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Natural Hazards and Earth System Sciences
Discussions

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

Anonymous Referee #3

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Overview

This paper utilizes a decade of continuous GPS observations within a large region of the southern USA and central Mexico to 1) realize a new regional reference frame and 2) identify anomalous station motions within this region. The concept of a regional reference frame is useful, and the reference frame appears to be well-realized, but the payoff from removing regional-scale tectonics is minimal. Most of the text is devoted to describing station motions in one of several locales (Houston, the Mississippi Delta, Mexico City) and providing explanations for observed motion. Unfortunately, these explanations are for the most part minimally vetted, not grounded in models, and not

backed (except in one case) by complementary data (e.g. InSAR) that could provide spatial context. Finally, the point of using spatially sparse GPS data is its unmatched temporal coverage and resolution, yet the authors barely mention this application of the time series, focusing instead on secular rates over the analysis period. Rather than use GPS to (poorly) infer spatial characteristics of aquifer drawdown, for instance, the authors could instead use the GPS to provide fine-scale temporal context for InSAR-derived subsidence maps.

Major comments

p6656. There is no need to throw out stations with jumps in data (criterion 1). These can be estimated and removed.

p6656. The treatment of uncertainty in velocity, here in criterion 3 and elsewhere is ambiguous. If this is the standard error of the slope estimate (which is what it should be), then sigma emphatically does not "represent the average difference between the observed values and the regression line." That would be the average misfit of the residuals, which is something else entirely.

p6658. The authors go into tremendous detail describing transformation basics in pp6657-6658, then leave out the most important part, which is how they actually obtained the SGOMRF parameters relative to IGS08. The basics should be in a reference, not the heart of this section. At a minimum, the technique used in Wang et al., 2014 should be outlined.

p6659, line 8. Even the core stations that make up IGS08 will almost always have some residual velocity relative to that frame. This is because a no-net-rotation frame like IGS08 has to balance plate motion globally. So stating that a site is "not stable w.r.t. IGS08" is nonsensical.

p6659. The several threads of discussion about the "stability of the reference frame" suggests a misunderstanding of the definition of frame stability. A frame is stable when

its parameters (origin, orientation, scale and their first derivatives) do not drift or exhibit jumps over time. This is determined by the number, geometry and correlation of errors in the stations that comprise the frame, and cannot be discerned from the residual between frame-estimated and actual velocity estimates at GPS sites. (For instance, actual velocity errors could be really high, but if those errors are random and there are a large number of reference stations, the frame would be perfectly stable).

p6661. The two references about Houston subsidence, which are papers by the authors, not only present evidence that contradicts the explanation for the horizontal motions of ROD1 and TXCN, but those references are inconsistent with each other. This is a place where InSAR would provide invaluable context.

p6662. Dokka et al.'s (2006) SLA detachment, of which much is made in the text to describe the motions of a single site (FSHS), would actually impact all the sites in the Mississippi delta region shown in Figure 9a. To invoke this explanation for one station while ignoring its impact on all the rest is not a rigorous analysis. In fact, FSHS's unusually large seasonal motion in the east component (it is mounted to a building) suggests that site-specific effects are in play. Here, too, InSAR would be invaluable, as would a more judicious application of Occam's Razor.

p6664, line 27. There is a general seaward increase in rate of subsidence in the Mississippi delta (which is a nice observation), but Figure 9 is disingenuous in how it presents these time series. They should be plotted in order from south (bottom) to north (top) so that the actual change in rate can be assessed by the reader. Right now, the plot is by rate, which can easily be misinterpreted by readers who do not carefully check the order in which the stations appear in the plot versus their position on the accompanying map.

p6665. The significant discussion about lobe switching in the Mississippi River delta and its effect on subsidence rates is ungrounded in fact. There is not nearly enough spatial resolution in the GPS to discuss variation between lobes of the delta without

something like InSAR to back up this claim. If contemporaneous subsidence rates were confirmed to be spatially distributed by lobe, then the discussion would be justified and fascinating. Right now, it is taking what could be a site effect at BVHS and improperly spinning it into a geophysical story.

Figures. For all plots of time series within a single figure, the time axes should be identical (even if different stations span different periods), the vertical axis for N/E should be the same for each station and between stations (even if this means de-meaning time series), and the vertical axis for U should be the same between stations if possible (if not, it should be pointed out in the caption).

Minor comments

p6654. The north-to-south, etc., conventions are unusual and non-intuitive. These displacements are positive-N/E/U, and should be presented this way.

p6655. It would be helpful if the authors would spend a sentences or two describing their outlier removal algorithm.

p6657. Equation (3) would be much more intuitive in matrix-vector form.

p6658. More than one reference for an author in a single year should be designated with a lower-case letter (e.g. 2014a, 2014b)

p6659, line 3. This should say "near-zero vertical velocities"

p6659, line 15. In fact, common-mode errors are not "easily and incorrectly interpreted as the result of local faulting activities." Local faulting generates local displacement gradients, while common-mode frame errors do not.

p6660, line 8. "The rigidity of the GOM region" is not standard terminology. Something about internal deformation of the entire region being minimal at the regional scale would suffice.

p6661, line 2. If "a rapid-subsidence bowl is forming around The Woodlands area" it

should be shown on Figure 6. Better would be to use a figure showing this subsidence from one of the authors' earlier papers as a base plot.

p6661, line 24. Steady horizontal movement does not imply anything about the location of a station relative to a subsidence region unless independent evidence 1) corroborates the subsidence and 2) rules out other explanations, such as local tectonics.

p6662, line 26. The short discussion about TXPR appears out of place here. It does not add anything and could be removed if the authors wished.

p6663. The long discussion about growth faults (which appears to say that GPS doesn't help understand them) does not appear to belong in this section. It would be better situated in the introduction or conclusion.

p6667, line 7. This sounds like salt dome uplift should result in "considerable vertical movement," but that it was not observed by the GPS used here. In fact, the next few lines suggest that the uplift rate is negligible. A slight revision of the text will clarify this.

p6669. The UNAVCO website has an acknowledgement statement for use of its GPS data that properly calls out its own sponsors.

Figure 6. This is a near-duplicate of Figure 9b, with less data.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6651, 2015.