Grouped Comments and Responses

Lake breezes vs. lake-breeze fronts

Reviewer 1) 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 title is inappropriate it should read “Identification of lake breeze fronts in.....”.

Reviewer 3) R1 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".)

Response:

The identification of the lake breeze circulation in the Great Lakes region has often focused on detection of the lake-breeze front (e.g. Lyons, 1972; Ryznar and Touma, 1981; Laird et al., 2001). Detection of the frontal gradient was also important for the analysis of lake breezes in Moroz (1967) and Estoque et al. (1976).

In addition, the lake breeze identification criteria in Table 1 state that “an area of broad divergence over the lake and the adjacent shore often indicates that a lake-breeze circulation is present and may be used to support the presence of a lake-breeze front”.

Therefore, we agree with R3 that a global word substitution as suggested by R1 is unnecessary, as is a change in title.

Re R3’s comment about the events being a subset, the chosen study domain makes it very difficult for a lake breeze to occur on one of the lakes without the presence of a lake-breeze front somewhere in the domain. For example, a Lake Erie lake breeze occurred on June 24th, 2007 (see animations referred to in the text). Due to the southerly onshore flow, no lake-breeze front is detected with the lake breeze on the north side of Lake Erie. However, a lake-breeze front is detected on the south side, and therefore the lake breeze is counted in the statistics. Even in a northerly flow, with no Lake Huron northern shore present in the study domain, lake-breeze fronts form on one side of the southern part of Lake Huron or the other. In addition, through the study period, there were no days when lake breezes were prevented from developing due to a strong synoptic wind.

Therefore, we believe that no lake breezes occurred that were not identified via detection of a lake-breeze front. The text has been revised to make this point more clear.

Claiming greater accuracy

R1) 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.

Page 11, line 23: The authors must demonstrate that their technique is more accurate. See Major Comments 1 and 2.

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.

Re “…providing what we believe to be an improved depiction of Great Lakes lake breeze behaviour.” Page 3, line 16: It is not sufficient to state a belief. In scientific work, claims must be substantiated by analysis.

R2) The fact that the authors are reporting here significantly more incidence of lake breezes than that of previous studies may be related to the definition emphasis here on lake breeze fronts.

R3) I agree with R1 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 #1 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.

Response:

We removed claims of greater accuracy throughout the document and instead, as suggested by R3, state that our approach involves a more comprehensive analysis of a greater variety of data. We also emphasize that we do not exclude any days from the

analysis based on the presence of synoptic-scale fronts, observed cloudiness, or the absence of a return flow aloft.

Re close calls, instead of adding a new mesoscale analysis figure to an already lengthy manuscript, we discuss the mesoscale analysis in Figure 3 in much greater detail, describing why lake-breeze fronts were positively identified in some locations but not in others.

Detection methodology

R1) 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.

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.

R2) Page 11, Line 19-24: The manual lake breeze identification process described here is somewhat problematic in terms of reproduce ability and needs to be made more objective.

R3) 2. R1 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.

Response:

Manual methods for detecting surface boundaries have been used in many previous studies, often with far less documentation of the methodology than we have provided, as shown in the selected examples below.

Purdom (1976) used GOES satellite imagery to investigate convective initiation at surface boundaries including lake/sea breezes and thunderstorm outflow boundaries. His description of detection methodology states only that such mesoscale phenomena “are readily detectable in GOES imagery”.

The widely cited Wilson and Schreiber (1986) paper investigating convective initiation at surface boundaries used a radar-based method for detecting low-level convergence lines. They simply described searching for radar thin-line signatures meeting spatial and temporal thresholds, then checking that these were not related to horizontal convective rolls.

Laird et al. (2001) described their methodology for identifying lake-breeze fronts as follows:

"The surface station data were examined for several distinct elements associated with the passage of a lake breeze. These included (a) a slight fall or general leveling off of temperature after the passage of the lake breeze, (b) an abrupt wind direction change from offshore to onshore at the time of passage, and (c) a short duration or steady increase in wind speed following the lake breeze passage."

