not including abstract, references, figures/tables

max word count is 7,500 **or** 26 double space pages

paper is at ~27 pages + ~8,500 words

Manuscript Number: MWR-D-16-0239

Title: "Fronts and Convective Cold Pools in the Oklahoma Mesonet. Part I: 15-Year Climatology"

Authors: Andrew T. Lesage and Steven K. Krueger

Huge sections of this are the result of merging the 2 papers together so reviewers haven't seen now - Chapter 3 or pieces that tie into it.

Recommendation: Reject

SYNOPSIS

This paper uses Oklahoma Mesonet data to analyze climatological aspects of frontal passages through the observational network (15 years, 1997-2011), based primarily on single station and 3-station pressure and temperature changes.

While the paper is well organized, I find the overall analysis somewhat superficial and significantly lacking in several respects. Specifically:

I think I've gotten rid of the pieces that caused the confusion + note that synoptic fronts are also picked up, but I don't know if I have done enough in this regard

- I'm not convinced that an examination of the magnitude of post-frontal divergence is sufficient to differentiate between synoptic cold fronts and convectively induced cold pools. The finding that 30-40% of frontal passages were of the cold pool type in DJF (Page 16, L337) seems to be evidence of this. Overall, there should be a more robust way of differentiating between these features of vastly different scales.

I removed the geographic section but I can't figure out how to explain a combined variable to someone who likes it in pieces.

- Front score (FS) isn't a physically meaningful variable and frontal passage times based on unequal/asymmetric triangles is highly dependent on the geometry of the observational network and frontal motion, masking much of the true signal that could be derived.

removed section

- The geographic finding (L341-342) that larger triangles had lower frequency of frontal passages reinforces the point that the authors' methodology unduly influences the findings.

removed from abstract, still very loosely mentioned at end + needs more

- While the authors claim the results could aid in modeling studies/understanding of cold pool processes and parameterizations, there are no specifics given on how this might be the case.

Overall, I don't think this manuscript meets the high standard of *Monthly Weather Review*.

MAJOR COMMENTS

✓

Mentiones usefulness for tracking fronts & highest resolution. Since they made it major comment I they may want more.

1. As I recall, one of the primary benefits of computing kinematic fields such as divergence using triangles is the ability to generate analysis points through the use of *multiple and overlapping triangles*. Such employment of triangles has been shown to provide a better estimate of kinematic parameters when compared to centered finite differences on a Cartesian grid (though it is computationally more difficult and rarely employed). With that in mind, I was surprised by Figure 1, which represents only a small subset of possible combinations, and without any overlap of triangles that would maximize the integrity of the analysis the authors are after (especially in regard to Figs. 11 and 12). So, I guess I'm not sure what the motivation was for the analysis scheme in this paper. Non-equal legs of the triangles introduced other problems (as mentioned by the authors). It's certainly not clear that the way in which the authors pursued the analysis is superior to a more traditional objective analysis, and suggests a lack of understanding of why triangular methods have been employed at all. Many of these issues are discussed in Doswell and Caracena (JAS, 1988).

①

2. P3, L36-38: Wouldn't synoptic cold fronts likewise typically have a region of divergence following passage of the front? The authors haven't established how this is a distinguishing characteristics of convective cold pools, and this seems to be an important part of their analysis. Also, there is no subjective check on the results to ensure that the classifications of the frontal types are accurate.

removed some + ✓

Noted use of triangles for spatial

3. P5, L80-82: Ideally the authors would have used both time series and spatial analysis to detect cold pools. To say that 5-minute resolution data were preferred over 40-km spatial data is a nonsensical comparison – clearly the 5-minute station data could have been analyzed as it was (time series) or on a 40-km grid, for example. Posing this strictly as an either/or proposition isn't an ideal motivation for the methodology undertaken.

②

4. P5, 2nd paragraph: The authors don't establish that surface divergence is absent following a synoptic frontal passage, which is necessary to show that your procedure was effective in distinguishing between a true convective cold pool and a synoptic frontal passage. A temperature drop and pressure rise is also present in a non-convective frontal passage, so it's not clear that these features are properly being separated out. This is a shortcoming of the entire paper.

③

Not sure how any choice wouldn't be arbitrary.

5. P9: The "front score" (FS) is clearly an arbitrary parameter, with arbitrary weighting of the two ingredients (temperature and pressure), over an arbitrary time frame (30 minutes). It's not clear why this front score was necessary; alternatively an algorithm could have detected a boundary passage based on separate pressure and temperature temporal trends. The authors have also not done any type of sensitivity testing to establish how their results may be influenced by the arbitrary choices included in

① ② Not applicable if synoptic/convective confusion doesn't come about.

