## 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

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'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.

This study was not intending to separate the two though the wording in the original was such that it suggested it was which was a serious flaw since it is correct that this procedure is not able to differentiate adequately between synoptic cold fronts and convectively induced cold pools. The single most egregious problem in this regard was in the title with the use of "convective cold pools", a faulty term that came from trying to separate out the cold pools in this study from persistent cold air pools which is the default interpretation for many who have experienced wintertime inversions in the Salt Lake Valley. The wording in the paper has been altered in many places to make it more clear that this method will find both convective and synoptic fronts and does not attempt to distinguish between them. More references have been added to highlight the variety in frontal passages which can occur that have been studied in previous works.

• 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.

The geographic distribution was the most affected by this issue and is problematic enough that it has been removed from the paper. The FS is responded to more in Major Comment 5.

• 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.

The geographic distribution section has been removed from the paper.

• 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.

The mention of parameterizations in the abstract has been removed. More references to parameterization studies have been added.

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

# MAJOR COMMENTS

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).

The primary benefit of using the triangles as they are is to be able to track fronts across the Mesonet at the highest resolution allowable on such a grid. Case studies, which we originally intended for a part II paper, have been merged into this paper in order to better demonstrate what this method looks like when put into use. The alternative methods suggested would have more usefulness in mapping the geographic distribution involved in Figures 11-12 of the original. That geographic distribution section has been removed from the paper due to the difficulties pointed out by multiple reviewers in handling the uncertainty in an irregular grid.

 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.

This and major comment 4 are side effects of the overriding issue noted in synopsis comment 1.

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.

The sentence has been removed. The analysis combines the temporal analysis at an individual station at 5-minute resolution with a spatial analysis at the Mesonet triangles to identify frontal passage occurrence across the triangle which can be shown in case studies which have been added to better show what the method looks like visually.

4. P5, 2<sup>nd</sup> 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.

This and major comment 2 are side effects of the overriding issue noted in synopsis comment 1.

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 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.

The multiple tiers of front scores have been done away with just leaving the one threshold of 3+. It is possible that this could have been done based on separate pressure and temperature temporal trends though that would have still required looking at both. Figure 2 is one of many station time series looked at for the summer of 1997 period this analysis started with before expanding to the 15 years. Generally, the stations looked similar with a front score around 3 being a large enough separation from the noise primarily between -2 and 2. A new figure (now Figure 3) has been added to show the FS for all 5-minute observations at all stations over the 15-yr period. As would be expected from Figure 2, larger magnitude positive front scores are much more common than their negative equivalents with the separation becoming more prominent around a FS around 2. Selecting a threshold of 3 was to try and maximize the number of legitimate fronts without picking up some of the noise.

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.

The geographic distribution is the section most affected by this issue and has been removed from the paper because of it. This does present a smaller challenge for other results though with the Mesonet stations being sited on an irregular grid, there is no way to adequately get rid of the entirety of the issue. Attempts to turn the Mesonet data into a regular grid would introduce uncertainty involved in interpolating the data to the regular grid.

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.

The geographic distribution section has been removed from the paper due to the difficulties surrounding it as noted in convincing detail by multiple reviewers.

## MINOR COMMENTS

1. Page 3, L30: A convective cold pool has "cold air" in a relative sense, and should be stated as such.

The reference to "a region of cold air" has been removed.

2. P3, L46: Provide a reference for introduction of "mesohigh" into the convective nomenclature.

Added in a reference for this (Stout et al. 1957).

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.

Yes, the sentence has been reworded to note this is referring to temperature recovery/moderation.

4. P4, L70: Isn't a "bow echo" a mesoscale convective system (MCS)?

Yes, the sentence has been reworded to avoid implying that to not be the case.

5. P5, L88: "Smaller and shorter lasting" as compared to which studies?

Most of this paragraph was removed or shifted around. With regards to the duration, it was removed since the duration in this study is not looked at beyond the differences between the

case studies that have been added to this paper after merging the initially planned two papers.

