Review of *"Glacial Isostatic Adjustment modelling: historical perspectives, recent advances, and future directions"* by Pippa L. Whitehouse, submitted to: Earth Surf. Dynam. Discuss.

1. General comments.

This is a comprehensive review of the Glacial Isostatic Adjustment (GIA) problem, in which the emphasis is about the historic development, the recent advances and possible future directions. Despite the word 'modelling' appears in the title, there is little about the technical aspects of GIA and as a consequence the approach is rather qualitative and focussed on the description of the geophysical processes involved in GIA. A series of nice illustrations are proposed, useful to understand how GIA operates on global and regional scales, something that is certainly of interest for beginners in this field. The strength of the paper is in its completeness and organisation, which make it very easy to follow despite its considerable length. The weakness is in some missing details and references and sometimes in a too involved description of the various topics. In some parts, the paper seems to have been written in a rush, so that some smoothing and re-thinking is recommended before it can be considered for publication.

I have a series of minor to moderate remarks, various considerations, notes and hints, listed in the following, which I hope are useful to improve an already very good contribution.

2. Specific comments

Line 1ff. In some previous works (e.g., DOI:10.1007/s10712-016-9379-x), a distinction is made between the Earth's response to past ice melting and that to present ice melting. Of course the physical principles are the same, but a few explicatory words can be of help. For example, when dealing with low-viscosity regions in Section 4.3, it should be clear that in this case we are dealing with the GIA caused by recent melting.

L6. Not only to 'global' ice sheets; in some parts GIA is in response to regional ice sheets (e.g., again Section 4.3).

L7. Actually, GIA 'follows' the entire history of glaciations, which is 100s of kyrs long. I do not think GIA should only be viewed only as the response to glacial unloading. Perhaps some words to distinguish between 'post-glacial rebound' and GIA can help, in this respect.

L18. (LGM, ~21,000 years ago).

L44. GIA can be "seen" (and it was actually seen) by looking at the migration of the shorelines in northern Europe, as summarised here. However, in the whole paper there is not so much attention about the general effects of GIA on the time evolution of the past Earth's topography (e.g. former "land bridges" are only mentioned at L759). May be it could be useful to look at the literature of contemporary GIA investigators, in this respect. I am sure that a little section about this important aspect of GIA modelling would add some value to this review.

L52ff. I suggest to change item (viii) into “interpreting the gravity field of the Earth and its rotational state” or something like that.

L60ff. This is a remarkable historical overview, indeed. I have some miscellaneous observations. *i)* Did Jamieson only consider elastic deformation? *ii)* Was his work influenced by the ideas of Airy about isostasy? *iii)* It is probably important to mention, for later comparisons, the famous Haskell value for mantle viscosity. *iv)* Regarding uniform viscosity Earth models, the Darwin spherical model has had a role in the development of GIA models (see the review of Peltier, 1974, who should be quoted e.g., at L176 between O’Connell and Chatles, in my opinion). *v)* I am surprised to see no mention to the fundamental work of Love, who with his “Love numbers” certainly prepared the advent of the modern GIA models, starting from Farrell and Clark (1976).

L175. I think ‘is the equipotential’ is better than ‘is an equipotential’.

L176. This can be shortened into ‘in the absence of winds and currents’.

L180ff. This is nice illustration, in words, of the concept behind the ‘k’ Love number.

L187. I would suggest ‘constrained’ *in lieu* of ‘determined’.

L190ff. The role of the sea level equation (SLE) in GIA modelling has been recently reviewed in detail in DOI:10.1007/s10712-016-9379-x.

L190ff. Eq. (1) and the material that follows is fairly good, but it can be improved, I have a suggestion. Why not starting with the SLE in the native form *S=N-U* (new equation 1)? This would be helpful, since in this way one can define relative sea level change (*S*) and absolute sea level change (*N*), and vertical displacement (*U*) since the onset. These are quantities of fundamental importance for a full understanding of the remainder of the paper. If this is agreed, Eq. (1) becomes Eq. (2) and so forth… Also, the unnecessarily cumbersome symbol DeltaSL could simply become *S* and so on al L196 and L201 (and in other places, I think).

L194. Given that the unknown in the sea-level equation is *S* (provided that the history of the changes in ice thickness *I* is assumed to be known), one may wonder how surface displacements *U* or horizontal displacements *V* are obtained for e.g. comparison with GNSS data, discussed later in the paper.

L196: … is the change in *relative* sea level…

L201. This can be rearranged into: … are convolutions in time (over the ice sheets history) and in space (over the surface of the ice sheets and of the oceans….).

L204. It is not actually a uniform shift 'in the geoid', it is a uniform shift in *relative* sea level, according to the equation labelled by (1) in the present form of the paper.

