

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 #3

Received and published: 11 February 2011

Overall, I find this to be a very good paper, worthy of publication with few modifications. Since my opinion differs considerably with those of Anonymous Referee #2, I think it's most useful to present my review in the context of #2's review, highlighting important points of agreement and disagreement.

1. #2 says that "lake breeze" should be replaced by "lake breeze front" throughout. The paper uses lake breeze fronts as an indicator of the presence of a lake breeze. I don't think a global word substitution is necessary, but the manuscript should explicitly note that it is only considering that subset of lake breeze circulations that include a lake breeze front. (Example: Page 1, line 17 could be "Lake breeze fronts were found to occur on 90% of study days" or "Lake breezes were found to occur on at least 90% of study days".)

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2. #2 has strong concerns about the subjective nature of the lake breeze front identification technique and its relative accuracy. I find the technique to be appropriate and adequately described in the text. I agree with #2 that accuracy must be proven rather than asserted. It would have been nice if a couple of stations were withheld from the analysis data set so that frontal passages observed in 1-minute data could have been compared to analyzed frontal passages, but even so there would be subjectivity in the comparison. So the authors' approach has potentially greater accuracy, but greater accuracy has not been demonstrated. Such a demonstration would be nice but is not necessary for publication as long as actual claims of greater accuracy are removed. The authors are free to claim that their approach involves a more comprehensive analysis of a greater variety of observational data. I don't think a 2x2 table would prove greater accuracy because the criteria for land breeze fronts (which effectively serve as definitions of land breeze fronts) will be different for any two techniques. I agree with #2 that inclusion in SI of a couple of examples of "close calls" (with detailed explanation of reasoning) would be valuable because it would make it possible for readers to accurately judge where the analyst sets the dividing line between land breeze front and non-front.

3. I disagree with #2 that the paper relies "very heavily in [sic] Hayden et al 2010" [2011 in currently posted version]. It's only mentioned a few times as background information, and none of the methods or results of the paper rely upon it. Its citation here is fine.

4. I feel that Levy et al. and the present paper were sufficiently different in scope and purpose to merit separate publications.

5. "southern" might be part of a region name or might be merely an adjective modifying the word "Ontario". Capitalization should depend on which sense it is used.

6. (p3, line 9-10) I have the opposite view to #2 on why this needs to be changed: the statement is so broad that no single study can address it.

7. I think a conceptual figure is useful, but Fig. 2 has some serious flaws. It is not

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based on Fig. 1.7 of Stull (1988), but Fig. 14.7 of Stull does depict a lake breeze so this is probably just a typo in the figure caption. I agree with #2 that the lake breeze front needs to be identified. The depiction of the TIBL is incorrect (its vertical extent certainly does not increase as rapidly through a capping inversion as through the underlying statically neutral layer), but I could not find an example of any schematics in the literature that include both a sea breeze front and a TIBL so I give the authors credit for trying! A correct TIBL depiction in the context of Stull's 14.7 would have the TIBL top begin at the coastline, connect through the heads of the two wind arrows over the coastline, and stop at the top of the "cool air". Finally, Stull's 14.7 has wind arrows that conform to the law of conservation of mass and are therefore at least physically possible, while the authors' Fig. 2 has wind arrows that cannot be made to conform to any streamline pattern and therefore must misrepresent the wind patterns.

8. (p13, lines 15-17) Do Figures 1 & 4 show the lakeshore segments considered here? If so, say so. If not, identify the proper extent of the lakeshore segments.

9. (p14, top) Part of the difference is that you are comparing previous statistics for single sides of the lake with your own statistics for all sides. Laird et al. (2001) reports the frequencies for the east side of Lake Michigan, the west side, and both sides, for both their own work and the Lyons (1972) study (see their Fig. 2). Simple math (east side frequency plus west side frequency minus both sides frequency) yields a June-August overall lake breeze frequency of 62% (for Laird with COMET logs) or 46% (for Lyons). Furthermore, these estimates doesn't include lake breezes on the south shore of Lake Michigan. By examining a geographical area that includes lake shores of all possible orientations, the authors have made lake breeze detection much more likely.

10. I agree with #2 that lake-land temperature differences are more relevant. Also, since temperatures at Windsor are also affected by lake breezes (more lake breezes would lead to lower maximum temperatures), the land station should be one farther inland whose temperature is relatively unaffected by lake breeze development.

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11. "overly stringent" and "unduly restrictive" are also an unfair characterization because the authors of previous studies may have been more concerned about false positives than you are.

12. (p15, line 9) Laird et al. (2001) did not exclude days with synoptic-scale frontal passages in the region, but did exclude days with widespread cloud cover due to cyclones and fronts. So it should be listed under the fourth bullet rather than the first.

13. (p16, 11-13) Surely you mean "any" rather than "all" here. The use of "all" means that you require a lake breeze to be present simultaneously on all shores before the start time is triggered, etc. This presents the possibility of a pathological situation in which a lake breeze is present from 10-15 LST on Huron and from 17-22 LST on St. Clair and Erie. In that instance, a lake breeze is never simultaneously present on all shores and there can be no start time.

14. I find Section 4.3 to be the most interesting part of the entire manuscript and strongly urge its retention.

15. (p20) Given the prevailing wind direction, the penetration distances reported for Lake Erie are almost certainly underestimates of the maximum penetration distances around Lake Erie as a whole, while the domain is large enough to capture the true penetration distances for Lake St. Clair. This issue should be explicitly noted in the text.

16. (p22, line 3 and elsewhere) The text sometimes considers "shore" to refer to a portion of the coastline around a lake (example: "downwind shore") and sometimes seems to consider "shore" to refer to the entire coastline (as here). I strongly recommend sticking with the former usage to avoid ambiguity. In this instance, there was no modeled LB on the western shore of Lake Erie, so saying "all shores" is not appropriate. Similarly, p24, line 15 should be "shores of all lakes" rather than "all lakeshores". There are other instances too.

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17. (p22, lines 6-9) Rewrite sentence to remove orphan clause problems, possibly by replacing ", thus" with "that were".

18. (p27, line 20) I think the word "may" does not require supporting evidence.

19. (p28, lines 16-19) It would be appropriate to comment on model output frequency as well. Spatially resolving the front does not help much if the output has hourly temporal resolution.

20. (p29, line 11) The presence of synoptic-scale fronts is not (and was not previously) thought to impede the development of lake breezes. Days with fronts were excluded from previous studies because of the danger of misidentifying a synoptic-scale frontal passage as a lake breeze front.

21. (p29, line 24) I don't know if there's a PDF display issue, but this bullet item reads in full in my copy: "Daily penetration distances predicted by GEM-LAM were comparable to observed." I would complete the bullet item as follows: "...during daytime; at night, modeled fronts were longer-lived and penetrated farther than observed, possibly because of the difficulty of detecting fronts at night in the available observations."

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