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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Estimation of successive co-seismic vertical offsets using coeval sedimentary events – application to the Sea of Marmara's Central Basin (North Anatolian Fault)" *by* C. Beck et al.

C. Beck et al.

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Proposed responses for Manuscript NHESSD 2 C1455-C-1458, 2014 (modifications and corrections appearing in new version take all in account) A) Following Referee 1 (L. McNeill). I agree with the fact that the presented work is basically sedimentological with an implication for a co-seismic tectonic activity analysis. Both Title and Abstract are modified in this sense. Concerning the seismic stratigraphy/core data links, they have been detailed in a previous published work (Beck et al., 2007). Nevertheless,

main links – indicated on Fig. 4 will be underlined.

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Specific point 6: In order to establish event-by-event correlation, a very well-defined layer - usable as an isochron - was necessary. The chosen one belongs to the non marine/marine change level according to previous studies and dating; it is conspicuous, unique in terms of texture, composition, and bioturbation, and could be recognized on the three cores.

Specific point 7: I agree with a possible erosion below a density flow, which may eliminate some hemipelagic mud and biases the measurement of time elapsed between two reworking events; especially with a very low sedimentation rate. Grain size and shape, current velocity, and state of compaction of recovered hemipelagites, are the main parameters influencing this possible erosion. Nevertheless, in one of our previously analyzed case (Beck et al. 2007), a 210Pb decay curve and a 137Cs peak were detected below a silty/sandy, 2m-thick, homogenite, thus indicating a negligible erosion with respect to the hemipelagic sedimentation intervals (several centuries). Let's remind that, in coastal macrotidal regimes, few mm-thick clayey-silty layers (from suspended-load settling) are commonly preserved under up to 1.0 m.s-1 tidal currents carrying sand-size particles. There removing may occur but as mud-clasts; in the presently studied

basin, we only observed mud-clasts of hemipelagic laminated sediments in the large pre-Late Glacial event (respectively 8 m- and 5 m-1thick on footwall and hanging wall , Beck et al., 2007).

Specific point 8 and 9: In order to propose an estimation of moment magnitude, we discuss the choice of a dominant displacement (normal vs. lateral). Published tectonic models differ for the general NAF pattern across the whole Sea of Marmara, but they agree with the occurrence of a lozenge-shaped pull apart structure for the Central Basin (Armijo et al., 2005; synthesis in G. Uçarcus's PhD). Thus, a normal component is emphasized, especially for the here-discussed SW limit. This point has been analyzed in detail by Armijo et al. (2005), based on a dense high resolution survey of the sea floor along the submerged trace, in the whole Sea of Marmara. Detailed deep seismic imagery (Becel et al., 2010; Laigle et al., 2008) indicate a strong subsiding component. For the (eastern) Çinarcic basin, Kurt et al (2013) defined an oblique slip accommodated along the northern limit. A direct observation of slickensides were done on the western end of the (western) Tekirdag basin on the deeply submerged prolongation of the Ganos fault (Armijo et al, 2005): approximately 15°. Our attempt to estimate an inclination of the displacement vector remains hypothetical; nevertheless, based on above-mentioned available data, we locally favored a dominantly vertical displacement. In the new version, we test the influence of a 70° pitch. We do not assess any conclusion concerning earthquake hazards; as previously modeled and discussed by Hébert et al. (2005), if admitting dominant vertical throw, tsunamis genesis represents the main possible risk. Our final estimation – a corollary approach with respect to the sedimentological study - is an additional matter of testing the values of throw (realistic or not). As the sedimentation completely "sealing" the subaqueous scarp (see also Beck et al., 2012) appear composed of well-defined separated unique events, we concluded that each of the later ones correspond to a unique scarp genesis.

Armijo, R., et al., 2005. Geochemistry Geophysics Geosystems 6, Q06009, 29, doi:10.1029/2004GC000896. Bécel, A., at al., 2010. Tectonophysics, 490, 1-14.

Laigle, M., et al., 2008. Earth and Planetary Science Letters, 270, 168-179.

Specific point 10 and 11 and grammatical points: Corrections and modifications are done.

B) Following Referee 2 (anonymous).

Response to general comments According to chronology, Radiocarbon dating of the three MD core is not dense, and after preliminary measurements (Beck et al., 2007). For our work, only few additional dating were attempted for the Çinarcik Basin. Detailed chronology (Radiocarbon and short-live radionuclids) were conducted by other colleagues on the last millennia (short cores, Drab et al., 2012) and the last centuries (Uçarcus's, PhD). We basically focused on the pre-Holocene part to establish event-by-event correlation in the Central Basin; for that we needed a well-defined correlation level at the Non Marine/Marine transition, independent from any earthquake/landslide-triggered sedimentary event. The chosen one corresponds to these criteria and was also recognized in Cinarcik Basin's long core, indicated a general, widespread, paleoenvironmental change (chemical, mineralogical, and biological modification). We believe that additional attempt for the Radiocarobon dating of the envisaged interval should not bring more chronological precision, because of: error bar, lack of well-constrained reservoir effect (general and local ïĄĎr), and dependence on choice of C-support (debris reworked within turbidite, dispersed organic particles, shells). Conversely, for our purpose, lateral correlations established with different parameters (MS, VHR profiles, and layering) to discuss the differences between deep part and top of the southwestern scarp. Concerning what can be considered as co-seismic sedimentation – which is actually tsunami-generated sedimentation – its characterization has been developed by different teams since several decades and references (although not exhaustive) are mentioned, including our own published contributions. A complete session was dedicated to this topic during the last International Sedimentological Congress (ISC-IAS) held in Geneva (16-22 August 2014). In our draft, the final corollary results are not mentioned in terms of seismic hazards. We do not assess any

conclusion in this sense; as previously modeled and discussed by Hébert et al. (2005), if admitting dominantly vertical throw, tsunamis genesis represents the main potential risk. Our final estimation – a corollary approach with respect to the sedimentological study - is an additional matter of testing the values of throw (realistic or not). Ongoing work concerns the possible genesis of tsunamis by the measured offsets.

