Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-133-RC1, 2019

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Interactive comment on "Asthenospheric anelasticity effects on ocean tide loading in the East China Sea region observed with GPS" *by* Junjie Wang et al.

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Received and published: 6 October 2019

This paper is a follow-up to earlier work by this group (e.g., Bos et al., 2015) on anelastic effects in tidal loading, a long-sought goal of the earth-tide community. It is important work, and this is a carefully done study. Figure 1 will be immediately useful to many researchers. I have only minor comments, with the exception of my first point, which might be more important.

(1) I want to bring up a possible systematic error which, if not addressed here, needs to be addressed at some point by this group as they continue to do these kinds of studies. I think it could cause errors of a few percent, which could be significant. Specifically,

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I'm beginning to think these very precise geodetic applications need to account for the variable density of seawater – that rho in Eqns (1-2) should stay inside the integral sign.

My 2013 paper (already cited by the authors) has a section (Section 2.4) on this in the context of bottom pressure measurements, and elsewhere (Ray et al., JGR, 2009 – doi:10.1029/2009JC005362) we've seen evidence that GRACE may be sensitive to it (although at this stage, errors in the ocean tide models themselves still dominate GRACE sensitivity). A version of Duncan Agnew's SPOTL package does account for variable seawater density, but initially it was using the density at the ocean surface (I'm not sure about his most recent version). But a rising tide is caused by convergence through the whole water column, so for this reason, in the 2009 paper, I used the mean column density. After discussions with Chris Garrett, I think compressibility is also involved, and my 2013 paper uses an expression that Chris worked out (involving the speed of sound). In practice, for the ocean we currently have, Chris's expression is numerically about equal to the density at the seafloor.

I won't insist the authors investigate this effect here in this paper, but before they publish too many of these kinds of studies, it would be worth looking into.

Other minor things:

(2) Abstract, line 13, recommend inserting "the regional model" before NAO99Jb, because many readers, even in the tide community, may see NAO99 and think it refers to Matsumoto's global altimeter-based model.

(3) Next line: "the most accurate". Well, this is risky because who knows if someone has developed another regional model here. I'd say "an accurate" – but it's up to the authors.

(4) In Abstract, and also page 15, bottom, authors quote 1.5 mm and 0.8 mm. I don't understand this. From Table 4, this looks to be "comparing apples and oranges". One is a maximum error and the other is RMS. Seems misleading, unless I'm just not following

where they get these numbers.

(5) Page 13, line 5: Dahlen & Tromp is a massive book. I and many readers would appreciate your quoting the page number or even the Eqn number you're using when you cite this book.

(6) Page 2, line 36, where GPS is assigned an error of 0.3 mm. I don't accept this, because surely the errors in GPS are dependent on the length of the time series.

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