"The 1996–97 GOES visible imagery was examined for clearing of cumulus clouds associated with the inland penetration of the lake breeze."

"WSR-88D reflectivity data were examined during numerous events for evidence of an identifiable boundary (i.e., thin line) associated with the lake-breeze front"

Ryznar and Touma (1981) described their identification of the lake-breeze front as follows:

"…recordings of wind direction, temperature and humidity for each station were inspected for rapid changes which gradually progressed inland."

Given the above, we agree with R3 that our manual lake breeze detection technique is "appropriate and adequately described". The final version of the manuscript submitted to ACPD already included reference to the above papers, and a number of others (we have added Laird et al., 2001 and Ryznar and Touma, 1981 to this list). Therefore, we strongly argue that no further revisions to the description of the methodology are necessary.

Reliance on Hayden paper

R1) 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.

R3) I disagree with R1 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.

Response:

We agree with R3 that the paper does not rely heavily on the Hayden et al. paper. In any case, the Hayden et al. paper has now been published in ACPD and the reference information is provided in the manuscript.

Similarity to Levy paper

R1) The paper appears to be perilously close in substance and intent to Levy et al 2010 (already accepted for this special issue of ACP). I wonder if two separate papers are justified.

R3) I feel that Levy et al. and the present paper were sufficiently different in scope and purpose to merit separate publications.

Response:

We agree with R3 that the papers are sufficiently different in scope and purpose. The present paper is a systematic investigation of the influence of lake-breeze circulations during the BAQS-Met project. The Levy et al. 2010 paper provides a systematic look at several air quality-related parameters over the course of the project and a detailed investigation of complex meteorology and air chemistry interactions during only one short case study period.

Capitalization

R1) A region name like Southern Ontario demands capitalization of both words.

R3) "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.

Response:

'Southern' Ontario is not an officially recognized region name, since there are many other parts of Ontario such as southwestern Ontario, northern Ontario, northwestern Ontario, eastern Ontario, south-central Ontario, etc. In addition, the Levy et al. 2010 paper uses "southwestern Ontario" as does the Hayden et al. 2011 paper. The Makar et al. 2010 papers use "southern Ontario".

Regional versus Local

Re "However, under what conditions is air quality influenced by local or regional meteorological phenomena? This is addressed here…"

R1) Page 3, lines 9 and 10: Surely this matter is fully addressed by Levy et al (2010)?

R3) (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.

Response:

Levy et al. (2010) examined the relationship between local meteorology and air quality in the study region over only one short case study period, so this matter is not "fully addressed" in that paper.

R3 is correct that the statement as written was too broad since it appeared to address the relationship between local meteorology and air quality in a global sense. The intention, however, is to identify the conditions under which air quality *in and near the study region* can be influenced by local-scale meteorological features, such as lake breezes.

To make this clear, this section has been revised.

Figure 2

R1) 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.

R3) I think a conceptual figure is useful, but Fig. 2 has some serious flaws. It is not 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 R1 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.

Response:

This diagram is an attempt to show both the lake-breeze circulation and the layering of the lower atmosphere that results. The various layers are based on those identified in Stull Fig. 1.7 and are adapted for use here.

The diagram has been revised, and the flow within and adjacent to the lake-breeze circulation is now represented by streamlines, with the streamline within the circulation suggesting a closed circulation.

The lake-breeze frontal zone has been labeled.

R3's suggestion re the TIBL growing in vertical extent to the top of the cool air has been implemented.

In addition, a number of additional references to this diagram have been added to the text.

Time Format

R1) 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.

R2) The use of LT for time designation throughout the text and in some figures should be replaced by the more formal notation, LST (Local Standard Time)

Response:

We will follow ACP's guide for manuscript preparation and use HH:MM LT for time in local time. LT is introduced in Section 3.1 in the following paragraph:

"Finally, to determine synoptic wind regime characteristics during the study period, the 850 hPa wind at 20:00 local time (LT, equivalent here to 20:00 EDT and 00:00 UTC) from the closest rawinsonde station (DTX) northwest of Detroit, Michigan, was used (see Fig. 1 for location)."