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 this mention of it from the methodology.

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.

The Oklahoma Panhandle didn't allow for particularly useful tracking of frontal passages across the Oklahoma Mesonet for case studies. This reasoning has been added.

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.

The language here was cleared up to not suggest that the two are meant to be distinguished.

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.

Some of the language was removed or cleared up to avoid suggesting this method distinguishes between synoptic and convective systems.

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.

Case studies have been added to the paper, originally intended for a Part II, to help illustrate the differences.

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?

Yes, at the time we thought splitting it up into a paper on the climatology and a paper on case studies made the most sense, but we have since realized that the case studies help visualize the method and decided it would be better to have one longer paper instead.

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

This has been corrected.

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

Seasons are now defined in figure captions.

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.

This has been reworded to suggest an association between some fronts and cold pools but not a causation.

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.

A lot of this section was rearranged or removed. Mention that a triangle might only sample a portion of the MCS has been added.

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

Adjusted similarly to 14.

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

Adjusted similarly to 14.

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

Seasonal definitions have been moved up to the first figure that uses seasons.

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.'

It would be too high. This is one of the issue points that resulting from the paper having been worded in a way that suggested that it was trying to distinguish between synoptic and convective systems.

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.

The geographic distribution section, and along with it, all speculation regarding reasoning for the differences in those plots, has been removed from the paper.

# REFERENCES

1. All were accounted for.

# **REVIEWER COMMENTS**

If reviewer comments have been added in the form of attachments they are found attached to this email. 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

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.

#### Major comments

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.

A very regrettable choice in title, and other uses of "convective" throughout the paper, made the paper suggest it was trying to distinguish between convective and synoptic systems when that actually was not the intent. The title and many places in the paper have been edited to establish that this method will capture convective and synoptic systems and does not try to differentiate them. Because of that, the portion of fronts that are synoptic vs convective is left unexplored.

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.

The phrase "dynamically active" was meant to mean "still residing in a cold pool" but has been removed. Surface divergence is the nearest Oklahoma Mesonet variable analogue to near-surface negative buoyancy which has been used in other studies (Tompkins 2001, Feng et al. 2015) to identify cold pools. This study had strongly suggested, but was not meant to suggest, that divergence distinguishes between synoptic and convective fronts and so many edits have been made to fix that major problem.

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 abrupt pressure changes (whether associated with fronts or not) could be useful to aviators, for example.

Case studies have been added which shows the variety in different locations along the same front. More information in the introduction has been added to note the large variety in frontal passages. A primary reason temperature and pressure have been combined into the front score is so that the variety in frontal passages where a front with little or contrary temperature or pressure change could potentially still trigger a frontal passage to be detected if the other variable changes enough that the FS meets the threshold. There are limits to how able the methodology is at detecting the less typical cold fronts without expanding the range of acceptable outcomes to the point where false positives become more of an issue.

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?

A route to a temperature gradient could be taken through front detection translating to front speed, which is shown in added case studies, but those steps have only been calculated for those case studies. The additional 5+ threshold has been removed from the study. 3+ was selected because looking at many stations (Figure 2 is one example but the others were similar), it seemed high enough to avoid picking up the noise in the FS values. A new figure (now Figure 3) has been added to show the equivalent of Figure 2 for the whole domain and analysis period as a histogram.

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 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 wellexposed 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.

The p and T values are calculated at individual triangles, though they are only done for triangles which experience a frontal passage, so all three stations which make up the corners of the triangle had to have met the front score threshold for the front to be

counted at that triangle. Any small scale events captured in one or two stations of the triangle but not all three were not counted so the divergence results do not get missed. Some of the text was reworded to better explain this. The decision not to use the three station mean of pressure and temperature for the triangle was due to concerns about the temperature recovery or pressure fall at the first station in a triangle reached by the front being blending out the temperature drop or pressure rise in the third station of the triangle by the time the third station is reached. The Oklahoma Mesonet description section includes more details now about station siting. Using the half maximum of divergence for the start and end time of a cold pool helps mitigate some of the potential issues with divergence that could arise from less than ideal siting or triangle size that result in some triangles having a higher average divergence than others.

