NHESS-2015-79, Reply to Referees comments

Dear Editor,

please, find below the point-by-point responses (*in italic*) to all the comments of the referees about the paper entitled "Source of the 6 February 2013 M_w8.0 Santa Cruz Islands Tsunami" by Romano F., Molinari, I., Lorito S., and Piatanesi A.

I acknowledge that all referees raised very reasonable points. Please find enclosed a revised version of the manuscript, in which we addressed most of their comments. This led, in my opinion, to an overall improvement of the manuscript.

Best Regards,
Fabrizio Romano

Response to Referee #1

Manuscript of Romano et al. addresses the source model of the Santa Cruz island M8.0 earthquake as revealed by inversion of 5 DART and 3 local tide gauge sea-level records. The manuscript has a very clear structure: Authors start by presenting observation data; continue by describing their rupture- and tsunami- forward modeling approaches; then – inversion scheme; checkerboard resolution test; resulting slip distribution with some rupture kinematics; and, finally, discussion regarding complex oblique tectonic regime as well as differences to the previous source models.
The Manuscript is written in a very professional way – clear, concise, well illustrated. A good example of scientific paper writing.

1) Page 1952, Line 4-5: try to re-write this sentence, because it is not clear what means "... events ... not dislocated ...". 'Dislocated' relative to what? I assume Authors mean aftershocks taking place exactly at the subduction plate interface, not deeper and not shallower. Along the plate interface, aftershocks can be of course 'dislocated' relative to the main shock or to each other.
Ok, revised.

2) Page 1956, last sentence: please re-write this sentence, now it reads just like a real mess.
We revised and greatly simplified the sentence.

3) Figure 6: I suggest to place graph labels (a)-(e) out of each panel or, at least, make them more visible (increase font size, use bold). It is hard to find them on the Figure.
Ok.

4) Same Figure: 'black star' mentioned in figure caption is missing on panel (d).
Ok, revised.

5) Authors apply the linear concept of Green's functions to model (and to invert) tsunami waveforms at 3 tide gauges located at a very shallow depth. Strictly speaking, linear long-wave approximation is not valid any more in the vicinity of tide gauges, together with the whole Green's functions concept. In their previous publication, Piatanesi and Lorito (2007) suggested considering Green's functions as an attempt to linearise the non-linear problem around some most representative slip value at the patches. Anyway, applicability of the Green's functions concept is to be proved numerically. Such a test is, to my understanding, not less important than the checkerboard resolution test. To do that, I suggest Authors to compare synthetic waveforms for the two 'predicted' models: (i) model which is a superposition of the pre-computed Green's functions (I assume that is exactly the model which they call 'Predicted' on Figure 5), against (ii) model computed 'from-the-scratch' using their best-fit source, without any Green's functions, just in one single Neowave run. Differences between waveforms at tide gauges should then prove (or disprove) applicability of the Green's functions concept for source inversion at coastal tide gauges. I think this is a very important point.
We checked the validity of linear approximation by following the referee's suggestion. The comparison between the tsunami waveforms obtained by Green's functions superposition and those ones obtained by a single run "from scratch" with NEOWAVE has now been added as a new figure in the Supplementary Materials (Figure S4). This test highlights that the linearity is sufficiently preserved at the coastal tide gauges. In particular, some small discrepancies are observed after the first cycle of the signal, when the nonlinear effects start to be more relevant. The overall differences are however much smaller than those between the predicted and the observed signals. The results of this test are in agreement with those ones published in Yue et al. (JGR, 2015) where a similar test has been performed to validate the linear approximation assumption.

