



Interactive comment on “Reprocessed height time series of GPS stations at tide gauges” by S. Rudenko et al.

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General Comments ————— This paper is a useful description of the work done by the GFZ group to reprocess a long time series of GPS data to derive improved estimates of the rates of change of station heights. These rates are relevant for correction of land changes at those stations located nearby to tide gauges in order to infer the absolute rate of sea level change. The authors have done a good job of describing their analysis procedures and provide a nice overview of the results obtained. Of particular technical interest for some readers will be a selection of station case studies that illustrate some of the complexities of data interpretation.

Especially since this level of processing documentation is generally rather sparse, I recommend that the manuscript be accepted for publication. On the other hand, readers

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interested only in the results as they apply to tide gauge correction or other direct geophysical applications might be disappointed. Such an objection should be overlooked. However, it would be valid to ask how effective the derived land rates are if applied as corrections to the colocated tide gauge trends, as has been done in the works by Woepplmann and colleagues. This key objective is ultimately not reached here.

While the manuscript is readable, it is not really well written. Numerous corrections and improvements are listed below.

Detailed Remarks ————— * title – “for GPS” would be better than “of GPS”

* p. 1026, l. 2 – “at the time” -> “for the time” or “over the time”

* l. 3-4 – “used processing algorithm and models” -> “processing algorithms and models used”

* l. 17 – “totally” -> “totaling”

* l. 26 – “switch” -> “the switch”

* p. 1027, l. 3 – Is it really correct to say “made possible a reprocessing within the TIGA project” rather than to say “made possible a reprocessing within the IGS”? I am not aware of any real TIGA reprocessing effort, whereas the IGS Analysis Centers have done so in a fairly consistent and coordinated way in the context of the IGS core products. Quite honestly, the TIGA project seems more like a virtual organization that is mostly concerned with operations at tide gauge stations rather than anything related to data analysis, at least in terms of visible results.

* l. 4 – “Different authors” -> “Various authors”

* l. 6 – After “globally”, I would be add a 2nd reference to Woepplmann et al., 2007, Global and Planetary Change, 57, 396-406, due to the significance of this paper.

* l. 6 – “King et al., 2010” -> “2010”. However, there is no transition from the previous sentence to this one, which shifts from past work related to sea level monitoring to

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sources of error in GPS time series. Concerning the latter topic, probably a great deal more could be said than just mentioning the work by King and Watson, which is mostly simulation. For instance, several studies have been made of colored noise errors and how this affects the accuracy of vertical velocity estimates. Other research has also been done on various types of systematic errors affecting station position estimates (e.g., troposphere and ionosphere modeling errors). It seems strange to pick just one item to mention here.

* l. 12 – "(Bouin and Woepplmann, 2010)" -> "(2010)"

* l. 12-15 – This part makes it seem as though corrections of tide gauge rates using colocated GPS vertical velocity measurements is not needed since there is such good agreement to begin with.

* l. 20-21 – "at about" -> "for about a"

* p. 1028, l. 2 – "the obtained results" -> "the results obtained"

* l. 17 – "Terrestrial" -> "The terrestrial"

* l. 20 – "using NNR-NUVEL1A" -> "using the NNR-NUVEL1A"

* l. 21 – "the rest" -> "the remaining"

* p. 1029, l. 8 – The satellite antenna offsets are really adjusted every day? This does not seem correct or wise, without some rather tight a priori constraint.

* l. 9 – Fixing of UT1 only for the 1st day of the week must imply that the daily LOD estimates are required to be continuous between days, right? If so, then this needs to be made explicit, as well as any other over-constraints on other parameters.

* l. 22-24 – Is it really correct to say that AS was turned off (NOTE: not "of") in May 2005, or is the intent to say that Selective Availability was turned off in May 2000? I would not agree that it is "clear" that the stability improvements can be correlated with any abrupt event. The dU performance looks relatively stable from 2001 onward,

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whereas the horizontal stabilities gradually improved over the entire data span. It does appear that the results became markedly less noisy at about 2004.0; is there any event in the data analysis that might explain this?

* l. 25 – "equiupment" -> "equipment"

* l. 27 – The cited reference to Dow et al. does not mention anything about the IGS combined Repro1 solution.

* p. 1030, l. 1-2 – "The accuracy of North ..." -> "The accuracy of weekly North ...". However, use of the strong term "accuracy" is not justified on the basis of the comparison shown in Fig. 4. For one thing, this just shows how GT1 compares with the IGS combined solution, which include GT1. All common mode errors are invisible in such a comparison. It would be much better to use the term "precision" though this is probably an overstatement too.

* l. 6 – "Lenght" -> "Length"

* l. 15-16 – It is well known that the formal standard deviations from GPS data analyses do not accurately reflect that actual measurement errors, which tend to follow flicker noise statistics. While use of the formal standard deviations might be OK for weights to estimate linear trends, they would definitely not be OK to compute accurate errors for the velocities.

* l. 16-19 – Does the trend change detection also find discontinuities in the position time series? If so, this should be mentioned; if not, then the procedure used to find such breaks should be explained, whether supported by known station events or unknown.

* p. 1031, l. 1 – Does "No atmospheric corrections" mean "No atmospheric loading corrections"? Some further elaboration is needed to explain exactly how the results for BRAZ, VAAS, and MAR5 were "corrected". It should be explained that these are special cases that are considered in detail in section 4.

* l. 6 – "(Blewitt et al., 2002)" -> "(2002)"

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* p. 1034, section 4.3.1 – Most commonly, post-seismic trends are modeled with exponential functions rather than linear segments. The proposed rate change in 2005 might just be an artifact of using linear segments.

* l. 18 – Need a reference for "Sato".

* p. 1035, l. 7 – "(Emery et al., 1991)" -> "(1991)"

* l. 11-13 – The trends discussed here presumably refer to trends in some tide gauge data, though this is not stated explicitly.

* p. 1037, l. 16 – "utilises" -> "that utilises"

Note that Table 3 also shows that the formal velocity errors from this study are much, much more optimistic than those of Bouin and Woepplmann, which is most likely because no account was made of correlated measurement errors here.

* l. 19 – "at 10 yr" -> "over the 10 yr"

* l. 26 – "comparion" -> "comparison"

* references – CDEW (1990) is mentioned several times in the article but is not listed in the reference section.

* p. 1043, caption, l. 2 – "larger than" -> "longer than"

Interactive comment on Solid Earth Discuss., 4, 1025, 2012.