#### Minor comments

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

This has been fixed.

 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.

The abstract has been rewritten with significantly more quantitative information.

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

The mention in the abstract has been removed. More introduction material on parameterizations of cold pools has been added.

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.

Adjustments have been made to the two noted examples. Significant portions of the introduction were re-organized and condensed to try and reduce this issue.

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

These lines, the first paragraph, was removed as part of cleaning up the repetition mentioned in the previous point.

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.

This sentence ended up being removed since meaningful comparisons cold pool durations did not seem particularly feasible due to the sizable differences in methodology.

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

Yes; this is now clarified in the paper.

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.

This paragraph has been shortened to just highlight the maximum potential temperature decrease and maximum pressure increase.

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

The sentence was reworded to address the potential in the original for it to come across as suggesting they are different.

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

Fixed; there were 36 cases.

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.

These changes have been made.

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.

What was meant by dynamically active was a continuing cold pool designation in this study. That phrase was removed. With the suggestion in Minor Comment #13 most of this section was either removed or moved down to the cold pool section of the methodology where it is noted that other studies have used negative buoyancy at near surface to identify cold pools. Surface divergence is the nearest analogue to near surface negative buoyancy from the Mesonet variables.

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.

Strong was meant to refer to exceeding the divergence threshold. Most of this paragraph has either been removed or moved to the methodology.

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?

These sentences were removed for the reasoning for 15.

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.

The case studies which have been added to the paper instead of the initial plan for them to be in a Part II look at cold pool duration but the climatology as a whole does not so the sample size is not large enough to justify retaining a comparison so the attempts to look at that has been removed.

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.

This sentence referred to here is among the majority of the paragraph that was removed.

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.

This sentence referred to here is among the majority of the paragraph that was removed.

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.

Mentioning the sponsors has been reduced to just the acknowledgments section. The first four sentences have been combined into two.

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

When checking where it came from, the ~40 km is the mean triangle side length. Looking

at the shortest side length for each triangle has a mean value just under 30 km, in line with Engerer et al. Either is probably justifiable for mean station spacing if made clear about the context but the paper has been changed to the just under 30 km usage while citing Engerer et al. on that.

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

Initial testing of this method was done using the summer of 1997 and when it appeared to be useful it was expanded forward to 2011 since the bulk of that research was being done in 2012. 1994-1996 were originally not included in the data order due to the potential of reducing the number of stations from the original (study original, not Mesonet original) 1997 total though that ended up being the case moving forward anyway since some 1997 stations did not continue in operation throughout the entire 15 years.

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.

Yes, this is meant to be the data source; the sentence has been reworded to better explain that. The access year is now used as the reference year.

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.

Yes. In case studies, which were initially planned for Part II but now are included, the Panhandle does not provide much of any usefulness for mapping frontal passages. This has been clarified in the text.

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.

This sentence was reworded and split into two sentences for clarity.

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

This has been fixed.

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.

Many of the statistics in this study are meant to represent what the average Mesonet triangle (or, for station specific statistics, the stations in a Mesonet triangle experiencing a frontal passage) would see with a frontal passage. Grouping into events would be proper for a framing where we are looking at the events the Mesonet domain would see. Since a

large portion of the frontal passages these triangles experience are from large MCSs, counting the large MCS for each station/triangle it crosses is correct to represent what the average station/triangle would see on average. Some changes to the text have been added to clarify this.

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

These have been fixed.

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.

Brief mentions of the instrument types has been added. Readers are also referred to Brock et al. 1995 for more details on the instruments, their uncertainty, and their resolution.

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.