6) How important is rupture kinematics for the final model? Authors employ radial rupture propagation with 1.5 km/s and note that the main seismic moment in their model was released between 15 and 45 seconds origin time. This 'less-than-one- minute' timing is, for sure, important for teleseismic inversions, but does it has any sense for tsunami generation and propagation? Would the predicted waveforms at buoys and tide gauges be changed if we assume an instant sea-floor deformation? If not, maybe we should try to keep the model as simple as possible?
Correct. Tsunami data, in principle, and in the specific case with relatively distant tsunami sensors, are less sensitive, compared to teleseismic data, to resolve the seismic rupture velocity. Therefore, in our inverse problem we used as a-priori information the rupture velocity estimation arising from teleseismic data inversion (Lay et al., 2013). Accordingly, comparing the tsunami waveforms predicted by an instantaneous model with those predicted assuming a finite rupture velocity (Figure 1 in this letter) we observe that the former are systematically anticipated (1-2 minutes). However, as pointed out by the reviewer, this is not much relevant in terms of the retrieved model. These differences are indeed much smaller than those with the observed waveforms. Nevertheless, we prefer to keep this (seismically-constrained) time-dependence in our model in order to make a qualitative comparison between the moment rate function obtained by teleseismic inversions and the moment rate function that we derive combining the position of the slip patches with the assumed rupture history. In order to avoid confusing the reader, we revised also the sentence at Page 1958 regarding the comparison of moment rate functions.