L209. The definition of eustatic sea level is important in GIA modelling, but here it is only mentioned *en passant*. Seen the beautiful historical Section 2.1, some words could be certainly spent on the work of Edward Suess on the concept of eustasy in his book *"La Face de la Terre"*. To be completely fair (and admittedly pedantic), eustasy is a static concept, only dependent on the amount of melt water change, on the density of water and on the area of the oceans surface. However, in his Eq. (2), the author has accounted for variations of the area of the oceans by a time-dependent A_o(t). This is a dynamic effect, however, since it also depends e.g. on Earth's rheology. So, I suggest to warn the reader about this caveat, or to re-write the SLE using an effectively constant A_o. I would personally prefer the first solution. I also note that in the recent literature the term 'eustatic' has been substituted by '*barystatic*' following the work of Gregory et al. (2013), something that could be mentioned or not. I finally observe that when a time-dependent A_o(t) is accounted for (this is the case for migrating shorelines), the sea level equation becomes non-linear; otherwise it is linear for an effectively constant A_o. This an important qualitative difference that should be mentioned, in my opinion.

L212. They are not really 'perturbations'; maybe they are 'terms'.

L222. The Maxwell rheology is describing a fluid behaviour, not a solid behaviour. In fact, in a creep experiment, just immediately after the instantaneous elastic response, the dashpot of the Maxwell body works as a Newtonian viscous fluid. The same at L223, and in other places like L570, for instance.

L226. If a power law is assumed, the Love numbers formalism is not viable since the problem becomes non-linear.

L234. The lithosphere is far from being purely elastic (see e.g. Ranalli, *The Rheology of the Earth,* or DOI: 10.1029/RG021i006p01458, or the papers by Burov, e.g., DOI: 10.1029/94JB02770). The main point is "why the elastic lithosphere approximation is so common in GIA studies"? (despite the evidence for a complex rheological profile).

L250. What 'self consistent' means here?

L247ff. All these extensions of the sea level equation make it a non-linear equation; this is an important point. See also my comment to L209 above.

L256ff. It can be worth to say that the effects of Earth rotation introduce a very long wavelength pattern mostly characterised by harmonic degree l=2 and order m= +/-1 terms. This high-energy component of sea-level change is clearly visible in the form of large lobes in the maps GIA fingerprints, see e.g, http://dx.doi.org/10.1016/j.gloplacha.2016.05.006.

L256ff. I would rephrase as follows “Since GIA alters the mass distribution of the Earth, it changes its (off-diagonal) moments of inertia, which in turn perturbs…” or similar.

L261. Why ‘over longer time scale’? Actually, rotationally induced Earth deformations also occur on short time scales, even elastically (i.e., instantaneously).

L262. Again, I do not capture the rationale for separating short from long time scales.

L274. I would avoid the term ‘transient’, since this term can also refer to non steady-state (e.g., Burgers) rheological effects. I think that ‘time-dependent’ could be a possible alternative.

L285. I am not sure that the meltwater fingerprints can be immediately associated to (or ‘based on’) the theory of Woodward. Indeed, Woodward used a rigid Earth and ignored the oceans self-attraction (only accounting for the gravitational attraction between the point ice load and the ocean mass). Hence, his fingerprints are approximations of the ‘modern ones’ based on a more realistic modelling.

L285 and L289. The term ‘fingerprint’ in the GIA context has been coined by Plag and Jüettner (2001) and adopted in numerous studies since then (see Plag HP, Juettner HU Inversion of global tide gauge data for present-day ice load changes - scientific paper - Mem Natl Inst Polar Res 54:301 special issue, 2001), and this should be fully acknowledged.

L293ff. One may simply wonder *why* peripheral bulges regions exist!

L297. If my hints at L190ff above are followed, the reader will be greatly facilitated here, where the concept of absolute sea level is utilised. Why ‘mean’? I do not think it is necessary.

L300 and 301. I am afraid I do not understand this couple of lines. Global (absolute) sea-level change obtained from altimetry is normally corrected for the effects of GIA, so ocean syphoning and all the other processes involved are certainly taken into account. I think the paper of Tamisiea (2011) is dealing with other aspects of GIA. It is possible, however, that I am missing the point, here.

L305. The ‘continental levering’ effect should be mostly visible far from the polar regions, right?

L312ff. Perhaps, it can be of interest to note that before the advent of the so-called pseudo-spectral method, a fully spectral approach was utilised (see e.g., the paper by Plag and Juettner quoted above). Regarding the methods in general, it could be instructive to explain *why* certain methods are used instead of others. For instance, the pseudo-spectral method is now standard when dealing with spherically symmetric Earth models with linear rheology. Similarly, the finite-element approach of Wu and coworkers is motivated by the introduction of a power-law rheology (for which the superposition principle does not hold), and so on...

L320. Here and in other places, the word ‘self-consistent’ should be used in a more specific way. I think that in the context of GIA the term ‘gravitationally self-consistent’ has been introduced to say e.g. that the change in the shape of the oceans determined by solving the sea level equation is consistent with the gravitational field (the oceans surface is an equipotential ad mass conservation is ensured). Similarly, ‘topographically self consistent’ indicates that the solution of the sea level equation for a variable topography is consistent with the present day topography (and with the gravity field). See the works of WR Peltier, where (I think) this terminology has been introduced first. See also point L250.

