Point-by-point response for the comments of reviewer #2

The font color of the reviewer's comments is in black and our response is in blue.

General:

This paper discusses the MITRAS model, which is an obstacle resolving meteorological model capable of simulating complex urban landscapes including thermodynamic effects. This seems to be an established code and a this detailed description on how MITRAS functions is a welcome contribution to the literature.

Authors appreciate the reviewer's opinion about the paper's objectives.

With regards to the model description, I would like to request further clarification regarding the coupling between the thermodynamics and the aerodynamics, i.e. the buoyancy forcing. The paper discusses in detail sensible and latent heat fluxes, but there is no mention of concepts like virtual potential temperature and eq. (5) does not show any buoyancy forcing. Does this mean that MITRAS treats temperature and moisture as passive scalars and can it only consider neutral conditions?

The solved prognostic equations in MITRAS use the Boussinesq approximation to consider the density DEVIATION only in the buoyancy term of the Navier-Stokes equations. The density deviation for a humid atmosphere is calculated according to the ideal gas law from the deviation of potential temperature and the deviation of specific humidity. The density deviation is used then in the hydrostatic equation to define p1, and with that impacts the model's aerodynamic components. This means that applications of MITRAS are NOT limited to those based on neutral conditions. We edited the corresponding text to clarify this point.

The incorporation of large scale forcings is a crucial element of these simulations and will be of interest to readers, but I found the discussion on page 3-4 to be somewhat confusing. Would it be possible to provide one equation in which p, $\tilde{p}$, p0 and p1, p2 and P2 are shown together so it is clear how these are related?

The requested equations are added and the text is edited to explain it.

I found the discussion regarding the turbulence modelling to be confusing. The authors use the term "filtering", suggesting that MITRAS uses an LES type formulation. However, both presented turbulence models, namely the Prandtl mixing length model and the Kato-Phillips model, are both RANS-type models. Sharpening of the language seems in place. MITRAS seems to fit most comfortably in the transient-RANS (T- RANS) or the VLES category of turbulence models.

The word 'filtering' is used here in its original context, rather that the LES context where small scales are removed from the solution of the principal equations, to show that the atmospheric state variables are divided into an average value over a finite time and grid volume and its deviation. However, as the reviewer pointed out this may be confusing. Therefore we edited the text to show the context of "filtering" and not be mixed with the LES mathematical filter.

The chosen applications are interesting and impressive but no validation is presented. The authors state that this will be done in a separate paper but this implies it is impossible to assess how well MITRAS is able to reproduce the physics of the built environment.

The purpose of this section is to show the model capabilities rather than to provide model validation, which will be done in a separate paper. Section 6.1 (comparison to wind tunnel measurements) shortly summarize some model validations done for the first version of MITRAS (upon which this version is completely based). Also different sections in the manuscript stated that the model results have been compared to physical model. We think that this is enough, at this stage, for readers to conclude that the model is good enough to reproduce the physics urban areas.

Detailed remarks:

1. Abstract: "...zero kinetic energy ..." Too much detail?

We have removed " and zero turbulent kinetic energy is assumed".

2. P2 l15: The LES code PALM should also be mentioned in the literature review.

The PALM model has been added to the list of the microscale building resolving models. The relevant reference is added as well.

3. P2 l19: …MITRAS is specific ... to what?

What is meant here is that MITRAS has the advantage that it is part of the M-SYS model system. The part of the sentence ' specific since it is' is removed.

4. P4 eqs (3)-(6): also present the continuity equation. Suggest to use subscript notation for the coordinate transformation terms, e.g. $\frac{\partial \dot{x}^1}{\partial x} = \dot{x}_x^1$ to distinguish the geometrical coefficients from the other derivatives. Also the Jacobian (?) is not defined.

The equations are changed using the subscript notations for the coordinate transformation terms. The grid volume α^* is already defined in page 5, we highlighted it although it was in the manuscript.

5. P8 L21-22: it is unclear what the authors mean/do. The text suggests they are using limiters but they do not specify which.

We changed the sentence as it is also required by Reviewer #1.

6. P9 L10: wall-functions seem in place as it is unlikely that the boundary layers near obstacles can be resolved. However, in section 4.1 it turns out that wall-functions ARE used. Clarify text.

Wall-functions are used as well near obstacle to consider the unresolved building details (such as balconies, chimneys, edges, etc.) which are considered bluff body and create vortices as well.

7. P9-10. The surface energy balance (20) does not consider the storage term ΔQ_S, but equation (22) does suggest there is storage in the ground. This seems inconsistent.

The surface balance equation is only to show the energy budget and the force term H. We changed this paragraph to clearly describe it.

8. P10 L19: Lateral BOUNDARY

We replaced the heading “3.2 Lateral” by “3.2 Lateral boundary” and the heading “3.3 Top” by “3.3 Top boundary”.

9. P13 eq 33: the maximum value 442413 of the roughness length seems pretty random. Does this number have any significance?

This point is discussed and addressed in the report of reviewer #1. Please find here a copy of our response to Reviewer #1 concerning this point.

The equation for the roughness length for the temperature at buildings in the manuscript contains two details, which may result in this confusion. The first one is substitution of the Reynolds number and the second is the limitation used to limit the roughness length. We have removed these details and we stick to a simpler form leaving these details to the model documentations. We have changed the text correspondingly.

10. P14 eq (40): this is not a backward Euler step; this strategy results in carrying out an Euler explicit scheme for the heat fluxes and an Euler backward scheme

We have removed “backward Euler step” from the text.

11. P15 eq (43). This equation is dimensionally inconsistent unless the coefficient cwt has dimensions s^{-1}.

The unit of c_{wt} is indeed s^{-1}, because it depends on the rotation speed of the wind turbine blades, given in "rounds per second". We added this information to the text to correctly describe this coefficient.

12. P18 eq (51): can be omitted

We have omitted Eq. 51.

13. P19 section 5.5: how is atmospheric stability dealt with?

It is considered during the initialization of the1d-model. The temperature profile is first calculated according to the temperature gradient set in the inputs. Then the pressure profile is calculated using the hydrostatic assumption. The density is calculated by the ideal gas law and the potential temperature can be derived from the large-scale profiles.

14. The paper contains some typos here and there and could do with a final proofread

We corrected typos and revised the manuscript using American English as the proofing language.