Response to Reviewers
Interactive comment on “Methodology for determining multilayered temperature inversions” by G. J. Fochesatto

Anonymous Referee #1
Received and published: 24 November 2014

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 10559, 2014.

General Comments
A simple iterative technique for defining temperature inversions within an atmospheric sounding is elaborated. The overall quality of this paper is poor because of lack of clarity and consistency in the writing. The Abstract and Introduction (and literature background) suggests that the major applications of this method are for identifying temperature inversions in a given atmospheric profile. However, the methodology is vague and there is discontinuity between the title, objective, and the results. Moreover, the author has only discussed one real atmospheric profile (Fig. 4), and has not even properly defined the classes of temperature structure being identified for this profile. Rather than focusing on including all classes (such as inversions, stratified layers, cold layers, etc.), the author should focus the analysis on identification of temperature inversions alone as the title suggests.

Response: Thank you for taking your time and review this article. The intention of this paper is to describe the methodology developed to determine multilayered thermal inversions. However the paper clearly goes beyond that element by providing a methodology that determines all classes of thermal profiles (assuming classes here is referred by Rev#1 as being the ones described in the paper). In order to bring consistency to the paper I agree on Rev#1 comments and suggestions to focus the paper only in the detection of thermal inversions: single or multiple.

Specific Comments
1. The title and abstract suggest that multiple thermal inversions will be detected, but the author states the objective as follows: “to determine all-thermal layers present in a given temperature profile” (pg. 5, line 22). Not sure what the author means by “all-thermal” or “thermal” layers? Does it mean all layers with a uniform temperature gradient? And how are they connected to thermal inversions? The term “thermal layer” is loosely used throughout the paper without a proper definition.

Response: Thank you for checking the document consistency. This comment also ties back to the general comment discussed previously. Refocusing the paper in temperature inversion detection will suffice. The term all-thermal layer which is what the algorithm does actually has a connotation beyond the specific objective of multiple inversion layers. Comment is taken in full and paper was reworded accordingly.

2. Define quantities that are being identified by your method. What is the definition of classes SL, SI, FT, CL mentioned in Table 2 (last column)? What exactly is meant by

“free troposphere”? (Note some classes such as SBI top, EI-1 and EI-2 are not even mentioned in the table’s caption). What is the basis for using this particular classification? Is this classification profile-dependent? And what is the meteorological (or other) significance of each class? The significance of temperature inversions (SBIs and EIs) has been discussed. But the choice of other classes and the reason for picking them is not clear.

Response: Thanks! This comment comes back to the same issue SL, SI, FT, CL mentioned in the manuscript. Since in this instance the paper was re-focused to account only for temperature inversion layers then clarifying these classes does not make any sense. However it must be indicated that the methodology is able to deduce temperature profiles with positive upward slope which are important in defining stratified layers close to the ground. These thermal layers play a significant role in problems related to air pollution meteorology. Therefore since the paper is only dedicated to temperature inversions whichever their origin is (local surface based inversion or synoptically driven elevated inversion) the point is basically responded.

3. The methodology described in this paper cannot be reasonably justified as a standard technique in its present form. Based on the literature review in the Introduction, there seems to be a focus on polar regions, while there is no mention about its applications/limitations in tropical/sub-tropical land/ocean regions. Moreover, only a single profile has been analyzed (Fig. 4). Even in high latitude regions, there can be considerable seasonal and spatial variability in the thermal structure of the lower troposphere (Ueno et al. 2005; Eastman and Warren 2010). Additional profiles should be included in the analyses to warrant the use of this method in other studies.

Response: This paper describes the specifics of the methodology to determine thermal inversions based on temperature profile. It is not the intent of the paper to describe -temporal series- of thermal inversion layers nor to determine the spatial or temporal variability of a temperature inversions on a given case study or analysis. I agree on the fact that the methodology has been developed and tested mostly in polar region where surface and meteorological forcing are such that create seasonally different temperature profiles morphologies. While this is an important characteristic of the high latitude other latitudes has their own scale depending forcing but would not differ much on the actual mathematical definition of what an inversion is about. The purpose of this paper submitted to AMT is to describe the methodology in detail and demonstrate its applicability. Now specific applications of this methodology was published elsewhere and are indicated in the text (Mayfield and Fochesatto, 2013) and (Malingowski et al, 2014). Attending to the Rev#1 request additional sounding profiles have been added to demonstrate the use of this methodology. On the other hand this methodology can be integrated in larger databases to analyze the impact of multilayered temperature inversions in the surface temperature for example but this is beyond the scope of the present paper contribution.

