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Interactive comment on "An urban large-eddy-based dispersion model for marginal grid resolutions: CAIRDIO v1.0" *by* Michael Weger et al.

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Firstly, we would like to thank the Referees for their valuable comments and suggestions, which helped us to improve the quality and readability of the manuscript. In this document, we respond to the specific comments of the Referees, starting with Referee 1. For all responses to the minor comments, we refer to the additionally supplemented pdf, where Referee comments are highlighted in red.

Response to comments of Referee 1

Specific comments

Printer-friendly version

Discussion paper

"The manuscript is rather long, which was already pointed out by the Editor. In my opinion, one solution for this could be to divide the manuscript in two, e.g.: 1) a more simple and concise model description manuscript with a simple evaluation study and 2) a manuscript presenting some atmospheric applications and sensitivity of the model. Now the model evaluation (Section 3) contains five different studies. Yet, only the comparison to the Michelstadt wind tunnel experiments (Section 3.4) represents a model evaluation. Section 3.2, instead, illustrate the model sensitivity. The rest of the studies illustrate the applicability of the model, but it is impossible to say how well does the model perform. Also, I think the annulus advection test might not be the most suitable one for a geoscientific journal."

We agree therein that parts of the manuscript were too extensive and some of the studies would better fit in a separate manuscript dedicated to model application and sensitivity. We tried to solve this issue by removing some of the studies and by shortening the model description. The annulus convergence test, as already pointed out, is not the most suitable one for this manuscript, so, we removed it. We also agree that the study with the idealized city basin is more of an illustrative example and does not really contribute to a better understanding on how well the model performs. This study will be replaced by an application study on a real city in the aforementioned separate manuscript. The rest of the studies we decided to keep for now and we collected them under Section 3 “numerical studies”, which also contains a third study concerning parallel scalability, as suggested by Referee 2 (see comments below). If the manuscript is still too long, we can remove this section completely and move the scalability test to the paragraph “programming language” of the model description. Section 4 now contains the evaluation study with the wind-tunnel experiment.

2. "The model description part of the manuscript is rather exhaustive and sometimes some variables are not introduced in the close vicinity of the equation. Please check these. You could also think if you could come up with some illustrative figures for this section."

We shortened the model description part where possible and reorganized some subsections to improve the overall structure. For example, the description of the advection scheme was rewritten in a much more compact form, as it previously contained a lot of text-book knowledge, which can be looked-up in other papers. We supplemented it with an illustrative Figure to demonstrate the reconstruction near diffuse obstacle boundaries. Non-essential parts of the pressure-solver description were removed for similar reasons, as there are many publications about multigrid algorithms. Most of the other subsections were shortened too. However, a few sentences were added to mention the programming language and the packages we used, as suggested by Referee 2. We further checked the manuscript for variables not introduced in the vicinity where they are used. In conclusion, the model description part is now shorter, and hopefully better to read.

3. "The objective to limit the computational costs of an LES model is very important. However, one should keep in mind what is the aim of the application. For instance, to resolve the flow in urban street canyons and courtyards, a spatial resolution of âĹij1 m is needed in order to directly resolve most of the energy and keep it LES. This should be emphasized in the manuscript. Furthermore, I find the first line of the abstract misleading. High spatial resolutions are needed to ensure accuracy in urban LES and to keep the amount of energy resolved by SGS terms small."

This is indeed an important remark which was not emphasized enough in the manuscript. We also thankfully incorporated the study of Xie and Castro (2006) which researched grid-sensitivity of LES models. In the introduction we now clearly state, that for an LES-model to be fully LES also within the urban boundary layer a spatial resolution of $\sim$1m is needed. Our application with diffusive buildings can therefore be interpreted as a hybride approach (or partly under-resolved LES) which still works well for the purpose of urban air-quality modeling on a larger scale (see also the cited study by Wolf et. al (2020)). However, if one researches the detailed wind field surrounding buildings, then clearly more spatial resolution is needed which comes always at its computational costs. Hence, our approach cannot make such simulations more efficient. But it can make urban microscale dispersion simulations cheaper, as the diffusive obstacle approach can shift the technical limits toward the coarser mesoscale. We carefully checked the use of the word “accuracy” in the manuscript as it shall not refer to the accuracy of LES models.

4. "The language needs revision."

We revised the language and checked the whole manuscript for grammar and punctuation.

Response to comments of Referee 2

Specific comments

1." Some sections of the manuscript are exhaustive and described in great detail. If the authors decide to keep everything within one paper, I suggest to shorten several parts of the paper to increase the overall readability."

This was already pointed out by Referee 1, and we therefore kindly refer to the first major comment.

2. "The "Michelstadt" wind tunnel experiment is a very nice example for model evaluation. However, the other parts of Section 3 are rather numerical sensitivity and convergence tests. Therefore, this section should be divided into two separate sections."

We followed this suggestion and put the Michelstadt wind tunnel experiment in Section 4. Section 3 is used for the three numerical tests, now consisting of the advection test with the circular obstacles, the rising thermal experiment, and the parallel scalability test. However, if the Reviewer suggests so, we can remove Section 3 entirely and move the scalability test to the model description part.

3. "Some technical parts of the model itself should be mentioned or explained. For example, why is it called CAIRDIO (if this is abbreviation, what does it stand for?). In

which programming language is the code written? Which libraries are used? Also, since the authors argue that the main benefit is the increased computational efficiency due to the diffusive interface approach, some kind of scaling analysis for a varying number of CPUs (or nodes) to test the parallelization would surely be of interest."

The full model name is now contained in the abstract and introduction. The code is written in Python and we added a short paragraph where we also mention the Python libraries we used. A parallel scaling test of the model is indeed very interesting, so we followed your suggestion and included such a test. We tested strong scalability for a range of 1- 400 CPU cores and a constant test problem.

4. "For most of the figures, the font size (especially for the axis labels) needs to be increased."

The font size of most figures was increased, especially of single-column figures.

5. "The authors should spend at least one more iteration on checking language and grammar as well as formatting inline equations."

We revised language, grammar and equation formatting.

Minor comments

See supplementary pdf-file

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2020-313/gmd-2020-313-AC1-supplement.pdf

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