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Review
of the PhD thesis by

M.Sc. Paweł Hordyniec

"Modeling of Troposphere Parameters from GNSS Observations to Ground-Based Stations and Low-Earth Orbiters"

for granting the PhD degree

in the field of Technical Sciences in the discipline of Geodesy and Cartography

Overall characteristics of submitted thesis

Thesis of MSc Paweł Hordyniec entitled: "*Modeling of Troposphere Parameters from GNSS Observations to Ground-Based Stations and Low-Earth Orbiters*" consists of the set of four papers. PhD project has been realized under the direction of dr. hab. inż. Witold Rohm, professor of the Wrocław University of Environmental and Life Sciences. MSc Hordyniec has also a co-advisor Dr. Cheng-Yung Huang from National Space Organization, Taiwan.
The book I have received is more then just publications: it begins with two-tier introduction. First we get theoretical part (Chapter 1), and next dissection of each paper separately (identified as A.1, A.2, A.3 & A.4). Each paper is described by a rigid set of topics: Introduction, Summary, Individual contribution (extremely helpful to assess candidates share) ending with Conclusions and Outlook. Chapter 3 embraces main conclusions and prospects (for the whole of research), and is followed by (emphatic) acknowledgements and wide literature.
After included papers we find declarations specifying authors share in each work (Appendix B) and information of other papers co-authored by the candidate (Appendix C).

Such formal scheme is quite transparent and orderly.
Reviewing PhD composed of publications is quite tedious task. In this case candidate really tries to help by including four-part summary of each article.

Manuscripts succession is logical but not chronological. Interesting thing here is timing: paper A.1 is from 2015 but A.2, A.3 and A.4 – all from 2018. It is really tremendous effort to create three voluminous papers in less than one year…
Manuscripts can be considered as a logical entity but topics vary and overlapping is rare.

Thesis concentrates around **two really innovative matters**:
- cloud and precipitation impact on GPS tropospheric estimates;
- developments of algorithms and software for radio occultations (RO) of low orbit satellites (LEO) including: ray tracer/wave optics (both in simulation and operational mode), code-phase signal processing etc.

These are separate problems, but candidate managed to integrate them in the paper A.3

Candidate has excellent knowledge of the literature: most papers he cites he also looked into. Each publication is full of pertinent references. Also "second hand" citations are rare.

First topic is highly intriguing, but as candidate states it is idea developed by his supervisor.
The greatest achievement present in submitted thesis, in my opinion, is not amount of publications but the developed software (based on precise algorithm engineering and tested in several experiments).
This numerical programming work alone is commendable for granting MSc Paweł Hordyniec the title of Philosophy Doctor in technical sciences.
Mentioned software development exploits are: advanced and multilayered system for LEO radio occultations data processing including: phase reconstruction with navigation message bits, development of wave optics software based on complex signal formalism tackled by Fourier integral operators, ground based ray tracer (to obtain STD) and numerically implemented Abel transform and inverse Abel transform etc.
Candidate has chosen very challenging tasks, unprecedented in Poland.
Unfortunately I am not qualified enough to fully appreciate this kind of work. Also this achievement is not properly documented.

As of papers *per se* I am a little less enthusiastic – reasons are given below.

Overview of the Introduction (Chapter 1)

Theoretical introduction can be continued in the future and widely circulated.
Chapter 1.2 is especially valuable, and should be broadened by some physical interpretations of complex refractivity concept and of deeper explanations to cited formulas. In common knowledge Rayleigh and Mie scattering effect only in attenuation like for light waves; this is not the case in microwaves!
The strong part is also history and systematic (even if incomplete) juxtaposition of mapping functions (chapter 1.3). Theoretical introduction to radio occultations is really helpful but too short (chapter 1.4). Equations 1.30 and 1.31 should be explained in depth.

Maybe it is better to begin such introduction with basics. Author emphasizes the lengthening of the signal by bending (beginning of chapters 1.1 and 1.3) only later mentioning the fact that main contribution to tropospheric delay is slowing effect, with geometrical lengthening far behind. For an ignorant reader, who uses this text as the source of information, it is misleading.

Technical/editorial remarks:

Citation for equation 1.1 is not needed – it is simply Snell law. Also citation for equation 1.32 is out of place – theory of ray geometry is very old.
Please mark L-band on the figures 1.2 and 1.3.
Figure 1.4 comes from results of publication A.1 (I believe) but it needs more explanation.

Overview of the manuscripts

Here are some considerations and characteristics of each paper:

Paper A.1:

Paper is rather typical. There are many similar publications. Candidate treats the results as a benchmark to compare in the following papers. But the testing period was too short, and I recommend weighting the results of this particular work with other comparison results mentioned in the literature.
Berg (1948) model is really outdated: it even does not include temperature which moves isobaric surfaces.