UTC is included only when discussing rawinsonde data because it is well known that rawindsonde data are available worldwide at 12:00 UTC and 00:00 UTC, and we want to ensure that readers know that standard operational rawindsonde data that are used in this study, not special soundings.

Climatologically Normal?

R1) Page 14, lines 10 to 16: Land-lake temperature differences (rather than absolute air temperatures) have been shown to be strong governing variables in driving sea and lake breezes. They should be used here.

R3) I agree with R1 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.

Response:

We state in the paper that "the greatest limiting factor for lake breeze occurrence was the presence of thick clouds and precipitation, typically associated with a passing low-pressure system." The land-lake temperature difference was not found to be a limiting factor during the summer months since nearly every day the air over land is warmer than the air over water.

Therefore, the climatological comparison is provided to give an indication of how often the area was affected by weather conditions resulting in cloud/precipitation and related reduced temperatures.

The text and the Table 3 have been revised to make this more clear.

"Overly Stringent / Unduly Restrictive"

R1) 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.

R3) "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.

Response:

These phrases did not appear in the final version of the manuscript submitted to ACPD. They were removed by the authors for reasons similar to that stated above by the reviewers.

Section 4.3 – Influence of Synoptic Wind

R1) 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.

R3) I find Section 4.3 to be the most interesting part of the entire manuscript and strongly urge its retention.

Response:

Section 4.3 is integral to the paper. The selection of case studies depends on the definitions in this section. These definitions are also used in related BAQS-Met papers. We strongly agree with R3 that this section needs to be retained.

Reasons for NWP Error

R1) 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).

R3 (p27, line 20) I think the word "may" does not require supporting evidence.

Response:

The text has been revised to include the possibility of model insufficiencies causing error.

Other Individual Comments and Responses

Reviewer 1

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.

Response:

The detection methodology used for this study does use changes in wind direction, temperature and dew point. Please see Section 3.3 and Table 1.

Page 11, line 5, Page 12 lines 15 & 16: These statements appear to contradict earlier statements about not relying on wind direction shifts.

Response:

We are unaware of any earlier statements about not relying on wind direction shifts. The identification of wind shifts is an important part of the lake breeze detection methodology that we use.

Page 12, line 13: Justify the choice of 0.1 m/s and 390 m.

Response:

Thank you to R1 for pointing out the missing justification. We required positive vertical velocity, but also wanted to reduce noise between 0.0 and 0.1 m/s. Therefore, positive vertical velocity greater than 0.1 m/s was used. After examining vertical velocities at all available model levels, vertical velocity at 390 m AGL appeared to best represent the maximum upward motion at the lake-breeze front. These points are now discussed in the text.

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.

Response:

Eichenlaub did not publish any criteria for his data. We can remove the reference to this being the longest stretch of consecutive lake breezes in the literature if required, but we believe it is worth alerting the reader to the significance of this event.

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”.

Response:

As argued earlier and supported by R3, lake-breeze fronts are used here to represent the existence of lake-breeze circulations. Therefore, the movement inland of lake-breeze fronts during the day describes an important aspect of the evolution of the lake-breeze circulation. We strongly argue that no revisions are needed related to these comments.

Reviewer 2

In the introduction and motivation section, it is stated that this article addresses the scientific hypothesis that local meteorological processes such as lake-breeze circulations exert a considerable influence on air quality in the study region but this aspect is hardly addressed specifically in the paper. It is important to get some indication of how often the same air mass moves between the land and water, i.e. quantification of

lake breeze recirculation patterns. From an air quality perspective, it is also important to quantify the relative contribution of local sources (including that associated with re-circulation lake breeze patterns) to regional and background sources contributions from long-range transport into the domain on high ozone days. These aspects should be expanded upon in the paper.