③ I got rid of the 3+ vs 5+ thing but can't really understand why combining ΔP & ΔT is such a problem if they are fine separate creating of the FS. Furthermore, choice of 3 and 5 for "fronts" and "strong fronts", respectively, also appears arbitrary (or based on one station time series?). There is nothing physically important about the FS, so an analysis of this particular parameter is not necessarily meaningful for other applications.

removed "strong" fronts ①

6. P9, L179-182: The Mesonet triangles aren't spatial symmetric. Therefore, the time elapsed for frontal passage through a Mesonet triangle isn't only dependent on the size of the triangle, but also the motion of the front relative to the vertices of the triangle. This is especially true for triangles with smaller aspect ratios. Again, these are factors pretty far removed from the true climatology of frontal/cold pool passages, and limit the ability to interpret and apply the results.

removed whole section

7. Page 17, L341-342: To me, it's a problem with the methodology when the frequency of frontal passages is related to the size of the 3-station triangle being considered. While this isn't surprising, it suggests the true climatology is being unduly influenced by the analysis method.

① I think getting rid of the geographic distribution takes care of the worst of this, I think the rest is mostly just the reality of having obs on an irregular grid.

MINOR COMMENTS

1. Page 3, L30: A convective cold pool has "cold air" in a relative sense, and should be stated as such. (removed)
2. P3, L46: Provide a reference for introduction of "mesohigh" into the convective nomenclature. (source added)
3. P4, L61: Not sure what is meant by "resulted in recovery of the cold pools". Presumably, what is meant is moderation of the air mass affected by the cold pool. (clarifies)
4. P4, L70: Isn't a "bow echo" a mesoscale convective system (MCS)? (fixed)
5. P5, L88: "Smaller and shorter lasting" as compared to which studies? (referenced source)
6. P 6, L100-101: The authors have listed in the acknowledgments that the University of Oklahoma and Oklahoma State sponsor the Oklahoma Mesonet…it's not necessary to have this sentence in the body of the paper. (removed)
7. P 6, L106-107: If time series analysis was preferred over a ~40-km spatial analysis, isn't it inconsistent then to exclude stations in the Oklahoma Panhandle? The authors may want to elaborate on why these sites were excluded. (explained)
8. P 9, L159-160: Temperature drops and pressure rises also comprise the core aspect of synoptic fronts. Again, not clear how the two are being distinguished. (replaced gust front with ... front)
9. P 10, L 187-189: Can you demonstrate that synoptic cold fronts don't have surface divergence of similar magnitude in their wake? There are obvious differences between synoptic cold fronts and convectively induced cold pools, but relying solely on divergence is probably not the best way to make this distinction. (cleaned this up)

cases added ✓ 10. Page 11, L205-207: A case example (i.e., figure) illustrating a divergence signature with a cold pool vs. a synoptic cold front would be helpful here.

fixed ✓ 11. Page 11, L207-209: Not sure what the authors are trying to say in this last sentence of the section. Also, not sure about "Part II". Am I reading Part I of a 2 part paper?

fixed ✓ 12. Page 11, L210: Section titles should have the first word and proper nouns capitalized only.

fixed ✓ 13. Figure 3: Are the seasons defined somewhere? I assume winter is DJF, spring is MAM, etc. (i.e., standard meteorological definitions).

fixed ✓ 14. Page 12, L229-230: What is meant by "strong fronts resulting in cold pools"? There is no cause and effect here. The leading edge of a cold pool is simply what you are referring to as a "front". To phrase things in this way is not acceptable.

not as applicable post-adj. ✓ 15. Page 12, L243-244: It is also possible that a Mesonet triangle might only sample just a portion of a particular MCS passing through the region.

fixed ✓ 16. Page 12, L 245: "Strong fronts resulting in cold pools"…ugh.

fixed ✓ 17. Page 15, L300: "…fronts leading to cold pools." …also not good.

fixed ✓ 18. Page 15, L313-314: Can the seasonal definitions be moved up into the seasonal section (3c)?

(1) 19. Page 16, L337: Does it make sense that 30-40% of frontal passages in the winter time were associated with convective cold pools? That seems excessively high.

section removed ✓ 20. Page 18, L367-369: High plains frontal passages are more common for several reasons, one being that an upslope (easterly) flow component north of the boundary results in adiabatic cooling and tends to enhance the temperature gradient and associated propagation of the front southward. I don't see the connection with the dryline discussed in the last paragraph of Section 3.