The method has a 2 hr limit for a frontal passage to cross a triangle to try and ensure that the station showing a cold front at one end is related to the one showing a cold front at the other end. A larger time range could pick up more instances that are not related and a shorter time range increases the speed necessary for a front to be detected. An 80 km maximum side length requires that the front move at 40 km/hr in order to reach from one side of the triangle to the other within 2 hrs. The smaller the maximum side length, the more triangles (including ones in the middle of the grid) would start to drop off the grid. A larger maximum side length would have an even greater disparity in front frequency than what it currently is. 80 km and 2 hrs were selected to try and balance these various factors. Some explanation of this has been added to the text. In the end there were enough disparities between triangle areas that the geographic distribution section was removed.

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

This has been added in.

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.

System velocity can be determined reasonably in the case studies, which have been added to the paper, but was not analyzed for the period as a whole, nor would the method for front tracking be usable for anything not experiencing a front at the time.

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.

These have been moved to the introduction and consolidated.

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.

The observation time example has been removed.

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.

Added in a sentence saying that the diurnal pattern, once calculated, is subtracted from the temperature and pressure time series. This leaves me with an adjusted set of temperature and pressure departures from the mean which is fine for my purposes since I am only using pressure and temperature increases/decades in this study. When I calculate those pressure and temperature increases/decreases with the adjusted values, the trends in the diurnal pattern are counted for since each timestep had a different adjustment.

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.

The sentence has been moved as suggested.

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.

The pressure variation is much, much smaller and would only marginally affect front scores that are very close to the 3.0 threshold. In retrospect, it was not necessary, but it was done nonetheless.

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

The pressure equation has been removed.

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

This is a leftover from early on in the process when temperature comparisons from station to station were considered something we might want to look at. Since that was not something we ended up doing and the elevation adjustment doesn't vary in time and thus has no bearing on a single station time series, it doesn't adjust temperature at all for the purposes of this paper and so this was removed from the text.

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?

The front scores used to identify a frontal passage at a station is the highest that station

reached within three hours in either direction so while it is very frequent that a station would have a front score above 3 for multiple timesteps, only a single time is selected. Case studies selected (originally planned for a part II but now merged into this paper) show this method follows well with where a front would be expected looking at radar images.

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.

The sentence has been removed.

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

The units in equation (3), which is now equation (2) in the edited version, are to the -1 so that when multiplied by temperature and pressure it leaves a unitless variable as the front score.

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.

This was something that would have been visualized more in the planned part 2 paper on case studies which has now been merged into this paper since the visuals from it better illustrate what is going on with the method. The case studies inserted into this now include a dry frontal passage case which had very few cold pools associated with the frontal passages. Additionally, a figure was added (new figure 4b) which shows that frontal passages with stronger divergence were much more likely to have precipitation occurring than frontal passages with weaker 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?

Other studies (now cited in the introduction, Tompkins 2001, Feng et al. 2015), have used near-surface negative buoyancy thresholds to identify cold pools. Since the Oklahoma Mesonet does not have vertical data surface divergence is a reasonable analogue for negative buoyancy.

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 of the lengthy discussion from lines 234-242 has been moved into the literature review or shortened. Significant portions of the qualitative discussion has either been moved to the conclusions or been removed for being too speculative as noted in other

comments.

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

Most of this has been moved up to the methodology section.

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.

 $p_{30}$  and  $T_{30}$  are intended to establish a connection to a front by ensuring they occur close enough to each other in time while p and T are used to evaluate the maximum pressure rise and temperature drop. Limiting the total pressure rise and temperature drop to the half an hour used for  $p_{30}$  and  $T_{30}$  it might not capture the total increase/decrease. Using the time range used for p and T to identify frontal passages it is possible that it could pick up on some instances where a sizable enough pressure rise and temperature drop occurred more than an hour apart from each other.

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.

This section has been edited to be more quantitative.

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.

In the new Figure 4b, it shows that the percentage of frontal passages with strong divergence are associated with precipitation at much higher rates than those with lower divergence. Precipitation is frequently associated with cold pools through its connections with downdrafts, negative near surface buoyancy, and surface divergence.

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".

An adjustment along these lines has been made to the sentence.

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.