Paper A.2:
This paper is extremely interesting; inventive experimental handling of GNSS solutions and meteorological data have been well documented.
Advanced and neatly planned experiment to search for cloud (and rainfall) impact on tropospheric GNSS delay has been made possible only thorough close collaboration of several persons (including meteorologist).
I consider declared share attached to the candidate (50%) as slightly exaggerated.
{Such problem if elaborated independently is enough in itself to qualify for PhD.}
There are some remarks:
In my opinion overblown use of slant delays and ray tracing procedure, when final analyses (Chapters 8.2 and 9) were performed in the ZTD form, artificially complicates the project. I believe the motive here is to underline MSc Hordyniec share (he has calculated slant delays in NWP model). Paper is too complicated: instead of fully focusing on question of cloud/precipitation impact authors also present wide array of STD results and other research.
Information of WRF model is too sparse (is it operational or test run, nowcasting or prognosis, what about boundary conditions, assimilation cycle? etc.). Representation of meteorological parameters in NWP models depend on so many details that ensemble forecasting is operationally employed. Author himself asserts (chapter 8.1) that quality of meteorological parameters in NWP model fields is of crucial importance.
Used WRF version is quite dense (4 km) but narrow in space (model grid map is not presented) – what about stations close to boundary?
Fortunately model run is verified (by co-author, not the candidate), but results are mixed, and in case of precipitation discouraging. A logical move was the usage independent (instead of WRF!) products considering cloud cover and precipitation - but this is work of W. Rohm.
I consider this paper as a demonstration of subtle methodology more then definitive problem solution with final results.
Terminological question: The term „overestimated" is over-used instead of simply "bigger values". We have two observables here: GNSS estimates are bigger, ray tracing is probably better estimate.
Technical remarks: Table 3 does not contain units; Fig. 6 should contain also relative differences.

Paper A.3:

Publication is extremely advanced, narrowly focused. Multiple phase screen (MPS) method of wave optics simulation includes also cloud refractivity. Purpose is to get realistic simulation of RO observations. Effects from clouds on RO bending angle are studiously researched. Question how the spherical asymmetries in atmosphere impact on bending angle is addressed in amazing detail.

It is a pity that MPS approach is presented only on theoretical level, we know quite little about real calculations. {Additional appendix needed for full assessment of this experiment.}

This work uses excellent amount of data.

Share of different cloud fractions (distinction based on temperature) provides also interesting insight into GFS model physics. Thanks to climate zone subdivisions we see lower tropopause height with greater latitude, and numerous ice clouds over equator. Big amount of low clouds over equator surprised me. These meteorological results could be tested against climatological data.

Manuscript A.4:

This publication is based on exceptionally technical and advanced topic of RO processing minutes. Candidate proposes (and tests) some innovative modifications of signal processing to improve contemporary procedures of handling RO LEO occultations (for Taiwanese mission FORMOSASAT-3/COSMIC). Procedure involves phase reconstruction (for both L1 and L2) from residual (atmospheric effect) Doppler observations with use of GPS C/A code. Navigation data modulation (50 Hz) bits are used for open loop data in lower troposphere. Occultation geometry (bending angle) is obtained both by means of geometrical and wave optics. Wave optics is needed to disentangle the incoming rays in multipath conditions. Next bending angle changes are inverted to refractivity profile (for each occultation) by numerical Abel transform. In the end meteorological parameters obtained by profile reconstruction is verified with radiosounding data and compared with standard procedure conducted routinely by Taiwan project.

I have some problems to discern where the input of Taiwan researchers ends (TACC/CDAAC products description) and original work begins (in chapters 4.1, 4.2, 4.3).

Equation 17 and 18 are empirical. There are also different formulas for water vapour pressure calculations. Also Smith-Weintraub refractivity expansion is quite old. Rüeger (2002; mentioned in the candidate own literature) provides discussion on refractivity models.

In case of manuscript A.4 (and to some degree also A.3) author tries to attain two divergent aims. Candidate needs to present his developed system for radiooccultations of LEO satellites and experimental verification details. On the other hand he strives to create publication valuable for a geophysical journal.

Despite great effort this compromise works in a way far from intended.

Technical report of developed software, algorithms used, numerical tests documentation, results verification is one thing. Paper in a general purpose geophysical journal ('Terrestrial, Atmospheric and Oceanic Sciences') is quite another. For a successful publication we need clear message, interesting results about natural phenomena.

This crucial dilemma is tackled in the end of next chapter of this review.

Manuscript in the present form would be perfect match for hypothetical journals: "Radio Occultations" or "GNSS Measurements in Space".