(1) I don't have to address this if the paper isn't giving a conv vs. synoptic impression.

REFERENCES

1. All were accounted for.

like the other comments on this I think I have made enough adjustments but since it's the key complaint I'm hesitant to categorically say it's done

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EDITOR COMMENTS

The issue with the distinction between synoptic fronts and gust fronts is critical to this paper. That the authors have not convinced the reviewers (or the Editor) that this distinction is possible with the given quantities is a critical flaw in this work.

In support of that issue, I note from previously published work (including my own) that sometimes fronts in Oklahoma are not associated with strong pressure gradients. In fact, you don't even cite the most highly cited paper on a cold front in Oklahoma (Sanders 1955).

Schultz, D. M., 2005: A review of cold fronts with prefrontal troughs and wind shifts. Mon. Wea. Rev., 133, 2449–2472.

Schultz, D. M., 2004: Cold fronts with and without prefrontal wind shifts in the central United States. Mon. Wea. Rev., 132, 2040–2053.

Schultz, D. M., 2008: Perspectives on Fred Sanders' research on cold fronts. Synoptic–Dynamic Meteorology and Weather Analysis and Forecasting: A Tribute to Fred Sanders, Meteor. Monogr., No. 55, Amer. Meteor. Soc., 109–126.

Schultz, D. M., and P. J. Roebber, 2008: The fiftieth anniversary of Sanders (1955): A mesoscale model simulation of the cold front of 17–18 April 1953. Synoptic–Dynamic Meteorology and Weather Analysis and Forecasting: A Tribute to Fred Sanders, Meteor. Monogr., No. 55, Amer. Meteor. Soc., 127–143.

See also the chapter by Ed Kessler in the Sanders monograph.

This paper also discusses unusual frontal passages in Oklahoma:

http://www.ejssm.org/ojs/index.php/ejssm/issue/view/7

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REVIEWER COMMENTS

If reviewer comments have been added in the form of attachments they are found attached to this e-mail. They can also be accessed by logging into the Editorial Manager as an Author and clicking on "view attachments.

Reviewer #2: Recommendation: major revisions required

I think
purged
nost of the
peculative
discussion
his
efers
o.

This article uses AWS data from the Oklahoma Mesonet to construct a climatology of frontal passages and cold pools in Oklahoma over a 15-year period ending in 2011. The scope of the article appears appropriate for Monthly Weather Review and the results have potential to be of interest to the meteorological community. However, I believe that there are several fundamental errors of logic in the method, and some key aspects are not explained in sufficient detail. These issues raise significant concerns about the validity of the results. Furthermore, much of the discussion accompanying the results is speculative and various claims are not well-founded or well-reasoned. To address these issues, substantial changes to the method would be required, which would probably necessitate a near-complete re-working of the analysis. An alternative might be to present the results as a climatology of surface temperature and pressure changes, rather than a climatology of fronts.

1

Major comments

1 Don't have time for that.

I think I've taken care of the synoptic/convective issue but since its the biggest complaint it needs checking for any potential misinterpretations.

1. The definition of, and distinction between, cold pools, synoptic fronts and gust fronts is not clear. For example, the word 'front' appears to be used interchangeably to describe synoptic fronts and gust fronts. One underlying issue is that it isn't possible to distinguish between these phenomena in any reliable way using only the parameters described (surface delta T, delta P and divergence). The literature review concentrates almost entirely on cold pools associated with MCSs, and the fact that the analysis captures some synoptic-scale fronts is only mentioned in passing at line 141. It seems entirely probable that synoptic fronts are captured, but how many, and what percentage of events fall into this category, compared to gust fronts associated with deep convection? These aspects are important for meaningful interpretation of the results, but they are not considered further. A clearer statement on the motivation for the study would help - it is not clear how the results are intended to be used.