Response:

All of the papers in this special issue of ACPD/ACP investigate air chemistry aspects of the BAQS-Met experiment, with the exception of this one. The focus of this paper is intentionally on meteorology alone (as stated in its introduction).

The paper reviews the widely known meteorological impacts of lake breezes and their effect on air quality, then spends considerable time assessing the meteorological influence of lake breezes in the study region. The assumption is that the lake breezes in the study region will have similar meteorological impacts to those already known and described in the review, and the degree of lake-breeze influence on air quality corresponds to the degree of lake-breeze influence of the meteorology assessed.

The air quality perspective is explicitly covered in several related papers: Hayden et al. 2011, Levy et al. 2010, Makar et al. 2010a, Makar et al. 2010b, and McGuire et al. 2011. Several of these are discussed in the text.

Quantification of lake breeze recirculation patterns is greatly assisted through knowledge of the chemical make-up of the air mass, and is investigated using air chemistry measurements by Hayden et al. 2011 and Levy et al. 2010. Both of these papers are referred to in this paper.

In addition, R1 and R3 did not find fault with the focus of the paper on only the meteorology.

Therefore, we strongly argue that the focus of the paper should remain unchanged.

R2) Page 17, Line 9: The occurrence of lake breeze with synoptic wind speeds up to 22.6 m s-1 seems to be unusually high. Could a lake breeze occur under such strong geostrophic winds aloft?

Response:

The synoptic winds were greater than 20 m/s on both 23 Aug 2007 and 7 Jun 2007. Such values were present in both the 12Z and 24Z rawinsonde profiles for the day. In each case, high-deformation lake breezes were detected. Therefore, the results do show that lake breezes are possible with strong synoptic winds aloft.

Reviewer 3

(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.

(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.

Response:

The shore segments considered here are those shown in the figures. However, lake-breeze fronts were tracked well beyond the study domain when necessary. Therefore, penetration distances were based on the distance from the shore to the lake-breeze front even if the lake-breeze front penetrated beyond the study domain. The text has been revised to make this more clear.

(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 shores of all possible orientations, the authors have made lake breeze detection much more likely.

Response:

Thank you to R3 for discovering this inconsistency in the comparison of lake breeze data. The text in section 4.1 has been revised to correct this error.

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.

Response:

Agreed. The text has been revised to address this issue.

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.

Response:

Thank you to R3 for noticing this inconsistency between the analysis and the text describing the analysis. The text has been revised to correct this, and further clarifications have been made. Several typographical errors in the start/end times were also discovered, and revisions were made to correct these.

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.

Response:

In the manuscript, ‘shore’ is meant to refer to a segment of lake shoreline within the study domain.

R3 is correct in noting that the modelled lake breeze did not include a detectable lake-breeze front on the western shore Lake Erie. However, a modelled lake-breeze front was identified on the western shore of Lake Huron (see Fig. 8), and it was stated in section 4.3 that “if a lake-breeze front was positively identified on at least one of the downwind shores of the lakes in the study area, then the lake-breeze circulations on that day were considered LD”.

Nevertheless, the text has been revised to ensure no ambiguity exists in the usage of ‘shore’ in the manuscript. All instances of ‘lakeshore’ and ‘lake shore’ have been changed to ‘shore’ to increase clarity.

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

Response:

The text has been revised to correct this problem.

Re “In order to accurately predict these flows, model grid-spacing (both horizontal and vertical) should be chosen in such a way that lake-breeze fronts are adequately resolved.” (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.

Response:

Agreed. This paragraph has been revised to include this point.

(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.

Response:

In fact, in several papers, it does not actually say why they are excluded. In Laird et al. it implies that cloud associated with frontal passages would prevent a lake breeze. But your point is taken and the sentence has been revised.

(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."

Response:

This was apparently a PDF problem. The full sentence reads: "Daily penetration distances predicted by GEM-LAM were comparable to observed values, except for Lake Huron lake breezes where model values were considerably higher than observed."

NOTE:

Other minor corrections to the text for accuracy, clarity, spelling and grammar have also been made where necessary.