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Interactive Comment

Interactive comment on "GPS derived ground motions (2005–2014) within the Gulf of Mexico region referred to a stable Gulf of Mexico reference frame" *by* J. Yu and G. Wang

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Review of NHESS-manuscript "GPS derived ground motions..." by Yu & Wang

Yu & Wang (re-)analyze a large number of GPS time series from the larger Gulf of Mexico area and present a new, regional geodetic reference frame (SGOMRF) that could become a valuable tool for researchers as well as other stakeholders within this region. I should stress, however, that I am by no means a GPS-expert; my review therefore focuses on the broader implications of this work.

The authors use 450 continuous GPS records which to my knowledge is an unprece-

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Interactive Discussion

Discussion Paper

dented number for this part of the world, although only a subset actually appears to be used in the manuscript (the Abstract mentions 161 records – please clarify). I found the combined horizontal and vertical velocity analysis in relation to subsidence bowls associated with groundwater extraction intriguing. I suspect others may have observed similar phenomena in the past, here or elsewhere, or is this a novel finding? This should be addressed, presumably by means of references. This also brings me to what I see as the main weakness of the study in its present form: the lack of recognition of the recent literature on the subject. This leads to several outdated (and in some cases erroneous) interpretations of the notoriously complex subsidence problem. This aspect will therefore require a great deal of attention to make the work suitable for publication.

A glaring omission is the recent paper by Karegar et al. (June 2015 issue of Geology) that focuses on Louisiana; not in the least because some findings in the present study (e.g., uplift at three sites in northern Louisiana) contradict Karegar et al. On the other hand, the results presented by Yu & Wang for coastal Louisiana are generally quite similar to those from Karegar et al., with BVHS as a notable exception. These things should be addressed. Since the Karegar et al. paper is the successor of Dokka et al. (2006), it should also be noted that this more recent study has abandoned the notion of a "South Louisiana allochthon". It is increasingly accepted that the dominant contributor to land-surface subsidence (potentially including horizontal motions) is the compaction of shallow strata. As an additional note, the authors may want to know that a manuscript currently in revision with Basin Research shows that time-averaged millennial-scale fault slip rates in the perceived "breakaway zone" are ∼10ˆ-2 to 10ˆ-1 mm/yr. In other words, to the extent that the breakaway concept has any legs, the associated rates are likely slow enough that they are largely irrelevant from a natural hazard perspective.

The authors repeatedly cite Ivins et al. (2007) (notably on page 6665) but they cannot do so without fully addressing the findings by Wolstencroft et al. (2014). This latter paper showed that the former study was flawed due to a series of unrealistic input data and assumptions. I'll limit my comments here to one example: the authors mention the influence of delta-lobe switching as a possible driver of differential subsidence rates in the Mississippi Delta. However, Ivins et al. did not explicitly examine the effect of the switching of individual delta lobes on spatially variable subsidence rates across the delta plain. In contrast, Wolstencroft et al. thoroughly investigated this problem, demonstrating that its effect is negligible and beyond the resolution of GPS records. They used a sophisticated delta-load model and showed that the Ivins et al. numbers for sediment load volumes are off by an order of magnitude. Again, this is merely one example: there are many more critical issues here that the authors need to correct by careful reading of these two papers. They should also examine Yu et al. (2012, EPSL) which quantifies the differential motion (of the Pleistocene surface, not the land surface) between the Mississippi Delta and other portions of the US Gulf Coast at a very high resolution, providing conclusive evidence that the rates predicted by Ivins et al. are at least an order of magnitude too high. These findings have major implications for the driving mechanisms of subsidence (i.e., deep crustal processes are typically an order of magnitude slower than shallow processes). Finally, as a side note: the explanation provided by the authors for the lower rate at BVHS is exactly opposite to what one might expect if delta-lobe switching was a significant factor, given that this station is located near the currently active depocenter whereas LMCN and GRIS are located on a lobe that was abandoned 600 years ago. But again, the elastic lithosphere in this region is simply too thick (likely >100 km) over this short timescale to allow this process to have a significant footprint.

The authors cite Dokka (2006) as well as his more recent 2011 paper. It should be noted that these two papers differ substantially in their interpretation, with a shift from faulting in the Michoud area to groundwater extraction. After the 2006-paper was published, it was quickly shown (Edrington et al., 2008, GCAGS Transactions) that these particular faults largely ceased to be active after the Middle Miocene. Thus, the Dokka (2011) paper is much more in line with other findings. On a related note, Meckel (2008, QSR) has examined the role of groundwater extraction in coastal Louisiana; this should be acknowledged.

Subsidence rates in SE Louisiana (4-6 mm/yr) are said to be minor (Abstract and page 6669). While these rates may be a lot lower than in some of the other focus areas (notably Mexico City), they are actually alarmingly high given the low-elevation nature of this setting. I don't think this should be downplayed, in particular because of the caveat that these are really minimum rates (see below).

My final comment is not just targeted at this manuscript but pertains to GPS-studies in subsiding coastal settings more broadly. In order to enable a comprehensive interpretation, the nature of the foundation of GPS units needs to be known and related to the local stratigraphy. To their credit, the authors mention the nature of the structures to which GPS units are attached in a few cases, but much more information is needed (notably anchor depth). Importantly, it should be noted (page 6664, bottom line) that GPS stations do NOT record shallow sediment compaction which typically is limited to the uppermost 5 to 10 meters below the land surface. GPS anchors are nearly always deeper (often tens of meters below the surface) and thus do not capture these shallow processes. Therefore, the value of the data presented here (notably for coastal Louisiana) would increase dramatically if specifics about anchor depths were included.

In conclusion, while I feel that the work has considerable potential, the authors have yet to fully place their findings within the context of the recent literature. If they do so, this could become a welcome contribution.

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