acknowledging they do, the portion doesn't matter

this ties into modeling application

2. Cold pools are defined as cases in which temperature decreases and pressure increases are associated with horizontal divergence greater than some threshold value. The motivation for partitioning of events by divergence magnitude isn't obvious (those with divergence are described as 'dynamically active' - but what does this mean?). The partitioning seems illogical given that the defining feature of a cold pool is its temperature deficit, relative to the surrounding area, rather than the presence or absence of divergence. For these reasons, I feel that the classification system doesn't bear scrutiny. I'm also left wondering about the dynamical significance of the divergence, which could presumably occur in association with a number of different mechanisms (not just evaporative cooling or downdrafts). Synoptic fronts sometimes exhibit post-frontal divergence, so I suspect it doesn't provide a reliable distinction between MCSs and synoptic-scale frontal events either. An alternative approach would be to use surface analysis charts and/or radar data to identify a set of events, classifying (if possible) into fronts (i.e. synoptic-scale fronts), gust fronts (i.e. mesoscale features associated with deep, moist convection), and other types (e.g. dry fronts, drylines etc.) and then comparing the surface parameter values for these types in the same way as has been done in the existing analysis. Alternatively, the existing temperature/pressure change events could be cross-checked with other types of data to investigate the type of each event, and confirm (or otherwise) whether each is associated with a frontal passage. Whether these alternative approaches are appropriate depends on the motivation for the study, but I feel that they would yield results that are more widely useful and more easily compared with other studies.

proxy for precip

removed

added plot showing divergence → precip

not doing another study

not so focused on event type since not doing conv. vs syn.

my low correlation shows that variability

3. That frontal passages are associated with temperature decreases and pressure increases is a central assumption of the analysis (e.g. lines 159 - 160). Although this may be true in general, temperature and pressure changes on frontal passage have been shown to be very variable from case to case, and even within a single case in different locations along the front. Also, temperature increases are occasionally observed in association with gust fronts. Drylines are mentioned at line 368 as a possible explanation for the higher incidences of frontal passages in western Oklahoma. However, temperature increases might normally be expected on dry line passage, at least in the daytime. If so, such events would not be captured using the analysis method employed. I would argue that the approach is too simplistic to yield a meaningful climatology of frontal passages for these reasons. One could argue that the results constitute a climatology of temperature decreases and pressure increases, rather than of fronts. A climatology of abrupt pressure changes (whether associated with fronts or not) could be useful to aviators, for example.

looking for cold pools

removed dryline stuff

this would involve redoing so much

all thresholds are arbitrary that could be used for this

4. The thresholds used to identify frontal passages are apparently arbitrary and appear to have no obvious physical basis. For example, no reason is given for combining T and P in the way described to obtain FS, or for the choice of FS threshold values of 3 and 5 for fronts and strong fronts, respectively. It is difficult to relate the FS to metrics such as temperature gradient, which are more commonly used to describe the strength of a front. Temperature gradient could be calculated from the gridded data, but this would probably require analysis event-by-event over the whole domain, rather than looking at temperature and pressure changes at individual stations in isolation. A more physically meaningful approach would be to analyse delta T and delta P distributions (or T and P gradient distributions), and then base the thresholds on the known distribution of parameter values. Figure 2 shows this kind of information, but only for one station and for a fraction of the analysis period. Can distributions be constructed for the whole domain and whole analysis period?

St removed

not doing this

fig 3

made fig3

5. The delta T and P values are analysed at individual sites, while the divergence values necessarily require calculation using wind data from three sites (the results described at lines 202 - 205 appear to be a consequence of this issue). This has implications for the results because smaller-scale events are more

This piece of it to the sentence that finishes on the next page is not an issue since my ΔT & ΔP stats are based on ones associated w/ triangles experiencing a front, not solo point instances. Slightly reworded a bit.

I didn't like using the 3 station mean of ΔT & ΔP in the stats because it would blend out the front so that the 1st station could recover the $\Delta T\downarrow$ or $\Delta P\uparrow$ some before the last station is reached

likely to be missed in the divergence results than in the delta T and P results (some of the non-divergence fronts may therefore in fact be smaller-scale features for which the divergence is not resolved, which again calls into question the classification system used). A more consistent approach would be to compute all three parameters using the gridded data (i.e. analyse delta T and P as mean values at the same three sites used to obtain the divergence, adjusting the analysis period at each station to account for system velocity, if needed). A related issue is that the regions of convergence (and divergence, to a lesser extent), are often concentrated into relatively narrow zones along/behind the front or gust front. It's not clear that convergence and divergence values derived from a network with a station spacing of ~40 km would be representative of values near the front itself. A further issue with divergence calculations from gridded observational datasets is variation in the exposure of different observation sites within the domain. Less well-exposed sites will record lower mean wind speeds than better-exposed sites, leading to artificial regions of convergence and divergence between stations in the gridded analyses. There is no evidence of measures having been put in place to compensate for this issue, which calls into question the validity of the gridded divergence values.

can't do much about this with a 30km resolution

→ mentioned station citing in Mesonet section, mentioned halfmax for divergence cold pool duration helps mitigate some potential ... influence of location

fixed

Minor comments

1. Line 16: Replace 'lowest' with 'smallest'

fixed

added

2. Lines 16 - 26: these sentences lack the quantitative information needed to ensure that the abstract functions as a stand-alone piece. For example, what is meant by 'similar' (line 17) and 'substantially more likely' (line 23-24)? Actual values of mean delta T and P, and percentages of events in each season could be included, and the text could be made more concise in general.

