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Solid Earth Discussions

Interactive comment on "Improved finite-source inversion through joint measurements of rotational and translational ground motions: A theoretical study" *by* Michael Reinwald et al.

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The paper is very interesting and deals with an actual topic of employing seismic rotational components in finite source inversions. It is basically well written, namely the first part. The results showing that slip at shallower depths is better resolvable (in the particular model considered) from six-component data are quite convincing. Also the importance of horizontal-axis rotation rates, containing vertical ground motion gradients, for a dip-slip scenario is nicely documented in the results. Some statements at the end of the paper (Discussion, Conclusions) seem to be too optimistic and should be reformulated with a greater caution having in mind that only a simple 1D model has been taken into account in the synthetic study.

I found some typos and I also add a few specific comments:

Page 1, Abstract, Line 3: first sentence - 'seismic source inverse problem' is more proper than just 'seismic inverse problem'

Page 2, Line 24: (( ))

Page 3, Line 16: /vecm ???

Page 3, Line 19: 'denotes' instead of 'denots'

Page 3, Line 21: the abbreviation 'pdf' has not been introduced (it should be done in Line 15)

Page 3, Line 24: What 26 parameters ?(they have not been specified yet, they are explained later, on Page 5)

Page 3, Line 24: Metropolis algorithm ... add citation, e.g., Hastings (1970)

Page 3, Line 26: the these

Page 4, Line 12: Are the 44 receivers at the Earth's surface? Are the stress-free conditions taken into account in the calculations? (simplification of the horizontal rotation rate components).

Page 4, Line 16 and Fig. 1 caption: not the whole fault but fault trace is illustrated ...

Page 4, line 19 and after: the slip model is unclear to me. Are the point sources used only to calculate the average rupture time for each of the subfaults? Is N in Eq. 6 number of subfaults or number of point sources?

Page 5, Eq. 6: not all the quantities in the equation are described properly in the text

Page 5, Line 26: There are already existing rotational sensors capable of measuring in this range of horizontal-component rotation rate amplitudes, e.g. Rotaphones, six-degree-of-freedom portable seismic sensors (a few references to consider: Brokešová J., Málek J., and J.R. Evans (2012). Rotaphone, a new self-calibrated six-degree-of-

freedom seismic sensor, Review of Scientific Instruments, Vol. 83., 086108; Brokešová J. and Málek J (2013), Rotaphone, a Self-Calibrated Six-Degree-of-Freedom Seismic Sensor and Its Strong-Motion Records, Seismol. Res. Let., Vol. 84, No. 5, 737-744; Brokešová J. and Málek J (2015), Six-degree-of-freedom near-source seismic motions II: Examples of real seismogram analysis and S-wave velocity retrieval, J. Seismol., Vol.19, No. 2, 511-539).

Page 6, Line 8: it should be noted that Fig. 5 correspond only to Scenario 1 (strike-slip) and regular station distribution.

Table 1, Caption: The largest difference in information gain...? What difference? between the 6C and 3C cases? Than it is the shallowest subfault layer.

Figs. 5,6,7,8: vertical axes should be labeled.

Page 7, Line 6 and Fig. 8: improvement (6C vs. 3C) in resolving rupture velocity is not convincing.

Fig. 7 and 8: What is the fault dip? (Page 5, Line 13: dip varying from 90 to 45 degrees is mentioned; it seems to me that the authors originally intended to consider several dip values but in fact present only the normal-fault case - it should be clarified).

Page 7, Line 12: “because more receivers are selected north and south of the fault than in east and west direction.“ ... i didn’t catch the sense of this sentence.

Page 8, Line 12: ‘calculated’ instead of ‘measured’ (synthetic study).

Page 9, Line 32: The sentence starting “ This applies ...” is unclear.

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