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Solid Earth Discussions

Interactive comment on "Migration of Reflector Orientation Attributes in Deep Seismic Profiles: Evidence for Decoupling of the Yilgarn Craton Lower Crust" *by* Andrew J. Calvert and Michael P. Doublier

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This paper addresses the interpretation of deep 2D crustal seismic reflection data acquired along crooked acquisition lines. It provides an excellent demonstration of how the crooked nature of the seismic line, which most often is considered a detriment, can be exploited to provide geometrical information that further constrains geological interpretation of the seismic profiles. Specifically, the true orientation (strike and dip) of individual reflectors is determined through stacking analysis for a range of assumed trial strike and dip values. The attribute associated with each coherent reflector (strike

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or dip) is then geometrically migrated and overlain on the 2D migrated seismic section for direct comparison. The added value of this methodology is in providing constraints on 3D orientation and subsurface positioning of reflectors that helps clarify geological relationships (cross-cutting fabrics, disconformities etc.)

There are some aspects of the paper that require further justification or clarification. The ones which I would suggest are the following.

Strike and dip as used by the authors refer to local (at the scale of the reflectors) measurements; i.e., the values provide the true local orientation of the reflector. As such, for more complex 3D geometries, it is important to recognize that these are local measurements. In making a structural interpretation, they contribute to an understanding of the true geometry, but don't directly identify unique structural domains. I don't think the authors make this entirely clear, giving too much credence to the resultant strike values (on p2 l4 it is stated that 'Reflector strike is probably the orientation attribute that contributes most to an interpretation').

The authors associate differences in reflector strike directly to different strain regimes (e.g., flow directions, vergence, and age of deformation) which I don't think is an immediately obvious connection. Justification of this underlying assumption should be given or further explanation of the basis for interpretation (observed geometries and relationships between reflection bands or zones) should be stated.

In terms of the algorithm, is there a trade-off between reflector strike and dip determined by the grid search algorithm; i.e., are there multiple values of strike and dip that give the same goodness of fit and how large are the related uncertainties? How does the trade-off depend on the angle of dip? Presumably, the trade-off is quite severe for low dip values (e.g., in the limiting case of a horizontal reflector, the strike is undefined). Can you quantify this with some simple plots (perhaps something like the plots in Levin 1971). Some assessment, or at least a description of some of these issues and the related uncertainties should be included.

As described in the text, the attributes (specifically, reflector strike values) are migrated in 2D as line segments so that they can be overlain on the 2D migrated seismic sections. This is useful. However, it doesn't resolve the old issue of applying 2D migration to crooked line seismic data (which is really 3D). The authors acknowledge this, and note that the facets could actually be migrated in 3D. In contrast to the authors, I think that 3D migration can be quite informative. Perhaps they could make some statement about conditions under which this might be useful.

In regard to attribute migration, the description of the process needs some further elaboration. Currently, the text reads 'Using these apparent dips and the 1-D stacking velocity function, each attribute sample was migrated to a 320 m-long linear segment centred on its output location with only the most coherent event retained at each position, as described earlier.' Why 320 m?

The text states that reflectors with migrated dips of > 50 degrees were excluded in the process. This in conjunction with the postulated large uncertainties (see earlier comment) for very low dips, suggests that the method is most useful for intermediate dips. Is this a fair statement? If so, perhaps should incude this in the conclusions.

At various places in the text, dips are referred to as 'steeply' dipping when they are actually moderately dipping. This should be checked.

For the reader who is interested in applying this method, it would be very useful to provide some guidance on how large a S-R azimuth range is required to get reasonable estimates.

It is stated that 'however, in practice, those parts of the seismic line where it is difficult to obtain orientation angles are reasonably well predicted by the range of available source-receiver azimuths, which is defined to be the number of one degree azimuth bins for which there are seismic data available.' But, isn't it really the azimuth range that is important? For example, cases where there aren't a large number bins, but the actual range is large would give good results?

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In the interpretation, only results where uncertainties are < 30 degrees are included. But based on the stated uncertainty range (30 degrees) the lower crust has the same strike values within the uncertainty limits. This would suggest that the method is not particularly useful for the lower crust, but it's real value is for the middle and upper crust. It would be useful to include a plot that shows the uncertainties in section format.

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