removed from abstract

3. Lines 27 - 28: I think you need to be more specific about how the results may be of use to the modelling community (if not in the abstract, then certainly in the main text).

fixed noted one

merged paragraphs

one found

4. Introduction (general): there are several instances of ideas being introduced in one place and then being reiterated later in the paper. This results in a lot of repetition and gives the text a disjointed feel. For example, it is stated on line 31 that cooling is due to evaporation of precipitation, and the same point is made in a new paragraph five lines further on. The first two paragraphs are essentially describing the same thing and could therefore be combined and reduced. Please look for other instances of repetition throughout the manuscript and remove the superfluous material to make the presentation more concise.

removed them

5. Lines 30 - 33: Please add some references to support these statements.

removed it

6. Line 60: Are there other studies that can be cited? Citing just one leaves the reader confused as to why this has been singled out for special mention.

fixed

7. Line 61: Please clarify what is meant by recovery of the cold pool. Should this be recovery of temperature?

cleaned this ... highlighted max ΔP & ΔT

8. Line 64 - 67: Readability is hindered by the fact that the various stages are not described in chronological order (these sentences deal with first storms, dissipating, mature, then dissipating). Mentioning that the deficit decreased to 5.4K in the dissipating stage lacks meaning when the corresponding value for the deficit in the mature stage is not provided. These issues give the impression of an incomplete and unnecessarily-selective resume of the results.

fixed

9. Lines 68 - 70: This is not surprising, given that bow echoes are mostly (perhaps entirely) a subset of MCSs.

fixed

10. Line 68: Please replace 'dozens' with the exact number of cases.

fixed

11. Lines 78 - 79: It could probably be mentioned here that the Mesonet stations are automatic weather stations. Also, remove 'surface' before 'Oklahoma' on line 78.

removed phrase ✓

check Sund Krueger

✓ 12. Line 83: What is meant by 'dynamically active'? (also see major comment #2). Please provide some references that would support the idea of an association between cold pools and regions of surface divergence.

strong replaced by exceeding threshold ✓ 13. Line 84: Please define what is meant by 'strong'. These details are probably best reserved for the method section. There are other mechanisms, besides downdraughts, that might be associated with surface divergence (e.g. rear inflow jets, and other flow patterns associated with dynamic pressure anomalies), and these should be considered.

removed much of this ✓ 14. Line 87: This sentence seems a bit vague. What is meant by 'maintain a cold pool'? How does the temperature of a cold pool recover and at what point is it considered to have recovered to the extent that a cold pool no longer exists?

removed some case studies of the comparison added ✓ 15. Line 88: How have the size and longevity of identified cold pools been assessed? I see no discussion of this anywhere in the paper, so how can these aspects be compared to other studies? Unless there is specific evidence to support the claim, it should be removed.

removed this ✓ 16. Line 89: This statement cannot be backed up because no evidence is provided that the surface divergence is due to precipitation evaporation in the analysed cases (it wouldn't be possible to show this using only surface data in any case). If the idea is based on the results of previous studies then those studies need to be cited, and it needs to be clarified that an assumption is being made based on the results of those studies.

removed this ✓ 17. Line 91: No evidence is provided to back up the claim that these events are residual or dissipating cold pools (the temporal evolution of the cold pools is not investigated), so this statement is entirely speculative. It could simply be that the analysis does not resolve the divergence in some of those cases, or that the divergence magnitude has failed to reach the threshold value at any stage in its lifecycle. Since the divergence threshold appears arbitrary, the suggestion that it represents a dividing line between dissipating/residual cold pools and other cold pools appears unfounded.

fixed ✓ 18. Lines 99 - 101: It would be more appropriate to list the sponsors of the Mesonet in the acknowledgments, and combine the remainder of the opening three sentences into a single, more concise sentence.

fixed ✓ 19. Line 103: Engerer et al. give a mean horizontal spacing of just under 30 km. Is there an explanation for the discrepancy?

