

## ***Interactive comment on* “Boundary layer structure and decoupling from synoptic scale flow during NAMBLEX” by E. G. Norton et al.**

**Anonymous Referee #2**

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Review of MS-NR.: acpd-2005-0081

The paper presents a comparison of near surface and lower troposphere wind measurements taken at Mace Head during the NAMBLEX field campaign in August 2002 with data from ECMWF analyses, available in 6 hourly intervals. Data from in-situ instruments (sonic and standard anemometers) and remote sensing systems (UHF profiler and sodar) are used for comparison. The main findings are decoupling of boundary layer flow from synoptic flow aloft during land/sea breeze events and remarkable agreement of wind direction and wind speed between measurements and model analyses at most of the other times. Using UHF wind profiler data a reduced diurnal cycle of convective boundary layer development was detected most of the time at the coastal

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site, where the near surface vertical stress profiles indicate the existence of an internal boundary layer generated by the shoreline and influenced by the wind direction and the tidal height.

### General Comments

I had two completely different impressions when reading the paper:

(i) Although the results presented are not really breathtaking new, a nice case study of boundary layer characteristics and flow pattern in the transition zone from maritime to continental PBL properties and comparison with model analyses is given.

(ii) The intrinsic scope of the paper remains unclear: Is it to serve as an introductory or background paper, explaining the meteorology necessary for the understanding of the NAMBLEX trace gas and aerosol measurements? In this case at least the citation and a short summary of these results are missing. Or is the paper submitted as a part of a special issue about NAMBLEX? If yes, there is no citation given of the other manuscripts of this special issue and no links to the results of these papers. Or is the article planned as a stand alone paper about boundary layer characteristics using in-situ and remote sensing systems as well as model results? In this case the authors missed the opportunity to discuss the differences between coastal and inland PBL structure (p3202, l16).

I recommend publication after a revision of the structure of the manuscript in order to get a stand alone paper: Keep the discussion of the boundary layer and the development of internal boundary layers and their characteristics and the comparison with model analyses in the focus of the paper. Include the relevant references and some numbers indicating the agreement between model and measurements. Exclude the links to trajectory calculations and trace gas and aerosol measurements, which are neither discussed nor cited in the paper.

### Specific Comments

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1. Scientific questions, scope of ACP? The paper characterises the coastal PBL and determines the conditions for trace gas and aerosol dispersion during a one month period. As mentioned above, the trajectories responsibly for the dispersion and the concentrations measured or the source strengths calculated are not given. Either this should be included (or summarised, or cited) or the paper should concentrate on PBL characteristics only.

2. Novel concepts, ideas, tools, data? The PBL concept, including the ideas about the development of internal boundary layers, is certainly not new, but the data analysis and the comparison with model analyses is well done and the results give useful hints for model analysis improvement.

3. Substantial conclusions? Useful results concerning the development of the coastal PBL and embedded internal boundary layers are given. The comparison with inland PBL structure is missing. The wind data comparison between measurements and analyses should be quantified in more detail for comparable periods. Mean wind shear (speed and direction) between upper and lower level could be calculated. It is stated in the paper that the investigation of decoupling periods is of particular interest (p3193, l 14). But the reasons for the decoupling and the land/sea breeze development are neither discussed nor the relevant papers have been cited.

4. Methods and assumptions valid and clear? Yes

5. Results sufficient for conclusions? Yes

6. Traceability of results? As far as the meteorological results are concerned the traceability is given. But it is mentioned that the PBL data analysis is done for a better interpretation of chemical measurements (“described elsewhere in this volume”). No hint, no citation, and no summary of this chemical measurements and conclusions is given.

7. References? Beside very little standard literature dealing with remote sensing and

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the PBL, some references about chemical compounds and aerosols within the PBL are cited. On the other hand available literature about the comparison of measured and model calculated data and about remote sensing of the PBL structure is missed.

8. Is the title appropriate? Yes

9. Is the abstract concise and complete? Yes

10. Is the overall presentation well structured and clear? See “General Comments”

11. Is an elimination of figures, tables, text etc. necessary? Fig. 11 shows some general knowledge and is not discussed in the text. The figure should be eliminated and replaced by a sketch of the Mace Head experimental site including coastline, topographical information and placement of the sensors.

#### Technical Corrections

1. What is the reason for lowering the frequencies in the last two lines of table 3?

2. Fig. 1 (wind direction): I wonder, whether the offset between the 2 levels contributes to clarity. Differences between upper and lower level are not easy to distinguish in the Figure.

3. p3198, l14-15: The sharp veer in wind direction is seen in the model as well, but 12 h earlier.

4. p3199, l 4: The sea breeze is not represented by the ECMWF model. Give some model information (horizontal/vertical resolution etc.) to be able to distinguish which circulation scales can be resolved by the model and which ones not.

5. p3199, l 9-10: That depends on the time the chemical species are released in different heights. Change “chemical measurements” to “species measured”.

6. p3199, l22-24: The wind directions are similar to those at the ground. I can't see a clockwise turning in Fig. 2. Calculate some numbers indicating that finding. The mean

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wind speed is higher in the upper level (numbers).

7. p3200, l26 to p3201, l1: Isn't it wind speed which shows differences between sodar and surface measurements? (Fig. 4)

8. Fig. 5: What is the reason for the low wind speed data of the sodar at 28 and 29 August.

9. Tab. 4: "Comparison statistics", Altitude 1000 m or 1100 m (as given in the text)?

10. Fig. 6: Is it 1000 m or 1100 m?

11. p3203, l 16: The maximum reflectivity is varying between 500 m and 1500 m throughout the day.

12. p3203, l 25 (Fig. 10): What is the exact time of the passage of the front at the profiler? For me the change in wind direction is not easy to understand.

13. Fig 12, Figure caption: a) is wind direction and b) is tidal height.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3191, 2005.

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