L321. I do not think that *all* integral equations need to be solved by iteration. Iteration is often invoked to solve non-homogeneous Fredholm equation of the second kind, which is the type of equation the sea level equation is. In any case, most importantly, the physical reason for which iterations are needed is that the change on the ocean mass distribution is not known a priori, contrary to the ice distribution.

L322. The Section on Data is very smooth. Regarding 2.2.3, I only observe that before GPS data, very long baseline radio-interferometry (VLBI) data have also been employed to test GIA models and to constrain the Earth’s viscosity profile. This can be traced easily in the literature.

L400ff. I do not fully agree on the role of horizontal GPS observations; their recognised sensitivity to the shallow upper mantle rheology and to the presence of lateral variations in the Earth’s mantle properties should be regarded as an advantage, not as a limitation.

L402ff. Similarly, I do not think GIA model predictions are typically provided on a reference frame whose origin lies at the centre of mass of the solid Earth. Quite often, instead, they are given in the reference frame of the center of mass of the whole Earth system, so that they can be directly compared to geodetic information (actually, in GIA modelling the transformation between the two frames is straightforward). I agree on the reference frame origin uncertainties, which is indeed a major problem.

L416ff. This is true, but I think the main problem with GRACE is that it observes gravity field variations due to all surface and internal sources (including e.g. mantle dynamics associated with non-GIA processes, post-seismic deformations, and so on). I also note that some very useful insight into the actual meaning of GRACE data has been recently given by BF Chao in DOI:10.1007/s00190-016-0912-y.

L444ff. I am not really getting the point here. Can examples and/or citations be given to support or explain this sentence?

L454. As far as I know, a possible interaction between GIA and seismicity has been first proposed by Gutenberg and Richter in their book *Seismicity of the Earth and Associated*

Phenomena (1949), and there is a long story of relevant contributions to this field since the 90s, probably listed in the bibliography of Dr. Steffen's works. Some considerations along these lines could help to put flesh on the bones of this succinct subsection.

L467. I agree that the definition of intervals for the viscosity of the upper and lower mantle has been (and is) a key-result in GIA modelling. But I also think that the recognition that GIA data requires a rheologically layered mantle (i.e. a viscosity jump between the upper and a the lower mantle) is even more important. It would be very useful to trace this key-result in the literature.

L475ff. Although the physical process is the same, it is important to let the readers know that contrary to the studies mentioned at ~L470, in those quoted here the source of GIA is recent ice melting from small size glaciers and ice caps. By the way, I definitively agree that defining these low-viscosity layers is a key result of GIA modelling.

L479-L491. This paragraph is OK, but still it is difficult to see sharp key results. Itemisation can help? May be the text can be modified accordingly. In any case, since the 70s the Toronto University GIA school has given important contributions into the definition of the ice volume change since the LGM with the help of GIA modelling, and this should be fully recognised, in my opinion.

L492ff. I realise that summarising all the attempts to constrain the configuration of individual ice sheets in a short paragraph is not easy. But here one is left with the impression that all the studies mentioned converge to similar results. However this is not the case; for instance, as the author of this manuscript knows, different studies show diverging results for the history of melting chronology of the Antarctic ice sheet during the late Holocene. Actually, resolving this uncertainty could constitute one of the future challenges in GIA modelling for Section 4 below.

L501. The sentence should be probably tuned differently for the Greenland and Antarctic Ice Sheets. In any case, GIA corrections are particularly uncertain because of the uncertainties about the melting history of Antarctica.

L522ff. Just a comment. There are uncertainties associated with limited data availability and modelling capability. However, I think that the uncertainties described by Tamisiea (2011) are of different nature, resulting from genuine misunderstanding of the physical meaning of the various terms of the sea-level equation.

L584. Yes, this is an important question. Concerning global GIA models, I do not think we are now in the position of saying that 3D non-Newtonian models perform better that 1D Maxwell models in explaining e.g. the sea-level variations observed during the Holocene. So, I do not know whether an increased model complexity is indeed required.

L615. I am not really an expert in sedimentary isostasy, but I know that sometimes sediment loading and sediment compaction occur at a very small scale. In view of that, is there some indication of the spatial resolution of the GIA models in which these effects are taken into account? Maybe some information could be conveyed to the readers in this respect. I am also curious about how mass is conserved in these sedimentary GIA models, since this would require detailed information about the sediments (re)distribution, etc…

L653. The total mass change should be zero, by mass conservation; probably 'mass redistribution' is meant here? A few lines below (L669): I am missing why an inverse approach is neither dependent on the ice loading history nor from the Earth structure. What does it depend upon? Overall, this whole Section 3.4 is definitively not written for newbies and is somewhat confusing, in my opinion.

L676. Section 4: future directions. This is a very nice (and very personal) view of the possible future developments of GIA modelling. I do not have specific comments on that.

L827. A very short conclusion that sounds a bit vague. I would have preferred a few (better if itemised) statements summarising three or four take home messages.

Hope this helps.