Specific point 1: The pre-Holocene period were chosen: 1) because of better-defined tsunami-related deposits; 2) a situation during which, as for other areas of the Sea of Marmara or in other studied cases, active fault traces - with clear vertical component - have no influence on bottom morphology because of systematic sealing by differential sediment redeposition on depressed areas (including hanging walls). During MARMARASCARPS Cruise, directed by R. Armijo and J. Malavielle, shorter Küllenberg type cores, dedicated to the last millennia, were retrieved from the different basins. Beside, a set of 100 short cores (35 cm) were visually picked using VICTOR R.O.V., preserving sediment/water interface. We participated to these two parts of the survey. The processing of these two sets of core were included in L. Drab's and G. Uçarcus' PhD theses. Drab et al. (2012). Polonia et al. (2004) and, more recently, McHugh et al. (2013) proposed such inter- and intra-basin correlations, and deduced correlation with historical ruptures which occurred along main strike-slip or oblique slip (not on Central Basin limits). The attribution of sedimentary events to historical ruptures are made through chronological correlations (see also Chapron et al., 1999; Pouderoux et al, 2012; etc.). At the difference our purpose (see Beck at al., 2012) is to directly "connect" an active trace activity to sediments deposited along this trace.

Specific point 2: A core information remains punctual, and the Homogenite-Turbidite events are basically caused by reflected tsunami/seiche effects reworking one or several fluidized subaqueous landslides. First, the later ones may occur without seismic shock; second, even if identified as earthquake-induced, the HmTu don't necessarily reveal the location of responsible ruptures. With respect to the relative positions of the two cores, the 29 site is in the center of the basin, and not at the exact foot of the scarp, but, if following on VHR image the thickest homogenite (pre-LG), it changes appears abruptly close to it, and not progressively (as on the Northeastern side. Although the seafloor morphology displays a unique scarp, the subsurface structure is more complex due to probable clastic injections. Our major argument is the fact that the strong thickness increase of each separated depositional event is systematically related to its position in the deepest part.

Specific point 3: I agree with a possible erosion below a density flow, which may eliminate some hemipelagic mud and biases the measurement of time elapsed between two reworking events; especially with a very low sedimentation rate. Grain size and shape, current velocity, and state of compaction of recovered hemipelagites, are the main parameters influencing this possible erosion. Nevertheless, in one of our previously analyzed case (Beck et al. 2007), a 210Pb decay curve and a 137Cs peak were detected below a silty/sandy, 2m-thick, homogenite, thus indicating a negligible erosion with respect to the hemipelagic sedimentation intervals (several centuries). Let's remind that, in coastal macrotidal regimes, few mm-thick clayey-silty layers (from suspended-load settling) are commonly preserved under up to 1.0 m.s-1 tidal currents carrying sand-size particles. There removing may occur but as mud-clasts; in the presently studied basin, we only observed mud-clasts of hemipelagic laminated sediments in the large pre-Late Glacial event (respectively 8 m- and 5 m-1thick on footwall and hanging wall , Beck et al., 2007).

Specific point 4: Please, see response to this point in the first paragraph (Response to general comments)

Specific point 5: I propose the same response as for Referee 1's Points 8 and 9. In order to propose an estimation of moment magnitude, we discuss the choice of a dominant displacement (normal vs. lateral). Published tectonic models differ for the general NAF pattern across the whole Sea of Marmara, but they agree with the occurrence of a lozenge-shaped pull apart structure for the Central Basin (Armijo et al., 2005; synthesis in G. Uçarcus's PhD). Thus, a normal component is emphasized, especially

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C) Following Interactive comment (Christopher Sorlien). The proposed Mw estimation is a corollary of the main conlusions concerning a very small part of the NAF northern trace; this attempt was developed in order to check if our conclusions about the vertical components of successive offsets were realistic/reasonable or not. We are not discussing the general strike slip-related activity and associated hazards. We do not address any results concerning the whole Sea of Marmara portion of the NAF. Our conclusion in terms of hazards concern the genesis of tsunamis, not regarding any paleo-magnitude. Published tectonic models agree with a pull apart significance of the Central Basin's inner basin. Its SW limit (8 km), which is clearly displayed by detailed morphology/bathymetry (see Armijo et al., 2005), should be submitted to tension and the corresponding throw should be dominantly normal. This has been analyzed and discussed by Armijo et al. (2005), based on a high resolution multibeam survey (see also Uçarcus' PhD, 2010). I agree with the fact that we could use a non vertical pitch (rather 70°, which is proposed in the new version). Concerning the approx. 15°- rake (observed onboard R/V Atalante using Victor ROV; Armijo et al., 2005, Fig. 9), we discarded their availability for the small studied portion, for two reasons - and we rewrote this part more clearly -, because: 1) they probably concern a deep submerged prolongation of the Ganos Fault (Armijo et al., 2005) (far from the studied zone) and the vertical component is reverse , 2) it seems difficult to apply this kinematic only along the small analyzed portion.

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