Discussion considering formula and overall features of PhD thesis

I have some overall (maybe personal) impression from the lecture of this particular PhD. Let me summarize the style of candidate's papers and research:

a) extremely advanced numerical methods (algorithm and programming dexterity of software developer),
b) problems geometry and signal processing tackled in extraordinary detailed and systematic way,
c) procedures tested on relatively small data set (in relation to time or space distribution),
d) rich vocabulary but the language too complex, meticulous, official (to fit into journal requirements, but with some exaggeration).

First two points are positive so I see no reason to explore them.
Last two need explanation:

Ad. c) Relatively small sets of input data in some experiments are seriously concerning me. Detailed statistical and theoretical analysis (e.g. Chapter 8.2 in A.2) cannot replace hard empirics. We cannot always assume normal distribution of error contributors.
In case of GNSS meteorology even more then classical meteorology observables and processes are so entangled that formal analysis is nearly useless. Coda of papers A.2 and A.3 is that untypical phenomena (rainfall, cloudiness) introduce unanticipated results.

The danger of sparse databases in numerical experiments is alleviated by excellent knowledge of literature. I think that whenever candidate achieves (in such small database) results corroborated by papers of the others he feels satisfied.

In my outright opinion in case of presented here complicated computational systems (ray tracer, WO propagator and RO processing system) the only trustworthy verification is by numerous empirical tests in different circumstances. We need much more empirics and little heuristics!
For instance in paper A.2 we find excellent experiment with STD from WRF model and GPS. But in the paper A.1 COAMPS numerical weather model was utilised, why not try also COAMPS in ray tracing experiment for hydrometeor impacts?
Experiments and measurements in meteorology naturally should last at least one full year to get credible values, variations, biases. Next the results should be divided into seasons.
In the paper A.1 we have 62 days, in A.2: 56 days (and only one radiosounding benchmark) in A.4: testing consists of meager 37 collocations RAOB – RO!

For extreme weather case short period studies are understandable, but in these projects we do not try to find typical values, systematic biases etc.

It's not a big deal to improve the research - simply You need more collocations, longer series. In science just like with technology it's dangerous to stop the tests of innovative software/algorithms so early.

In this light only the paper A.3 is truly commendable: tested data are global, encompass the whole of the year 2016. Results have been analysed in 3 climate bands. What is especially encouraging this is the only 100% paper of MSc Paweł Hordyniec.

Let me digress here.

In my opinion further progress in GNSS meteorology can be achieved in analogy to numerical weather prediction: that means painstaking numerical tinkering by innumerable experiments, parallel runs, investing in data/processing throughput, new data sources…
Entirely new theoretical concepts will be rare, now the devil is in the details: technical, numerical, parameterization, theory translation into algorithm and software…
Endless stream of publications in the field of GNSS meteorology does not improve seriously our knowledge. Development of software and data sources can be progressed only in the form of national and international services.
Our candidate's role in such development could be prominent, thanks to his talents.

Ad. d) Problems in absorbing thesis' interesting results first of all come from extraordinary complicated topics which candidate bravely undertakes.
But the author himself is not blameless.

Here are some thoughts hopping and shortcuts examples:
In abstract A.3 we read: "*The main contribution to the refractive index comes from gaseous constituents such as pressure, temperature and water vapor.*"
In chapter 2.4, summary of A.4:
"*The relativistic Doppler due to orbit geometry is removed from observations to attribute the phase delay to atmospheric bending.*" – Is seems like General Relativity…
Unfortunate grammar can be exemplified e.g. by (chapter 2.1, introduction A.1):
„*The model needs to computationally efficient to allow for operational application in global GNSS networks that comes at the expense of data quality. The possibilities to obtain pressure values are, in order of importance, numerical weather prediction models and empirical models.*"
These are not the only equivocal statements by MSc Hordyniec.
Reader often has to work out the meaning slowly, by wider context.

Submitted papers are a demanding lecture.
Candidate is immersed in details for so long that he looses perspective how narrowly professional the text becomes (especially A.4). Author must consider not only details, results, but also average reader abilities to put it all together. Candidate does not have to sound so official, tense, distant and intricate to fit into scientific journal. It can discourage, disorient and even irritate the reader.
Fortunately Introduction (Chapter 1) is much more easy and reader-friendly.

The formula of a single seamless book (technical report) is ideally suited for this kind of achievement. Theoretical part (also needed for self education) can be broadened at ease. Candidate can show his technical tools: software structures, details of numerical translations of theoretical equations. Experiments can be planned also as informal software testing not only publishable geophysical results. Such monograph can be in international circulation.