I don't remember thinking much of it, I think I didn't want to lose stations moving backward but to some extent that was true moving forward too

20. Line 103: Was there a reason for non-inclusion of data between 1994 and 1997?

fixed, I think ✓ 21. Line 104: I presume the reference indicates the data source, but this is not clear. The date of access of the webpage is normally given for web citations.

clarified ✓ 22. Line 107: What was the rationale for exclusion of the Panhandle stations? I presume it has something to do with the difficulty of gridding data from these stations.

fixed ✓ 23. Lines 107 - 109: This sentence is difficult to follow. Suggest rewording to "only stations with at least 90% data availability in any given year were included in the analysis for that year", or similar.

fixed ✓ 24. Line 110: Panhandle needs a capital 'P'.

explained that counting at triangles is what I'm after so they need to be counted many times to represent proportion ✓ 25. Line 111: This point highlights a problem with the method, in that small systems could be detected if they pass over a single station, but whether this happens or not would be a matter of chance (I see this issue is mentioned at line 113). On the other hand, a large, rapidly-moving system would be sampled numerous times as it passes over many stations, so the dataset of temperature falls / pressure increases will be heavily biased towards contributions from the larger events. Grouping of temperature fall / pressure increase instances into events could reduce this issue.

fixed

26. Line 111: Replace 'resolution' with 'spacing' and 'suitable' with 'sufficient'.

27. Line 113: I would recommend inclusion of a brief overview of the instrument types used at the Mesonet stations, and of the achievable operational accuracy for each parameter.

mentioned 2hr limit for front passage, reviewer probably won't like the 40km/hr speed requirement due to this if front parallel to long side, may also need that figure I mention in Comment #51.

28. Line 125 - 126: Much more reasoning needs to be given to explain this decision. How have spatial scales been related to temporal scales? This implies that system velocity and the system size are known, but there is no discussion of either characteristic. This leaves the reader with the impression that 80 km is just an arbitrarily chosen figure.

fixed

29. Line 128: suggest adding 'of the domain' after 'borders'.

not even sure how that would work

30. Lines 134 - 135: An alternative solution to the timing discrepancies issue would be to use a 'time-to-space' analysis technique, if system velocity can be determined.

Cleaned up

31. Lines 137 - 140. These sentences are largely repetition of information contained in the introduction and provide no specific information about the method. Please move to the introduction and consolidate, or omit.

removed

32. Line 144: It's probably not necessary to give an example of an observation time, unless you use as part of a worked example.

step added

33. Line 144 - 150: If I have followed correctly, the method of removal of diurnal trends hasn't been described completely. The equation relates to calculation of a mean temperature at each observation time on a given day of the year. The removal of trends would require calculation of differences between adjacent observation times, but this doesn't seem to be mentioned. Please clarify and revise as necessary.

fixed

34. Line 148 - 150: the sentence beginning 'Without' is distracting here and might be better inserted at line 142, after the opening sentence of the paragraph.

kind of fixed

35. Line 150: I find it surprising that the mean diurnal pressure variation would be large enough cause spurious detections, as would be the case for temperature. It might be interesting to include a brief description of the diurnal pressure variation.

removed

36. Line 151: This equation could be removed as it is essentially identical to the temperature one.

→ It's a trivial effect, really negligible, but I don't know how to pull back on it since I used it.

37. Line 157 - 158: I don't follow the logic here. Why would elevation adjustments vary in time?

1

38. Line 162 - 165: this method would presumably lead to multiple detections of a single event at each site, with different magnitudes of temperature drop and pressure increase, depending on the time of the 30-minute interval over which differences are calculated, relative to that of frontal passage. Have you included any method for removal of these multiple counts?

removed

39. Line 167: the logic is questionable, given that temperature is not taken to be the defining feature of a cold pool in the current study.

not an issue

40. Line 168: Why are the units in equation (3) to the power minus 1?

cases & fig 4 added

41. Lines 197 - 198: how have dry frontal passages been identified? This suggests that other sources of data have been used. Or is it based on rainfall data from the Mesonet? It feels as though certain aspects of the method have not been described.

the half max does help with the triangle size influence on mean divergence

42. Line 207: What is the justification for defining the duration of the cold pool in this way? How does this compare to definitions used in other studies of cold pools?

...pkins ...ing ...ed ...oyancy → ...vergence

1 not an issue. Tracks fronts by triangle & the corner is hit simultaneously on both so it only turns on one switch for counting.

I haven't created a discussion section in part because reviewers disliked so much of my comparisons & more speculative ideas that I don't have all that much floating around. Plus the other reviewers didn't mind so I'm not sure how much is needed.

