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OPINION on thesis

“Sensitivity of the GEM model to different descriptions of city surface parameters over Warsaw”

by Anahita Sattari

Anahita Sattari in her dissertation investigated effects of various Town Energy Balance (TEB) parameterization variants on diurnal evolution of the atmospheric boundary layer (ABL) in the simulated atmosphere above the city of Warsaw. She used a high-resolution version of the Global Multiscale Environmental model to run 1-km resolution nested simulations of four selected days representing characteristic conditions for winter, spring, late spring and summer. After executing the simulations she performed a simple analysis of the results and evaluated selected results using measurements from six weather stations across the city.

The subject of the undertaken research is important. With growing resolutions of weather models understanding their performance over urban areas, inhabited by the majority of world population, is of practical and scientific importance. Using a global model in nested configuration is a good choice for this kind of research. Thus, the study undertaken had both scientific and practical potential, which was partially exploited by Anahita Sahari.

The dissertation

Structure of Ms Sattari thesis is typical. After very short introduction, in which the author described the objective as assessment of the “impact of the urban land cover on the development of the atmospheric boundary layer over Warsaw”, Chapter 2 addresses the scientific background of the problem . In particular the author provided a review of scientific literature regarding specific properties of urban surfaces and their impact on the ABL. A brief introduction to simplest parameterization (single layer urban canopy models), including TEB used in this study is also given. The chapter is well written, informative, yet could be expanded with the information on the boundary layer above Warsaw, several

papers with the experimental, as well as numerical studies of the properties of ABL over the city of interest appeared recently.

Chapter 3 entitled “Data and Methods” describes tools used in the study. After a brief description of the model used, the land cover database is shortly discussed. Generally, all descriptions are very sketchy and provide only a general information on data and methods used. E.g. very important preparation of TEB parameterization is squeezed to 5 lines (p.27, last 2 sentences). Almost equally short is the description of the model setup. There is no single information why 1 km grid resolution was chosen, why only 15 model levels in the lowest 5 km in the atmosphere is adopted. Is such sparse vertical resolution with only 4 model levels in the lowest 500m of the atmosphere enough? [4 levels were guessed from the results presented in the next chapter]. Why particular TEB categories were selected?

Similarly, there is no satisfactory information on the selection of cases used for the study. Why these particular days were chosen? Why the author considers them important? Why only 6 stations from 18 were selected for comparison of the results? Why no effort to account for vertical profiling from e.g. Legionowo or lidar was undertaken?

Chapter 4 “Results and Discussion” contains analysis of model simulation results. In the first part of the chapter (Section 4.1), vertical profiles from the surface to 3000m altitude of few selected parameters (temperature, virtual potential temperature, specific humidity and turbulence kinetic energy TKE) for “NO-TEB” and “TEB” simulations are presented for all 4 investigated cases, accompanied by some additional analysis of temperature differences between simulations with and without city surface parameterizations. It is not clear whether the presented and discussed profiles are domain averaged or single point. There is also no information (no error bars) on the variability of the presented values over the city area.

Interpretation of the TKE profiles presented is very uncertain by various reasons. Why wind profiles, together with the profiles of TKE are not plotted and analyzed? What is the meaning of TKE in the plots? Definitely, values of TKE come from the turbulence parameterization in the model. Which parameterization was used and what is the meaning of TKE in the context if the model output? How does TKE parameterization reveal real properties of turbulence in the atmosphere?

While the reviewer agrees with the majority of the conclusions in this section, their presentation and discussion is hardly satisfactory. E.g. when discussing elevated humidity inversion and condensation why no relative humidity profiles are presented? Is the vertical resolution of simulation enough to capture effects discussed in the thesis?

In the second part of this chapter (Section 4.2) differences between 3 TEB categories used in the simulations are discussed. The analysis is deeper and better done than that in the first part of the chapter, however several elements of the discussion raise questions. Why NO-TEB simulation output is not included in the comparison? Are time series presented in the dissertation are from the single-point from the model domain? Which point and why this point? What is variability of presented parameters within the city?

The stronger fragment of this part of the chapter include discussions of the Urban Heat Island (UHI), including spatial maps of surface temperatures and differences between the TEB categories including diurnal effects. I like also discussion of the variations of specific humidity, yet it is speculative at this vertical resolution of the model in the ABL.

Somewhat surprising is the section about precipitation (Section 4.2.3). Are cases C1-C4 selected due to presence of precipitation in the modeled data? Figs. 3.4-3.7 show only minor precipitation observed in C1 case, yet precipitation is present in all modeled cases. What kind of precipitation it was? Frontal? Convective? 1-km resolution is considered “cloud permitting, at least in the case of deep convection. Why in the analysis there is no comparison of surface convergence/divergence with the model-resolved vertical velocity in the model levels above the ground? What was stability of the modeled atmosphere?

In the Section 4.3. modeled (with various TEB categories) and measured in six locations surface temperatures are compared. Generally, except for the stable boundary layer conditions agreement is satisfactory. For a winter case C1 there was substantial discrepancy between the the model and observati0ons in all during whole the day with the shirt lasting big temperature drop in early evening in the simulations. How this effect can be explained? Why the simulations differed so much from the observation? Is this the artifact, or case in which the model prediction failed completely? Is it a representative case to make winter evaluation of the parameterization? [1]

Chapter 5 “Conclusions and Future Work” presents summary of the findings. Generally, the conclusions repeat main findings from Chapter 5 and are written in systematic and clear way. However, the main conclusion is missing. Does various cases of TEB parameterization really improve the model performance? What are advantages and disadvantages?

Generally the thesis seems like a one of first drafts of the publication from the performed research, not like the extended and comprehensive presentation of the problem with deep and thorough

1 Minor comment. Is Fig. 4.20 wrong or wrongly described (TEB-CLC, TEB-HB)?

discussion of the results. While, in the opinion of the reviewer, performed simulations may contain a lot valuable material, neither their presentation, nor the discussion are on the satisfactory level.

Overall judgment of the thesis.

In her thesis “Sensitivity of the GEM model to different descriptions of city surface parameters over Warsaw” Ms Anahita Sattari took the ambitious, yet not very successful effort to investigate the “impact of the urban land cover on the development of the atmospheric boundary layer over Warsaw” in contemporary high-resolution numerical weather prediction model simulations. The PhD candidate proved technical and numerical skills needed to set-up complicated simulations and produce successful outputs from coordinated simulations and to make simple analyses of the results. She also demonstrated basic understanding of urban meteorology and boundary layer meteorology. However, the thesis itself and some results presented are at the edge of acceptance. Information on the case selection, simulations and analysis of the results are far from being complete and raise many doubts, some of them – most important for the reviewer, are pointed in this document.

Conclusion

In normal situation, 3 years ago, I would suggest a major revision of the thesis along the guidelines written in the review. However, after two years of pandemic the situation allows to raise the question whether this is a good recommendation. After reviewing the dissertation I am not certain to which level deficiencies of the thesis result from the objective difficulties related to pandemic, e.g. hard to correct without common collaboration with the supervisor problems in presentation. The presented work can be a first sketch of a good publication, which afterwards could be turned into a good dissertation. On a basis of the presented manuscript I am not able to answer the question whether Ms Anahita Sattari has all knowledge, skills and abilities which justify granting her doctorate. In my opinion the decision whether the major revision is required or whether the candidate should get an instant chance to defend her dissertation should be given by the doctoral commission, which knows more about the possible problems and difficulties. In this second case my vote on granting Ms Sattari the doctoral degree will depend on the defense and on the way she will answer the questions I raised in this review.

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