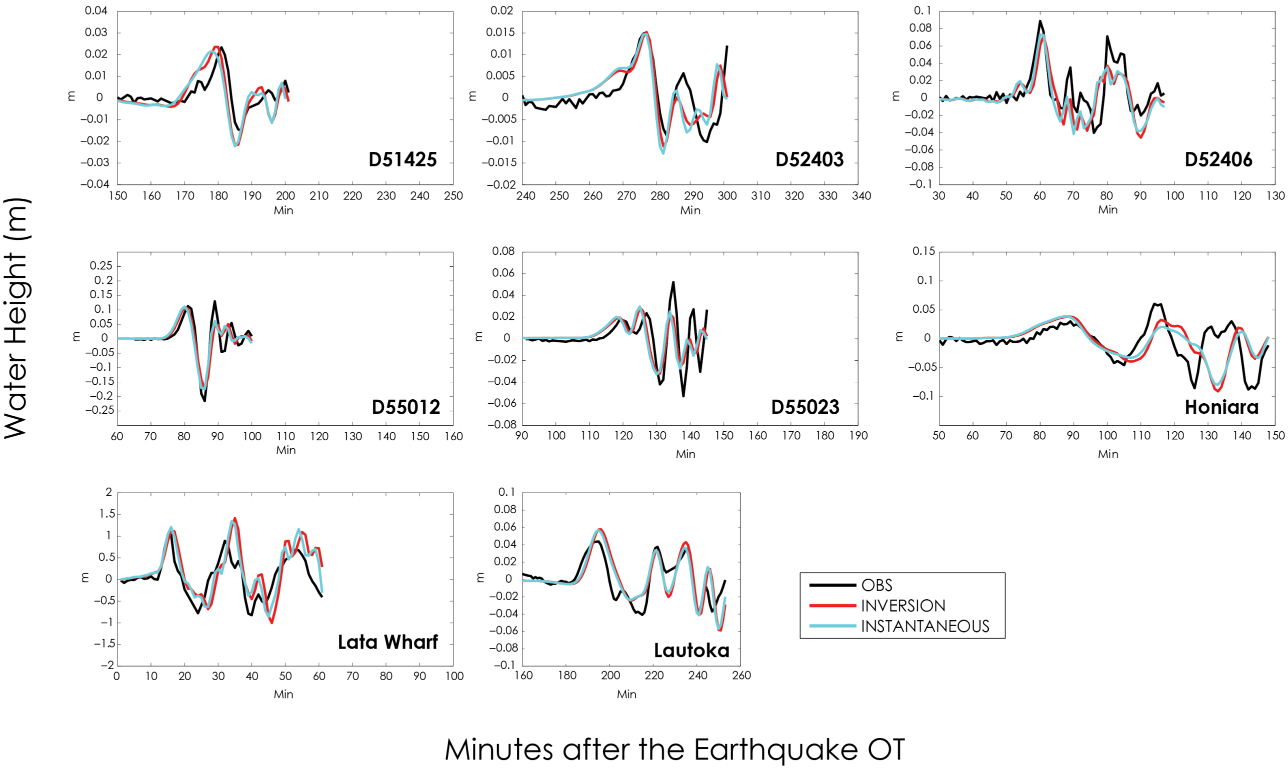

***Figure 1** – Comparison between tsunami waveforms predicted by an instantaneous model (cyan) with those ones predicted assuming a rupture velocity (red). Observed tsunami waveforms in black.*

Response to Referee #2

The manuscript by Romano et al. is well written. The authors used tsunami waveforms to estimate slip distribution on a 3-D plate interface. The tsunami waveform inversion that they used is scientifically sound and they implemented the method with high discipline (i.e. using highest freely available bathymetric and topographic data, doing checkerboard test). This study shows that the main slip region is located south east of the epicenter, which was absent in a previous slip inversion study using teleseismic waveforms by Hayes et al. (2014). I have only minor comments for the manuscript.

1) Fritz et al., (2014) reported a maximum tsunami run-up of 11 m (mentioned in page 1951, 5, and page 158, 10). I wander how high is the simulated tsunami run-up (or coastal tsunami height) from the author's source model. Can their source model explain the observed run-up?
Since we have not a high-resolution bathymetry/topography dataset, we are not able to properly model the tsunami runup resulting from our source model. After performing a forward modelling (without applying no-flux boundary condition at the shoreline), we observe that the maximum tsunami heights predicted at the 50 m isobath around the runup measurement point (Fritz et al., 2014) are in a range of values from ~4 to ~6 m Thus, in principle our source model might be compatible with the observed runup, even if in this case a narrow comparison results to be difficult.

2) The discussion section is very long compare to other sections. The discussion section will be much easier to read if the authors divided it into subsections.
We followed the referee's suggestion, and now the Discussion section has been divided in 2 sub-sections.

3) Page 1956: In principle, teleseismic data well constrain the earthquake seismic moment and the seismic rupture history, and, compared to tsunami data, they are less sensitive to the spatial details of the slip distribution (e.g. Yue, 2014). Page 1958: Thus, despite of the differences among the models, and even though tsunami data are not particularly suitable to resolve the details of the seismic rupture history, our source model, at least at the first order, is in agreement with the moment rate functions resulting from studies that used teleseismic data. Another reference can be added, Gusman et al. (GRL, 2015) shows that teleseismic data provide stable moment rate function while tsunami data provide stable spatial slip distribution.
Reference: Gusman, A. R., S. Murotani, K. Satake, M. Heidarzadeh, E. Gunawan, S. Watada, and B. Schurr (2015), Fault slip distribution of the 2014 Iquique, Chile, earthquake estimated from ocean-wide tsunami waveforms and GPS data, Geophys. Res. Lett., 42, doi:10.1002/2014GL062604.
Good suggestion. We added the reference Gusman et al. (2015).

Response to Referee #3

Written in a concise and straightforward manner, Romano et al. (2015) made a great effort to retrieve the fault slip models of February 6 2013 Mw 8.0 Santa Cruz Islands earthquake with the constraints from tsunami observations at DART stations and tide gauges and to make their reinterpretations of the physical process, compared with other previous models (Hayes, et al., 2014 and Lay et al., 2013). One highlight of this paper is the nontrivial significance of using realistic 3D geometry. As they demonstrated, it could potentially change the locations of slip asperity during this earthquake, arguing for large seismic slip SE of the hypocenter reaching the trench where it was partially hypothesized to be slipping aseismically in previous studies. Their results and interpretations are generally convincing because of better consistency with run-up observations and good match between predicted and observed tsunami waveforms.

1) Regarding the synthetic checkerboard test, more careful discussions about uncertainties in the problem is lacking. First of all, it's not mentioned that whether synthetic waveforms have been contaminated with artificial observational errors (e.g., white noises), which should always be done for synthetic tests.

