even considered for dealiasing, which does not make sense. A centred-difference scheme to estimate the first-order derivatives should not remove so many points, so I suspect that there is a bug in the implementation. Besides, a dealiasing technique is supposed to provide a solution for all measured Doppler velocity bins.
5. The validation with in-situ aircraft winds will only contain non-convective cases, since aircraft will avoid convective situations (especially at take-off and landing). Consequently, the validation shows that in non-convective situations, the technique works reasonably well. But all existing dealiasing techniques will work well in those simple situations.
6. When choosing datasets A and B, it seems like you are combining radar data in each set with different Nyquist velocities? If that is indeed true, it makes the interpretation of the PDFs of difference very difficult. Also, a much better metrics to estimate the accuracy of the technique than PDFs of difference would be to estimate error rates (i.e., how often is the technique wrong when Doppler data should or should not have been dealiased), see for instance Louf et al. (2020). Small percentages of wrongly dealiased points won't really show in mean and standard deviations of differences.
7. In the conclusion, the authors estimate that the procedure correctly dealiases about 90% of aliased data, which is a 10% error rate. This is definitely not good enough, especially operationally. The authors do not seem to realize that at all. Advanced techniques have error rates in the 0.1 to 0.2% (again, see recent publications).

8. The comparison with NWP based on rejected points with a 20 m/s threshold is interesting because it includes convection. However, the issue with using NWP is that it generally does not fully resolve convective-scale dynamics (even with a 4.4km grid) and convective systems are generally not at the right place. These issues are clearly discussed in the papers mentioned above, but not acknowledged anywhere in this paper.
9. I did not provide detailed editorial comments, given my recommendation to reject the paper.

References:

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