4. The temperature gradients, dT/dZ, should be clearly distinguished as either positive (temperature inversion), negative (temperature lapse rate) or isothermal ($dT/dZ = 0$). For example, in Fig. 3 it is more sensible to discuss the relation between error and positive gradient strength (strength of temperature inversion) if the objective is indeed to identify multiple temperature inversions.

Response: The manuscript was refocused on detection of thermal inversion dT/dZ >0 and multiple occurrence of these temperature inversions on a single profile. No concerns on other kind of temperature gradients.

5. What about the dependence of the error on the depth of each inversion layer. Maybe shallow weak inversions are not detected as easily as sharp, deep inversions? Once again, the classification includes “shallow inversion” but it is not clear what is the definition of this particular class. It is very plausible that the error strongly depends on the strength and depth of the inversion layer being identified.

Response: Thanks for this clarifying comment. Indeed a different numerical constraint is represented by shallow or deep inversions. Therefore we have addressed that element in the paper. Now modified Figure 3 panel left provides a better explanation of how the convergence error was obtained and as well how the thermal gradient was deduced see Fig. 3 panel right.

6. The Introduction suggests that the atmospheric boundary layer (ABL) inversion will be defined by this method, but nowhere is there a clear/concise definition of the ABL.

Response: Thanks for this clarification. The paper is about a methodology to determine the thermal inversion by determining the slopes changes in the temperature profile. On the other hand the paper describes the ABL emphasizing local and synoptically driven thermal inversion formation. This is described in the Introduction. The manuscript has been modified to indicate how the method defines the inversion layer in the section 2 page 5 lines 14 to 18.

Technical Corrections

There are many grammatical and typographical errors which the author should carefully correct prior to final publication. For example, Line 16, pg. 13, “on of” should be “one of”.

Response: Thanks for this careful check. I changed as per you suggestions. There are other instances in which error and typos were identified and are clarified.

References:

Ueno, H., Oka, E., Suga, T., & Onishi, H. (2005). Seasonal and interannual variability of temperature inversions in the subarctic North Pacific. Geophysical research letters, 32(20).

Eastman, R., & Warren, S. G. (2010). Interannual Variations of Arctic Cloud Types in Relation to Sea Ice. Journal of climate, 23(15), 4216-4232.

Response: Checked these two references and incorporated. Thanks!

Anonymous Referee #2
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The study presents a simple algorithm to identify temperature inversions approximating the sounding with broken line. After having described the technique and associated errors, the author presents results on 15 Jan 2014 at 00 UTC at Fairbanks International station. Changes and deeper analysis are expected to improve the paper.

Response: Thank you for taking your time and review this article. I appreciate your recommendations. I have concentrated the manuscript in temperature inversion layer detection eliminating all other classes presents in the AMTD version. I clarified the description of the convergence error as function of inversion layer depth and temperature gradient between the base and the top of the inversion layer (see figure 3). I have also included two soundings for analysis. The second was taking during one IOP of Wi-BLEx. It can be seen in this case a much better description of the vertical structure provided by the GPS sounding. In order to keep the paper under a sizable number of pages I'm not including further testing of the methodology on other places. I honestly think these two examples illustrate the robustness of the method. It is obvious that based on the retrieved structure one can apply a further constraint (e.g., minimum layer depth or minimum temperature strength) to further consider the realization of a temporal series. On the other hand the morphology of the Alaska's interior temperature profile is complicated enough that I will argue that if a method works here then it will work anywhere.

General comments
The paper is often unclear and sometimes incoherent in presenting the study.
For example, the definition of error thresholds in temperature gradients is ambiguous: are these values referred to abs(dT/dz)? or only to dT/dz?

Response: Based on your comment and those of other reviewers the paper was revised and modified accordingly. In the current version of the paper to AMTD dT/dz was consider as positive, negative or close to zero in the first version. Now after refocusing the paper in only addressing thermal inversion layers then we only consider the detection of dT/dz >0 occurrence. The convergence error section was rebuild and a new figure 3 replaces the previous one.

Moreover, it is not clear which type for radiosounding data have been used: in case of TEMP WMO messages, only relevant points are often included in the message, while raw radiosounding data typically contain five seconds measurements. How do the performances change with finer radiosounding?

Response: Radiosondes used here cannot be TEMP WMO significant levels only because they are insufficient to define and locate in most of the cases multilayered temperature inversions. Data sounding used here are those available worldwide for example in the Wyoming data base http://weather.uwyo.edu/upperair/sounding.html. I have indicated this in the text. The only requirement for the method is to have height and temperature readings. I have incorporated an example of GPS sounding based on an experiment we conducted which is thoroughly reported in Malingowski et al 2014.

