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Interactive comment on “Observation of a local gravity isosurface by airborne LIDAR of Lake Balaton, Hungary” *by* A. Zlinszky et al.

Anonymous Referee #2

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Satellite altimetry has been used for long to measure sea surface height, and from this, to derive the marine geoid. In this contribution, the authors assess the potential of the airborne Lidar technique in order to derive a lake equipotential surface. The authors claim that their approach has the potential to complement terrestrial (classical) techniques of computing geoids.

Technically, I find the paper well-written and containing a lot of information. The authors describe an experiment – they had funding for airborne surveys, they collected a lot of data, and they compute a lake topography that they believe is close to hydrostatic equilibrium, which then does not really come as a surprise. Scientifically, the manuscript appears mediocre as a clear hypothesis is missing. In particular, this reviewer can neither discover an open scientific question to be addressed, nor a result which is sup-

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ported by thorough validation. With a background in physical geodesy, it appears hard for me to grasp how the results can be used for improving an actual geoid model. In fact, it appears the authors had all the information required in hand but did not make proper use of them. More details follow below.

In fact my view might have been somewhat biased from the onset, since already the title of the manuscript is incorrect or at least misleading. The authors write about observing a “gravity isosurface” being observed; later this is equated with geoid determination but this is not correct. A gravity isosurface implies that a surface of constant gravity is observed, such as an isobaric surface implies a surface of constant pressure and so on, however, a geoid is not a surface of constant gravity but of gravity potential. This should be corrected.

Lidar, as radar altimetry, provides the current sea surface which equals to an equipotential surface (not necessarily the local geoid) plus waves plus atmospheric (wind and pressure response), Earth, pole and lake tides plus non-tidal surface variability due to density change, currents, up-and downwelling, river plumes and many other effects – all of this has been observed in large lakes. In order to correct from sea surface to the geoid, all these need to be corrected, and I am missing a sound error budget for this procedure here. Was surface pressure change accounted for at all? Tides? There is a number of foggy statements “low current intensity at the time of flight”, “the turnaround time for water is more that two years, so the flow is very weak”, which appear unsupported by evidence (I cannot even see turnaround time, commonly a measure how long a water parcel stays in the lake, related to geostrophic current intensity)

Even if this may be difficult, the resulting corrected lake surface would have to be compared to a physical reference surface, the local geoid. While a lengthy description is provided on how the Hungarian national geoid has been computed in general, the essential information is missing: how good is the national geoid model around Lake Balaton? An overall predicted accuracy of 2 cm is meaningless in this context. What kind of data did they use over the lake and in the vicinity, and how accurate is the

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geoid? Were aerogravity data over the lake used?

With the geoid, and with the levelling stations connected to the national datum, it is not clear why the authors chose to disregard this information – ok, they want to do it independent – but compute a "reference surface" LMLL from 4 days of observation, of which they then complain it is "less accurate" as a reference surface.

Consequently, many of the authors' claims in the Discussion and Conclusions chapter are totally unsupported. While I have no doubts that Lidar is a valuable technique which should be studied to support physical geodesy, I don't think the authors provide a convincing proof that "ellipsoidal heights measured by Lidar might be used in the future to refine local gravity models".

The Discussion chapter also contains a misleading discussion on how altimetry is closer to GRACE that airborne gravity – all these three techniques work on totally different spectral domains, which means they complement each other but do not compete.

Comparing sea surface measurements to a geoid is complicated by the fact that the geoid itself is derived by integration over terrestrial gravity measurements which are usually sparse over water surfaces. A path that altimeter people went for validation, and that might be useful for assessing Lidar capabilities, would be to numerically derive marine gravity anomalies over the lake. These could be compared with ship-borne gravity measurements, should they exist over Lake Balaton.

On balance, I believe the manuscript may be published after taking the above into account, and after a through rewriting the Discussion and Conclusions chapter, but if the authors chose to withdraw the manuscript the loss would not be terrible.

Interactive comment on Solid Earth Discuss., 6, 119, 2014.