Review of Ph.D. thesis

Forward and Inverse Modeling of Planetary Gravity and Topography

by Martin Pauer

I provided the review on this thesis in September 16, 2008. Since the structure and results of the original text have not changed substantially, I will choose and refer to those parts of my original review that are relevant to the current version of the thesis.

This Ph.D. thesis is concerned with a classical question of geophysics: What can we learn about the internal structure of a planet from knowledge of its external gravity field and the surface topography? To answer it, the thesis first outlines gravity-field theory, then derives the theories of elastic isostatic and dynamic viscous mechanisms for compensating the surface topography of a planet and generating a part of its external gravitational field, then summarizes the basic concepts for inverting the gravity field often used in planetary science, and finally presents three applications of the derived concepts for interpreting the gravity field of two planets and a moon of the Solar System.

A general scientific conclusion one may make after reading the thesis is similar to what is known when inverting the gravity field of the Earth. Without having additional information about the planetary body's internal structure, for example, seismological evidence, the interpretation of the external gravity field is a badly posed inverse problem such that only the most general aspects of an internal structure can be learned. But the inverse gravimetric problem is also badly posed when interpreting the gravitational field in terms of a surface mass distribution, as has been shown for the Earth's gravity field when interpreting data from GRACE.

In 2008 I wrote.

I appreciate M. Pauer's effort to provide a comprehensive overview about the problem of interpreting the gravity field and topography of a planet. Consequently, it has resulted in a rather long text, but with a clear structure, always outlying the scope of the chapter at its beginning. By referring to many literature sources, M. Pauer has demonstrated that he has a comprehensive knowledge of previously published results.

This statement is still valid but with the constraint that the papers in Bibliography are listed up to the middle of 2008. There is no paper in Bibliography published during last 5 years. This is somewhat documented by the statement 'in press' at Hütting and Stemmer (2008) reference.

In 2008 I wrote.

On the other hand, for me it is difficult to judge his own contribution to the studied problem. Reading section 5.1, I got an impression that M. Pauer has taken existing concepts and applied it to a given planet. Surely, nothing is wrong on applied science, but I wonder if he, as the graduate of theoretical geophysics, has also made some original theoretical work? May I consider the theory given in section 3.4.1 to be his own theoretical contribution?

I my view, this point has changed. The thesis was shorten and the own contribution of M. Pauer is presented in new chapter 5.

In my 2008 review I raised a couple of comments to the text. Most of them were implemented in the revised text, but still a few are to be clarified.

Originally: p.8. Eqs. (2.34) and (2.35) are wrong. How can the condition $r < r'$ in (2.34) be satisfied if the integration over r' in (2.34) runs through the whole body as seen from (2.30).

From the condition $r > r'$ in (2.35), one may draw a conclusion that (2.35) is only applicable at an external point. However, (2.35) can also be applied at an internal point. Why? Can the author write the correct forms of (2.34) and (2.35) when presenting the thesis. **Now:** p.18. The conditions $r < r'$ in (2.31) and $r > r'$ in (2.32) are irrelevant. They should be relaxed form the equations.

Originally: p.34. I have severe doubts about eqs. (3.69) and (3.70). First, (A.33) does not give the right-hand side of (3.69). Second, the terms in (A.34) differ much more significantly than by the factor $\sqrt{\ell/\ell+1}$, as claimed after (A.34). This fact is valid for the normal component (A.33), if the term before the last one in (A.33) is multiplied by factor $1/2\ell+1$, which is probably a typing error. If it is so, one boundary condition for tangential stresses is missing. Leaving out one boundary condition which is linearly independent of others should be properly explained during the defense. **Now:** The explanation in the last paragraph on p.43 is still somewhat unclear. Why does the term with $\boldsymbol{Y}^{\ell-1}$ is only considered in arranging the boundary condition (3.52) and the other term at $\boldsymbol{Y}^{\ell+1}$ is left untouched? Why the boundary condition (3.53) is arranged by (A.31), but not with the help of (A.32)?

p.56, eq.(4.16). The multiplication by $t(\vartheta, \varphi)$ seems to be missing.

My conclusion in the 2008 review remains valid.

Conclusion: I have no doubt that M. Pauer is able to work as an independent scientist in a research field dealing with many types of descriptive information. He is able to combine information from various scientific sources, analyze them and draw conclusions. On the other hand, his mathematical considerations suffer from a slight lack of precision and exactness. My recommendation for his future work would be that he should be more mathematically robust than demonstrated in the thesis. Nonetheless, without any reservation, I recommend that the thesis be passed on to a public defense.

August 7, 2013 *Zdeněk Martinec*