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Interactive comment on “Lake breezes in the southern Great Lakes region and their influence during BAQS-Met 2007” by D. M. L. Sills et al.

Anonymous Referee #1

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Introduction:

The submitted paper reports on an observational (BAQS-Met 2007) and modelling (GEM-LAM) study of LBs (LB) and their potential influence on ozone air quality over the Great Lakes, and surrounding land surfaces. The analysis focuses on LB front positions in this region, and uses subjective multi-data analysis to detect LB fronts. It analyzes simple kinematic characteristics of LB fronts and compares those with published results from the same region. Sadly, the paper is uncritically put together and much narrower in focus than the authors claim. Major revisions will be needed before it can be considered for publication.

Major Comments:

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The authors have not critically considered what their paper is about, in spite of having a fairly clear statement of objectives. The title and much of the text refers to “LB” or “LB circulations” when in reality the analysis focuses almost entirely on detection of position and movement of LB fronts. This is true of both observational and modelling parts of the work. To remedy this the authors will have to either extend their analyses to include LB circulations, or to be explicit about the more limited focus of their work. If the paper retains its present focus, “lake breeze” must be replaced by “lake breeze front” throughout. The authors claim that their detection approach is “more accurate” than previous approaches. I believe this statement is a reflection of something far more complicated than simple increased accuracy. In any case, it is not sufficient to merely claim greater accuracy. Greater accuracy must be demonstrated, and not by noting a higher frequency of detection. This comment is closely linked to my first major comment. The authors make a strong claim that their LB detection approach demonstrates much higher frequencies of occurrence of LB than in all the (quite rich) previous publications. I believe this is because their techniques are detecting something quite different than was detected in previous studies. The obvious strategy in such cases is for the authors to reproduce the detection techniques of previous papers on their data set. This will presumably result in differences (in both directions) that should be captured in a 2x2 table. I am most concerned at the authors claim that the success of their detection method relies heavily on the experience and judgement of the first author. This implies that the work cannot be replicated, which makes it a dubious candidate for publication in a scientific journal. I acknowledge that they make a statement about other scientists developing this experience, but this is not sufficient. The solution is to make the detection procedure much more transparent. I recognize that complete transparency may not be possible, but the very least would be inclusion (possibly in supplemental material) of examples of the judgements made in LB front detection. The paper relies very heavily in Hayden et al 2010, which is intended for submission to ACP, but is still in preparation. This is not admissible. The paper appears to be perilously close in substance and intent to Levy et al 2010 (already accepted for this special issue

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of ACP). I wonder if two separate papers are justified.

Specific Comments:

A region name like Southern Ontario demands capitalization of both words. Page 3, lines 9 and 10: Surely this matter is fully addressed by Levy et al (2010)? Page 3, line 16: It is not sufficient to state a belief. In scientific work, claims must be substantiated by analysis. Figure 2: The figure does not show a LB circulation. It rather shows a number of thermodynamic features typical of coastal meteorology. It does not even indicate the position of the LB front. Furthermore, the figure adds almost nothing to the paper, and is only referred to once, and in passing at that. Page 10, line 1: The title is inappropriate it should read "Identification of lake breeze fronts in.....". Page 5, lines 2 & 3; Page 10, lines 2 to 8: The authors point out that previous work has used temperature, dew point and wind direction changes to detect LB. I cannot see why they then reject this approach and use a different one, without justification. Page 10, line 15: "15 LT" is not a formally correct time designation. The international convention of 1500 (not 15:00) should be used. LT usually means Local (Solar) Time, while the study seems to be based on Local Standard Time. The difference is not material, but must be made clear. UTC has no relevance in a study that addresses phenomena (such as LB) driven by local solar heating. This problem occurs in many places throughout the text, and in some figures. Page 11, line 5, Page 12 lines 15 & 16: These statements appear to contradict earlier statements about not relying on wind direction shifts. Page 11, lines 19 to 22 & page 12, lines 1 to 3: The matter of reliance on an individual is counter the principle of replication. See major Comment 4. Page 11, line 23: The authors must demonstrate that their technique is more accurate. See Major Comments 1 and 2. Page 12, line 13: Justify the choice of 0.1 m/s and 390 m. Page 14, lines 1 to 3: The authors must investigate this matter more carefully. The best approach would be to apply the Eichenlaub (1979) criteria to their data and examine exceptional cases in detail. Page 14, lines 10 to 16: Land-lake temperature differences (rather than absolute air temperatures) have been shown to be strong governing variables in

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driving sea and lake breezes. They should be used here. Page 15, lines 5 to 14: “overly stringent” and “unduly restrictive” are a matter of opinion. It may well be that previous studies detected different phenomena than are being detected here. See also specific comment 12. I suspect the difference may be because of the present work’s focus on LB fronts contrasted with previous papers’ emphasis on LB circulations. Page 16, lines 3 to 6 and page 26, line 2: This statement is premature until the matter has been carefully investigated in more detail. Section 4.3 adds very little to the study. Not much will be lost by its deletion, especially if the analyses I suggest add more text and figures. Page 21, line 10: The work really does not say anything about the evolution of LB circulation. It does say a lot about the movement of LB fronts. Page 22, line 1: again, the results of the analysis target LB front movements, not “evolutions of lake breezes”. Page 27, line 20: The authors have not presented any evidence that it is solely initial and boundary condition inaccuracies that cause the noted differences. How can they be sure the differences are not caused by model insufficiencies (of many kinds).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3579, 2011.

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