Now we performed a new checkerboard test where the synthetic tsunami waveforms are corrupted by adding a Gaussian random noise with a variance that is the 10% of the clean waveform amplitude variance.

We replaced Figure 3 with the new checkerboard results, and modified the text in section 3.1 accordingly. We also replaced Figure S3 within Supplementary Material with the new tsunami data fit.

Secondly, there is likely uncertainties in the forward modeling (GF). Although it's admittedly difficult to quantify this source of uncertainty, it should be easy to discuss whether this uncertainty could be believed to be small or has been greatly reduced with the choice of forward modelling approach and under the conditions of this problem. Without tests including and discussions about these uncertainties, the result of resolution test will not represent and probably overestimate the resolving power of tsunami data in the real world.

Correct. A sentence commenting the issue has been added in the manuscript.

2) The authors clearly demonstrate the major differences in slip models for studies that employ different fault geometries. In my opinion, the effect of non-planar fault geometry could be better supported if the authors explored a solution with all inversion parameters the same as the preferred model except with the planar fault geometry based on previous studies. Due to the difference in fault depth, slip amplitudes must be scaled in order to fit tsunami signals, but it's not so certain if the slip patterns would change and if so, how. The difference between this model and the preferred model could reveal how much of the differences results from different fault geometries or from different inversion schemes/parameters, which usually produce non-negligible differences too (e.g., Source Inversion Validation (SIV), Mai, 2012).

The maximum vertical surface deformation decreases with the focal depth for constant slip values; the maximum amplitude of the tsunami scales accordingly; increasing the fault depth also results in a relatively larger wavelength of the initial field (e.g. Geist, 1999). Thus, in order to fit a tsunami

waveform, in an inverse problem, the position of the slip should "adapt itself" accordingly, e.g. it would at least "move" along dip.
So, even if the point risen by the referee is important, and it would deserve a thorough study for a quantitative mapping of the uncertainties regarding fault geometry on the inversion results, this is beyond the purpose of the present study. Actually, we restricted to highlight the differences among different source models pointing out that they might be likely due to a combination of the effects of different data resolving power used and of different fault geometries. The effect of the fault geometry on the slip distribution for subduction earthquakes has already been discussed in previous papers for other past earthquakes (e.g. Lee, 2006; Moreno, 2009; Hayes, 2014); since subduction interfaces geometry are relatively well known with respect to other tectonic environments then, in the present study, we adopt a non-planar fault with the aim at reducing the epistemic uncertainties associated to the tsunami forward modelling.

3) Tsunami waveform is commonly treated as a type of quasi-static data, to constrain only the static slip during earthquakes. How the time-dependence of source would affect tsunami observations (Satake et al., 2013), and whether the tsunami waveforms contain kinematic source information are still open questions. In the kinematic tsunami model of this study, a spatially uniform, a priori determined rupture velocity of 1.5 km/s is imposed. Because of the imposed uniform rupture speed, the spatial distance of slip asperity directly translates to the time delay of major moment release. If tsunami data cannot resolve the kinematic process in this problem (this could be tested by comparing the waveforms from forward modelling the time-dependent and instantaneous uplift), the uncertainty and variations of the rupture speed alone would determine the shape of the moment rate functions. This could be discussed more.
Correct. Please, see response to comment #6 of referee #1.

4) Page 1950: Line 13, reaching to the trench => reaching the trench
Ok, corrected

5) Page 1951: Line 8, few days => a few days; Line 14, slip patches position => slip patch positions; Line 17, teleseismic broad-band P waves inversion => teleseismic broad- band P wave inversion
Ok, corrected.

6) Page 1952: Line 1, tsunami waves excitation => tsunami wave excitation
Ok, corrected.

References

Geist, E.L.: Local tsunamis and earthquake source parameters: Advances in Geophysics, 39, 117-209, 1999.

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