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Atmospheric Measurement Techniques Discussions

Interactive comment on "A quantitative analysis of the impact of wind turbines on operational Doppler weather radar data" *by* L. Norin

Anonymous Referee #1

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General comments:

Overall, I feel that this is an excellent paper that is well worthy of publication in AMT. The discussions are clear, the reasoning is (generally) sound, and almost every concern that I had while reading the paper was quickly addressed within a paragraph or two. It is a pleasure to review a paper that is this solidly assembled before it reaches the review stage.

Specific comments

My lone concern comes in the attribution of the deviations from normal in radar scan data above the wind turbines to anomalous propagation as opposed to increased turbulence or sidelobes. The authors use a fairly coarse method of assigning blame by

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divvying up the year into periods in which anomalous propagation (AP) is likely based on typical thermodynamic profiles associated with specific seasons. I am unfamiliar with Swedish boundary layer structures, and so I don't know how representative a typical sounding is of the atmospheric conditions on any given day. It may be that this method is acceptable given appropriate justification of the typical structure. However, it seems like there are more elegant ways of discerning if AP is present. Obviously, scans could be matched up to individual soundings, but it may also be possible to investigate non-turbine impacted cells for evidence of AP. If AP is present, for example, cells azimuthally adjacent to the turbines would show an expected temporal change in reflectivity as the inversion develops; if both turbine cells and non-turbine cells are showing the same general form of a change in reflectivity with time, AP is a stronger candidate to be the culprit than if the temporal change in reflectivity is limited to the turbine cells. These kinds of analyses would provide a stronger case that the enhanced spectral moments are due to AP.

This is really straddling the line between a technical correction and a minor revision. I am marking it down as a minor revision because, even though it is in significantly better shape than almost any paper I've read lately, I would rather err on the side of too much review. I leave it to the esteemed editor of this publication to determine if an additional review is necessary, and I will be satisfied with his decision either way.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 8743, 2014.