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Interactive comment on "The role of urban boundary layer investigated by high resolution models and ground based observations in Rome area: a step for understanding parameterizations potentialities" *by* E. Pichelli et al.

Anonymous Referee #1

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Review of the manuscript amt-2013-69

The role of urban boundary layer investigated by high resolution models and ground based observations in Rome area: a step for understanding parameterizations potentialities.

By E. Pichelli et al.

Summary This paper evaluates the mesoscale meteorological model WRF for the ur-

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ban environment of Rome, using many permutations of the available parameterization schemes. The study does not fit the journal's scope and is lacking sufficient innovative aspects and discussion to merit publication in the current state.

Recommendation: Substantial major revisions required.

Major comments 1. In my opinion this paper does not fit the scope of the journal. The aims and scope on the AMT website says: "Atmospheric Measurement Techniques (AMT) is an international scientific journal dedicated to the publication and discussion of advances in remote sensing, in-situ and laboratory measurement techniques for the constituents and properties of the Earth's atmosphere. The main subject areas comprise the development, intercomparison and validation of measurement instruments and techniques of data processing and information retrieval for gases, aerosols, and clouds." This study is purely a modelling study of the urban environment with some validation against observations. The instrumental techniques used here are not new and quite straightforward nowadays. 2. The paper does not show anything novel in the sense that we do not learn anything new. The widely used model WRF is used for many permutations of parameterization schemes, and the results are compared to observations. However, we do not learn anything why (in terms of physical interpretation or model development errors) the model has these biases, and as such the paper misses the opportunity to improve our understanding of the urban environment. So in the end the paper is not a step forward in science and therefore does not merit publication. 3. A high quality scientific paper covers a deep discussion of the model results and also comparison with earlier literature on its topic. This is missing at all in the current paper. The authors report some model biases (already for the rural conditions) that contradict to earlier findings in massive amounts of literature. Only a few references appear in the literature list. In addition, how representative are your results? They apply to Rome, but what does it say about other cities? 4. The accuracy of the measurement techniques is not discussed at all, which I would expect, especially for this journal. 5. The paper is lacking motivation behind the choices of the selected

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parameterization schemes. There is a massive amount of literature that could be help-ful to build hypotheses which combinations of schemes is advantageous and which ones not. Currently the selections seem to be made randomly. 6. Nothing has been presented how the urban morphological settings has been derived. Are these still the same as in URBPARM.TBL? I cannot imagine that these defaults settings (typical for USA cities) correspond to the buildings in Rome.

Minor: Paper is hard to follow since the authors refer to the figures in a non logical order, starting with Fig2, then 6 and 7 and then 1... Ln 63: a sonic is not passive: it obstructs the flow which should be accounted for. Ln 158: Sea breeze Ln 168: UTC Table 1: These standard deviations are taken over the whole time series or represent standard deviations within the instrumental averaging time. Unclear. Ln 191: Which version of WRF is used here. In all version older than 3.4.1. a bug in the stable boundary layer code was discovered in YSU (see WRF website). As such older versions than 3.4.1. should be ignored concerning YSU. Ln 258: finding contradicts with literature, but is not discussed. Ln 259: horizontal or vertical gradient? Ln 271: noon Ln 277 and later: units should not be italic P11: there are 2 typos in the footnote Ln 306: dont understand why part of the text is italic. Ln 309: RH is not a good quantity to evaluate WRF for humidity since it is not a conserved variable. RH depends on temperature and vapour pressure. If the model has the correct vapor pressure but the wrong temperature, you will give WRF a penalty for the wrong reasons. Use specific humidity instead. Ln 345: WRF cannot resolve downdrafts since these are parameterized in WRF... Ln 381:, Generally ....: on which objective measure do you base your statement here? Fig 6: the PBL at night is very deep for a nighttime PBL. Should be an easy case for the model! Ln 440: simplify instead of easy Ln 448: Again, downdrafts cannot be seen in WRF, since WRF does not model them.

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