In the introduction, the technique is described as relevant in arctic atmosphere, but in the following it is stated that it is important for several applications.

Response: High latitude offers this interesting variability winter and summer soundings with combination of local and synoptic temperature inversions. However the universality of the mathematical definition of what a temperature inversion layer is allows the method to be applicable anywhere. On the other hand, the mentioned applications are related to for example air pollution meteorology in which what is interesting is to determine the height of the top of the temperature inversion layer and its vertical stratification. However in considering all reviewers comment I have refocused the manuscript in describing only detection of temperature inversion layers.

In the conclusions, it is stated that the "methodology has been applied to the study of 10 years", but no description is reported (period time?, where?) neither results are shown.

Response: This paper in AMT is about describing a numerical methodology only. This methodology was utilized to determine temporal series of surface based temperature inversions layers and multiple elevated temperature inversions layer through 10 yrs. during the winter months in Fairbanks AK. Results have been published in Journal of Applied Meteorology and Climatology (Mayfield and Fochesatto, 2013).

Indeed, the paper presents only one real application of this methodology: the author should show results obtained from a larger dataset analyzing behaviors and errors under different atmospheric conditions. The classification of atmospheric layers (I, FT and so on) is not described and it is not clear how the column classification in Table 2 is obtained.

Response: Thanks! The method has been utilized mainly during winter time. However summer profiles has now also been analyzed the behavior of errors and convergence have been clarified. We have demonstrated the use of the methodology in large datasets (Mayfield and Fochesatto, 2013) as well as in intensive observing periods (Malingowski et al 2014). In terms of thermal layers classification it has been clearly pointed out by this set of reviewers that there was a need to refocus the paper in detection of temperature inversion layers only. Therefore description of other kinds of thermal layers is out of the scope of the actual paper version.

Specific comments

The statement on pag. 10571 "The retrieval of stratified layers may results perhaps of no practical importance for operational meteorological purposes" needs to be clarified.

Response: This is just an honest comment because in terms of operational meteorology the stratified layers don't play much of a role however in the study of air pollution episodes the degree in which stratification builds up near surface is of importance. Now based on the recommendation of this review the papers only addresses strictly the detection of temperature inversion layers.

Several mis-spells appear in the paper. For example:

- pag. 10563 please change Sect. in Section
- pag. 10568 please change Fig. in Figure - pag. 10596 line 13 "the study of 10 years.

upper air data"
- pag. 10569 line 21 "The method does not introduce new temperature points it rather reduces the amount of them according to the preset convergence value. However some degree of expertise in meteorology is needed to read the output data-structure and clasify the detected thermal layers."
- Table 1 caption is unclear.
- Table 2 caption: "Results of application of the numerical routine to extract thermal layers from" has to be rephrased.

Response: Thank you for the specific corrections. Corrections have been made.

Anonymous Referee #3

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This paper describes an algorithm used to objectively separate inversion layers in a vertical profile of temperature. The author describes the technique in Section 2 and then goes on to discuss the errors associated with this algorithm and how a user of the algorithm can set parameters to detect thermal layers of different lapse rates. After this, the algorithm is applied to a single sounding to demonstrate the algorithm and how the settings change the ability of the algorithm to reproduce the original profile. Finally, a discussion of the applications of this algorithm to different datasets and the weaknesses of the algorithm summarize the paper.

General Comments
Paper Strengths: A method such as this is a valuable contribution to the meteorological literature and the author makes multiple references to areas of atmospheric science this algorithm could assist in. With a good portion of the field focusing on high-temporal resolution solutions to atmospheric profiling, feature-tracking algorithms such as this is useful. In addition, methods that summarize the data are useful to operational meteorologists who must navigate the fire hose of data they must sort though to produce forecasts.

Response: Thanks for this very fair assessment of the usefulness of this paper. We found ourselves in the same problem and I decide to instrument mathematically what a meteorologist will do when analyze a specific temperature sounding plot.

Paper Weaknesses: A key weakness of this paper is the vague and somewhat unfocused text. It was difficult at times to determine a roadmap within the different sections as to explain why the described method works as various definitions of key terms in the context of the algorithm are not made. In addition, consistent terminology is needed to help guide the reader. Multiple grammatical errors and confusing language make this paper difficult to read. Future revisions should correct these weaknesses as they greatly impact the ability to understand and implement the algorithm.

Response: Thank you for this detailed account of elements. A common element in the review process has been refocusing the paper in only thermal inversion detection. This helped clarifying and concentrating the manuscript into well-defined objectives. Grammar has been checked thoroughly.

Specific Comments
1.) The methodology for the convergence error correction in Section 3 would do well with a figure to help clarify the example in the second paragraph as well as the different thermal profiles used to characterize Figure 3.