The weighting parts of this were removed, and what was left in this comparison was cleaned up.

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.

This possibility is now added in.

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?)

A plot (below) of the average magnitude of divergence for each triangle in 1997 shows that smaller triangles have higher divergence on average, particularly when triangle area is below 400 km<sup>2</sup>. The geographic distribution section has been removed since that is the part of the paper most affected by this issue.



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.

This sentence has been removed.

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?

Some similar climatologies have been added for the Atlas Mountains (Emmel et al. 2010, Redl et al. 2015) which looked at density currents and cold pools, and for Oklahoma (the Hocker and Basara papers noted by another reviewer which looked at supercells and squall lines).

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

The replacement has been made.

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.

This substitution has been made throughout the paper.

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.

This portion has been removed.

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

Same as 56.

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.

This sentence has been removed. It is no longer relevant since this version of the paper drops the multiple strength of front thresholds and sticks with just the FS >= 3 threshold.

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.

The geographic distribution section has been the hardest to try and justify the value of because of some of the difficulties in having an irregular grid. The decision is to just remove that section in its entirety.

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.

See: comment to 59.

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

See: comment to 59.

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

See: comment to 59.

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

This has been removed from the conclusion section.

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.

This sentence has been removed from the paper.

- 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.
- 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.

This paragraph has been moved to the introduction as suggested.

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.

The state borders have been added. The caption was rewritten to specify that this plot is after the triangles have been removed.

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)?

Now Figure 13. The sample size for each season is noted in the capture. The standard deviation in p and T for the 15 years has been added to the plot.

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?

The frequencies are calculated based on the number of fronts within the discrete bins for all triangles over all 15 years. The bins had a regular spacing though for simplicity the figure (now Figure 14) has been re-made using .1 K and .1 mb as the sizes for the discrete classes.

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

The title on the plot (old Figures 6-7 are now Figure 16) has been fixed to reflect this.

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

Standard deviations have been added to this figure (now Figure 19).

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:

- 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.

The abstract is now much more quantitative in content.

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

These lines have been omitted.

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.

The months in a season definition is now discussed with the first figure it is used with (old Figure 3, now Figure 13)

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

Weekly would be too much for some of these analyses to be plotted cleanly, such as the diurnal distribution figures which are now figures 18-19. The seasonal distribution subsection has been changed to a monthly distribution to provide more resolution on the temporal variability and allow for some comparison to the Hocker and Basara articles.

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

Much of this research was done in 2012 so at the time 2011 was the logical endpoint. Initially, we started with using the method for summer of 1997. 1994-1996 were excluded on the basis of thinking they could reduce the station set we had to work with in 1997 though in the end some stations did not extent from 1997 forward all the way to 2011 so it probably would not have made much difference in that regard to include those years.

- 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?

More referencing on Mesonet station siting parameters and instrumentation uncertainty have been added in the Oklahoma Mesonet dataset subsection of the methodology section.

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

This was clarified to "halfway through the frontal passages".

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?

Interannual variability is represented in the standard deviations on figures. More of the figures now have standard deviations plotted.

 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.

These articles have been added as references for comparison. While covering different subject matters (supercells and squall lines instead of fronts and cold pools) they are similar enough to be useful for comparison. Some of the analysis in this study has been switched from seasonal to monthly in large part to allow for more comparison with these studies. Interannual variability in this study is left primarily to the standard deviations which have been added to more of the figures.

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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.

The paper was not intending to establish a total separation of synoptic fronts and gust fronts though it is apparent that that is how it read to many of the reviewers and in retrospect there are places in the text and even title where that thinking would develop. It is correct that this paper does not provide the capacity to distinguish between one type from the other. There are numerous spots throughout the paper where changes have been made to try and make clear that the method will detect both convective gust fronts and synoptic fronts.

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

Most of these papers are now cited in the paper. They are valuable for establishing the large variety in frontal passages which can occur. The complexity is large enough that there will be some number of circumstances where a front will just not be detected by the method used in this study but it should be able to capture the vast majority.