Excellent examples of such technical publications are:
- Geodätisch-geophysikalische Arbeiten in der Schweiz (often ETHZ PhDs),
- Scientific Technical Report (STR) in the GFZ serie,
- Mitteilungen des Bundesamtes für Kartographie und Geodäsie.

In a classical PhD i.e. technical report candidate can present his skills and knowledge directly, skills we can only guess by selected results in case of formal publications. Set of papers does not present most of the software details and we cannot assess to what degree numerical methods and tools are correct. Thesis assessment is to re-review of journal publications and read the introduction. The young researcher is treated not as a student but grown up scientist (and here he appears to be one). I personally give the M.Sc. Hordyniec the privilege of faith (I trust supervisor and co-advisor), but not without doubts. This work is not fully transparent just like commercial solutions...

I would be very glad to read in more accessible form many details and steps of software structure and calculations:
- how does the ray tracer really work inside the WRF model grid (A.2) ?
- how the refractive screens were fed with meteo data from GFS model (A.3) ?
- how the ellipsoid origin was adapted to different Earth curvature (A.4: eq. 4, Fig. 4) ?
{It is quite tricky procedure in view of geocentric orbits.}
- details of numerical realisation of WO (wave optics) propagator approach (A.3 and A.4)
- details of phase reconstruction, signal processing in the OL mode (A.4) and much more...
(These are not the questions for public defense!)

In case when such technical work is divided into individual publications (and theoretical introduction) reader has to collect information like pieces of mosaic, and some blank patches still remain. If you squeeze into several boxes the flow of software, algorithm development and various trials you destroy some perspective, many constructive insights and sometimes also young researcher enthusiasm.
Scientific papers are not to show oneself work and effort but to share new knowledge, well verified experiences. But PhD theses are exactly to show candidates work and effort. This is why I consider PhD composed of papers from various journals rather bad idea. It is useful for PhD student to have one or two papers, but he has to present wider knowledge in the field and document research in a systematic way.
Most of the above reflections concerns widespread practice of putting PhD students directly to work on publications instead of giving them time to grow. MSc Hordyniec passed this demanding test relatively well. In this particular case candidate put great additional effort to make overall work intelligible.

Final Remarks and Summary

My opinion seems a little biased: more critical points then applause.
I consider my duty to warn candidate on deficiencies of his work to make him more successful in the future.

Once again I state strongly: **submitted thesis include extraordinary, advanced and new research**. It is truly a remarkable work prefiguring a bright future of our young researcher.
Most imposing result of candidate work is the software development, especially:
- **advanced and multilayered system for LEO radio occultations data processing** including phase delay retrieval, signal statistical smoothing (weighting by SNR ratio), using navigation message demodulation in resolving open loop tracking, wave optics application in multipath conditions, at last transformation to meteorological parameters profiles (full scheme in Fig. 1, manuscript A.4),
- **development of wave optics software based on complex signal formalism** tackled by Fourier integral operators is the second really remarkable achievement. Atmosphere in this approach is treated as multiple phase screen set. It is a part of RO software system.

Ground based ray tracer (for STD) and numerical direct and inverse Abel transform are also commendable successes.
Individual style of paper writing and research can be improved. But such software- algorithm talent is given form above and truly rare.

Conclusion

I consider submitted thesis by MSc Paweł Hordyniec as **exceptional scientific achievement. Problems undertaken are innovative, intricate and ambitious.**
His **research includes many original solutions**: theoretical, numerical and computational. This work is the proof of his diligence, discipline and commitment in scientific projects. M.Sc. Hordyniec contribution in submitted papers is predominant. Additional value is present in the theoretical introduction. Candidate proved his unusual ability to conduct scientific research both standalone and by team working (also in foreign institution).

The scientific achievement of M.Sc. Paweł Hordyniec in the form of the presented for assessment thesis and the research achievements to date **constitute an important and impressive contribution to the development of the discipline of geodesy and cartography**.
I ultimately state that the reviewed PhD thesis by M.Sc. Paweł Hordyniec contains original results of scientific research and meets the requirements of the Act of Law of March 14, 2003 on scientific degrees and titles (Dz.U. z 2016 r. poz. 882 i poz. 1311), Order of the Minister of Science and High Education of September 1, 2011 on the criteria of assessment of achievements of a person applying for PhD degree (Dz.U. z 2011 r. nr 196 poz. 1165), Order of the Minister of Science and High Education of September 26, 2016 on the mode and conditions of PhD proceedings (Dz.U. z 2016 r. poz. 1586).
I put forward a motion to admit M.Sc. Paweł Hordyniec to publically defend the PhD thesis before the Faculty of Environmental Engineering and Geodesy, Wrocław University of Environmental and Life Sciences.

Michał Mroczek