Response: Figure 3 was redone. Now panel left illustrate an inversion over which I have done the numerical simulation changing temperature across the inversion, depth and convergence error. So this is a very simple three-layer structure that need to be detected. Therefore a special software was made to determine the ranges plotted on the right panel.

2.) The terms SBI, EI, shallow inversion, cold layer, free troposphere, stratified layer are not explained, but are referenced in Table 2. Are these key layers identified by the algorithm? How are these specified?

Response: The numerical routine detects all thermal layers dT/dz>0, ~0, <0. Since the paper has been rewritten paying special attention of the recommendation of focusing on thermal inversions then dT/dz>0 is considered. Definitions of SBI, EI, Shallow inversion layers, cold layer and free troposphere are then used in table 2 and explained in the text.

3.) Here are a few examples of the confusing language and grammatical issues in the paper: Line 25 (page 1) to Line 2 (page 2), Line 18-20 (page 3), Lines 24-27 (page 3), Lines 21-24, page 5.

Response: Thanks! Problems corrected.

4.) Is there a larger dataset (i.e. the 10-year dataset mentioned) that could be used to help understand more about the error properties and recommended settings of the method? This may help any reader who wishes to implement this algorithm understand what to expect in terms of the performance of the method. Given that the author has experience with implementing this method to different datasets (via different references he has been an author on where the algorithm has been used), I get the feeling his experiences and challenges could provide a more through discussion of the algorithm's properties and motivation for the algorithm's development than what is offered.

Response: The paper submitted to AMT is about describing the methodology only. The methodology was utilized to describe the multilayered structure of the winter ABL in Fairbanks, Alaska (Mayfield and Fochesatto, 2013) over radiosondes series of 10 yrs. In this case it was selected a convergence error of 0.1 to make sure the temperature profile was exhaustively analyzed. In the modified manuscript a GPS sounding was incorporated allowing a better description of the thermal structure of the lower troposphere. It is clearly indicated that convergence value (epsilon) is internal on each step while final error (%) accounts for how much error was obtained between the final fitted temperature profiles and the actual profile. However in our experience what is needed is to introduce a further criterion on the data structure to obtain a desired temporal series. For example one can assume a criterion of only extract from the data structure inversion heights having a depth of 100 m or strongly-stratified SBI layers with more than 10 C/100m. But all these further criteria are beyond the description of the numerical procedure itself and correspond to a specific application of the methodology to a specific dataset. As another example, you can

derive a temporal series of EIs and extract simultaneously the dew point to classify them into anticyclone or warm air advection as has been done previously for instance in Mayfield and Fochesatto, 2013 but also earlier reported by Millionis and Davies.

5.) Within the conclusions, the author mentions "However the application of this methodology to a real case produces an overall error in the resampled profile that is different from the prescribed preset convergence factor epsilon." This statement identifies what seems to be a rather important problem with the method described in Section 3 that is used to modify the algorithm parameters to get an expected result. How significant is this problem on average? What has been done to rectify it when this method has been used elsewhere? Perhaps the issues raised in Comment 4 could be tied together with this.

Response: Page 10 Line 9 to 19. It seems the text was confusing. Now the statement below was reworded:

The relationship between threshold thermal gradient *dT/dz*, the preset convergence factor ε and the overall final error between the resample temperature profile and the original temperature profile is difficult to establish. Section 3 deals with the relationship between *dT/dz* and ε for theoretical profiles (i.e., constant temperature profile with controlled thermal inversions) to deduce this relationship. However the application of this methodology to a real case produces an overall error in the resampled profile that is different from the prescribed preset convergence factor ε. **This is important to differentiate since ε applies internally as convergence factor to increase fidelity in the temperature fitting over an Euclidean norm that is applied over a variable vector length step by step. While, on the other hand, the overall error indicating how accurate the resampled temperature profile reproduces the original profile accounts for the entire profile at once.**

6.) Figure 1: : :the caption should say which way does the sequence goes (left to right or right to left?)

Response: Thanks! Changes were incorporated in Figure caption.

7.) Rather than directly referencing the figures generally as summary illustrations of a point, it would be helpful to the reader if the author guided the reader's eye through the details of the figure. This would be especially helpful with Figure 3 as there weren't any clear steps in the example (paragraph 3 of Section 3) to use Figure 3 and the paragraph utilizes phrases like "the thermal gradient relaxes". Also, there is no final recommended epsilon value for this example.

Response: This section has been rewritten completely. Figure 3 now contain a sketch of what the numerical simulation for the convergence factor is and another figure illustrating the relationships obtained between dT/dZ, epsilon and inversion depth.