43. Results section (general): this seems somewhat disjointed due to the inclusion of discussion, repeated literature review, and comparison to previous studies, in between several different parts of the results. I would recommend re-ordering the section to contain only a concise presentation of the results, which could be followed by a discussion section containing the comparisons with previous studies. The lengthy discussion at lines 234 - 242 would be better incorporated into the literature review in the introduction. Much of it is repetition of lines 62 - 67.

most moved, shortened ✓

moved it to methodology ✓

44. Lines 217 - 225: Much of this material is a continuation of method, and should therefore be moved into the method section.

I see Δp_{30} & ΔT_{30} as establishing a connection & ΔT & ΔP as totals, just doing Δp_{30} & ΔT_{30} doesn't show maximum Δ & just doing a 2.5 hour Δp & ΔT doesn't necessarily show a connection or clear front depending on how it is spread out.

makes sure ΔT & ΔP are connected ✓

45. Line 221: Why has a different method been used to calculate delta T and P here, compared to that used in the FS calculations? The method should be consistent throughout, unless there is good reason for changing it.

made it more quantitative ✓

46. Lines 226 - 233: The description of results is almost entirely qualitative. The presentation would be stronger if more quantitative descriptions were used. Also, the use of a t-test, or similar, to establish which differences are statistically significant, might help to draw out the most important results.

→ not sure if there's really space in paper to fit more testing

div/precip plot helps this (fig 4) ✓

47. Lines 227 - 228: To me, the similarity in T and P differences tends to confirm the unsuitability of a partition of events by divergence. It's not really clear that the partitioning represents a division between cold pools and non cold-pools, given that the usual definition of a cold pool is temperature-based, not divergence-based. If it is to be retained, it needs to be justified more clearly.

fixed ✓

48. Lines 232 - 233: This sentence is difficult to read - use of parallel structure would help e.g. "summer pressure changes were smallest on average, while winter pressure changes were largest".

got rid of weighting stuff, should use regular numbers anyway ✓

49. Line 244: It's difficult to follow the logic of this, given that no description of the weighting method in Engerer et al is included here. Also, I would question whether a viable comparison can be made when the analysis methods are so different. The Engerer et al method could in fact be followed here. Your analysis period is twice as long, so comparisons using the same method might be worthwhile.

fixed ✓

50. Lines 248 - 249: The possibility that the differences in results could, at least in part, be due to the differences in method should be mentioned.

(1)

51. Lines 266 - 271: It seems likely that the geometry of the triangle and its orientation, relative to that of the front, could influence the calculated values of divergence. Could you investigate this possibility by using idealised data and varying the triangle shape and size? (I now see this is discussed at lines 341 - 344 - is a similar correction required for the divergence calculations? e.g. could the divergence threshold vary (inversely) with the size of triangle?)

removed ✓

52. Lines 279 - 280: The statement about evaporative cooling is entirely speculative. It is possible that evaporative cooling is larger in summer events, but there are other possible explanations. In addition, the mechanism(s) by which evaporative cooling could produce surface divergence on scales resolvable in the gridded dataset have not been discussed in any clear way - inclusion of such a discussion would make your arguments more convincing.

covered later Hocker + Basara ✓

53. Lines 296 - 297: The seasonal variation in the total frequency of events is quite striking. Are there any other climatologies of frontal passages against which the results can be compared?

fixed ✓

54. Lines 298 - 299: This sentence is carelessly worded. Replace 'the dominance of' with 'the higher frequency of cold pools in'.

fixed ✓

55. Line 300: 'Leading to' implies causality. 'Associated with' would be more appropriate. The same issue applies to lines 301 ('resulted in'), 330, 335 and 337.

(1) I have a plot for this, the one that's like avg div  area but don't know how it could be incorporated, or perhaps it's mostly just a response to reviewer thing with only a mention in the paper of the effect needed.

removed

56. Lines 303 - 308: There appear to be errors of logic in these arguments. Why would RH and the rain water mixing ratio be held constant over the different seasons? It seems logical that the mean value of these parameters would vary seasonally, as does the mean temperature. They would also be expected to vary substantially from case to case. What is the significance of convective versus stratiform precipitation (line 307)? Where is the evidence that high precipitation rates occur more frequently in summer and what is meant by 'high rates'? The whole discussion fees ill thought out and should probably be removed.

remove

57. Lines 307 - 308: Where is the evidence for this? References need to be provided to support the claim.

removed, no longer relevant,

58. Lines 321 - 324: I cannot follow this sentence even after re-reading it several times. Please revise and clarify. Also, 'quite large' (line 321) is another example of a vague statement which lacks meaning.

fixed

59. Line 340: The opening sentence of this section (and that of 3(c)) is superfluous. The title of the section makes it obvious that this analysis has been done, so it doesn't need to be stated again.

removed whole section

60. Lines 341 - 342: this seems to confirm the issue raised in comment #51, and tends to add weight to the argument for a case-by-case analysis over the domain, rather than looking at individual triangles and stations in isolation.

removed whole section

61. Line 350: The superscript after km should be 2, not -2.

removed whole section

62. Lines 358 - 366: Are there any other climatologies of frontal passages against which the geographic distributions can be compared?

removed

63. Lines 382 - 387: This is repetition of method which is unsuitable material for the conclusion section.

removed

64. Lines 392 - 393: This supposition appears to be based on the speculations of a previous study (as stated in lines 259 - 260). No investigation of the vertical structure of cold pools has been attempted here, so why comment on it? The weak correlation could equally be an indication that the frontal passages, as identified, actually comprise a rather diverse set of phenomena, including not only fronts.

1

65. Lines 407 - 418: This is exactly the sort of concise, quantitative statement of the results that would make for a convincing results section. However, it is not appropriate to simply reiterate the results in the conclusions section. Please move it to the results section and remove some of the less focused, qualitative material from that section.

moved

66. Line 430 - 439: This is the clearest statement of motivation, but its appearance right at the end of the paper means the reader goes through the entire paper without any clear idea as to motivation for the study. Please move this material to the introduction.

skipped

map added

67. Figure 1: Is it possible to indicate which triangles have been removed (e.g. by grey-shading them)? I can still see some low aspect ratio ones (or are these the removed ones?). Inclusion of the Oklahoma state border and a small selection of place names would also improve the figure.

added to caption

std added

68. Figure 3: It would be helpful to indicate the sample size for each data point in the plot. Could some indication of data spread be added to each point (as in Figure 10)?

made binwidth .1

69. Figure 4: how are the frequencies calculated? Have the delta T and delta P values been binned into discrete classes? If so, what are the size of the classes?

fixed

70. Figure 6: The plot title is somewhat misleading - this is divergence analysed across the triangles, not divergence of the triangles.

added

71. Figure 10: Could standard deviation bars be added to data points in this graph, as in Figure 9?

1 I've removed some of the qualitative material from the results section but it's not particularly obvious to me what I should have in the Conclusion section if I do as the reviewers suggest and shift or remove the majority of it.

Reviewer #3: Overall the manuscript is well written with results sufficient for publication. Listed are a number of suggestions the authors should consider in a final revision of the manuscript:

fixed

- The abstract is mainly qualitative in nature. Yet, there exists sufficient qualitative results (e.g., conclusions) that should be added to the abstract to quantify the critical results.

removed

- Lines 92-96 add little to the manuscript and should be omitted.

fixed

- The definition of the monthly periods is not provided until lines 313-314. However, the monthly periods were discussed much earlier in the manuscript. As such, the definitions should be moved to provide clarity of the analysis.

Switched some to mothly

- What is the justification for choosing seasonal (monthly) periods versus monthly or even weekly?

(1)

- Given Oklahoma Mesonet data spans 1994-2015+, what is the justification for choosing the 1997-2011 window?

(2)

- The manuscript provides little information on data quality assurance. However, given the results are entire based on the in situ observations, what are the potential "error" values that could impact the temperature and pressure values that serve as a foundation for the results?

Slightly adjusted, was explained

- Line 276-277 is poorly worded? "middle"?

included in form of stdev on plots which have been added to some

- This study focused on the seasonal and diurnal cycles associated with fronts and cold pools. However, the length of the dataset provides an opportunity to quantify the inter annual variability of those features at multiple temporal scales (annual, seasonal, diurnal, etc.). Why are these results not included?

- Given the convective nature of cold pools, the authors are encouraged to examine the results the the following articles focused on convective modes across Oklahoma:

Hocker, J. E., and J. B. Basara, 2008: A Geographic Information Systems-Based Analysis of Supercells across Oklahoma from 1994 to 2003. Journal of Applied Meteorology and Climatology, 47, 1518-1538.

Hocker, J. E., and J. B. Basara, 2008: A 10-year spatial climatology of squall line storms across Oklahoma. International Journal of Climatology, 28, 765-775.

While the overlap of the periods is not exact, the results offer important information regarding the diurnal, seasonal, and inter annual variability of convection in the domain that the authors should consider and compare. For example, years such as 1999 yielded increased convection compared to other years that are relevant to the results of the manuscript.

(1) 1st looked at 1997 data, then moved forward to 2011 while Working on this in 2012, didn't think much about the possibility of looking backward too

(2) added some Mesonet siting info to the OK Meso description. May not have added enough.