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Discussions

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Interactive comment on "Pathfinder: Applying graph theory for consistent tracking of daytime mixed layer height with backscatter lidar" *by* Marco de Bruine et al.

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We thank the anonymous reviewer for the thorough review and for the positive comments about our work. The general and detailed comments helped us improve the manuscript. We feel that some of the comments, which are just and would add to make the study even more complete, would in fact widen the scope of the paper beyond what we intended. We have argumented our choices in the response to the reviewers' comments. Further, our response to the items raised by the reviewer is listed below, point by point, below.

General comments

a. The reviewers comment questions the approach regarding the time resolution at which the data is processed and some suggestions are made on how the data analysis could be improved by essentially making a multi-pass analysis. We chose to analyse the data at the higher (native) time resolution, since averaging would indeed trigger the discussion on scales; e.g. is 10 minutes long or short for the given conditions? Since we realised that analysis of a full day would not be sensible, the analysis was divided-up in time intervals of 15 minutes, with the notion that this would not impose limitations to the MLH development, while keeping the computation relatively simple. The suggestion to develop a multi-pass version of Pathfinder is a very interesting one, which we should take into consideration. However, in the context of the current paper, we feel that the implementation would stretch beyond the scope of the basic new principle that is introduced, namely the application of graph theory.

b. We agree that R^2 alone does not allow the reader to judge the accuracy and performance of the Pathfinder algorithm and different methods. We added the proposed statistics of mean bias and absolute error together with other statistics where appropriate. Text of paragraphs 4.3.1-4.4, 4.5.1 and the beginning of 4.5.2 are changed accordingly.

c. The reviewer asks to connect the sensitivity analysis to the validation of the results. We agree that a more thorough validation of our new method would be helpful, also to find a better way to understand the settings of the parameters of the algorithm. However, we found that it was difficult to find independent data sets of MLH estimates that were as good or 'better' than the UV-lidar dataset and long enough and overlapping with the UV-lidar so that t would make sense to use these as a benchmark. Also the manual MLH determination is not flawless. Furthermore, again, we feel that the main contribution of the paper is the introduction of the Pathfinder algorithm based on graph theory. Even if the sensitivity analysis and validation is incomplete, we show that an improved consistency of tracking is accomplished against another technique based on

gradient analysis. We should very much like to put effort in the sensitivity analysis and validation in future work.

Detailed comments

1. "2-1: somewhere in the introduction you need to make clear at which time scale (record length) and time resolution you intend to derive MLH."

   This is a valid comment and we have changed the text, by adding a sentence about how the data are treated in terms of time resolution.

2. "3-15: could you provide a reference for the lidar equation. I appreciate that below the equation you explain a number of the components, but it would be helpful to the reader to have a place where he/she could read about the other components of the equation."

   We apologise for the omissions in the explanation of the lidar equation. The extinction coefficient (alpha) is now mentioned in the text, as well as a reference to basic lidar background material.

3. "3-27: 'parts of the data will be excluded as MLH'. At this stage the matrix that is shown in figure 2 is not yet in the readers mind. Hence, it is unclear what you mean with 'data' here. Is it a time slot (but a time slot is not a MLH)? I guess what you intend to say is that you imagine a single lidar profile and blank out certain parts of that profile where you know/think that the MLH cannot be located."

   We acknowledge that the statement needs better clarification. We have changed the paragraph with added text to better explain the stages of the method, assuming less prior knowledge.

4. "4-5, section 2.3.1. This section is missing theoretical backing of the different restrictions. The motivation of the values of height intervals etc. seem to come

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out of the blue. Please provide independent support for the choices you make here."

Although the structure of the paragraph might have been confusing and suggest otherwise, no complex theory is behind the choices made here. We limit the searching only to cloud tops, strong gradients and long term MLH maxima. The values chosen for relaxation are based on practical considerations.

5. "A number of suggestions to clarify section 2.3.1: First, I would suggest to add a figure here that sketches the MLH with the different disturbing items in it (residual layer, cloud layers etc.). Then it is easier to visualize and explain the various restrictions."

The text has been rewritten so it should become clear that the algorithm does not explicitly search for individual features like residual layers etc. A description of the different disturbing items guided by a figure should nog be necessary besides figures like Fig.3b.

Secondly, figure 1 (which is very informative by the way, well done!) mentions the detection of residual layer as a restriction. However, I could not find a specification of this restriction in section 2.3.1.

The algorithm does not explicitly search for the residual layer. As can be seen in the oval/process preceding 'residual layer' this sequence represents the part of Pathfinder that searches for a first strong negative gradient. Assuming this restriction does not trigger on fluctuations inside the ML, this should either be the correct MLH or the residual layer, hence the term 'residual layer' there. For better clarity and consistency with the text in 2.3.1, the term 'residual layer' in figure 1 will be replaced by 'strong negative gradient'. Also, the term 'Heuristics' will be replaced 'Climatology' and the section of auxiliary meteo data will be given dashed lines to clarify this part is optional and not discussed in this paper.

Finally, the text in section 2.3.1. could be shortened (and made more informa-

tive) if the various restrictions would be summarized in a table. Then details of numbers could be removed from the text. And the various 'thresholds' mentioned in figure 1 could easily be tracked down (I found it somewhat confusing that the figure simply mentions 'threshold' where probably each of these thresholds has a different value/meaning). Only when I arrived at the results section, line 8-20 I have to discover that there is indeed such a table. I don't understand why it is not referred to in section 2.3.1, where all these numbers are mentioned in the text as well.

Response: The values in Table 2 are specific for the ALS450 UV lidar installed at Cabauw. However, the paragraph should indeed be structured in a better manner. The word threshold here is used to state that a certain value of RCS or gradient is chosen for each restriction to trigger on. These are different for each of the restrictions.

Before going into the description of the restrictions, all will be mentioned in a list and linked to their representation in Fig. 1. The vertical (75m) and temporal relaxation (2 min) will be mentioned separately and the end of the paragraph, so it becomes clear there are 4 different restrictions and how these are treated.

6. "5-25 and further. I would like see a more thorough analysis here of the distinction between mean growth rates and turbulent growth rates. Apart from the growth rates themselves, also the time scale related to the distinction between what is mean and what is turbulent is important here. Now this distinction seems to be related/coincident with the 15 minute window size. This discussion is related to my first major comment (a)."

We agree that there could be a more extensive discussion on the turbulent scales, but as in our response to the general comment (a) we feel that the main contribution of the paper, i.e. the introduction of graph theory, stands as it is. We considered the publication of the concept in this paper could be the basis of further

important work as the reviewer suggests. We have not changed the manuscript for this point.

7. "6-1: Here you apply a weight to the points (where one would expect a large weight to be related to a large number). Later on you are talking about costs (the term that probably comes from graph theory). I think it would be better to start talking about costs from the outset. Then the reader has to make the mental jump from 'important = low number' only once."

Using two words for the same concept might indeed be focusing to the reader, the term 'weight' will be removed from the text altogether and replaced by 'cost'.

8. "6-9: Apparently, the first point in each time window is determined independently of the information that could be derived from the previous window. To me this seemed rather wasteful, as the continuity of the MLH is only retained for 15 minutes. Only in line 6-19 it becomes clear that indeed the windows are coupled in time (initialization from final value of previous window). However, this still poses a problem, when it comes to application of the method to a full day of data: the graph method is applied within the window (those points are treated as one dataset) and the subsequent windows are only connected by one point). Hence, the question is to what extent the full day evolution depends on positioning of the windows in time. It is unclear to me if the start-time sensitivity tests does this test on a full-day evolution or only on a within-window evolution."

The placing of the description of using the gradient method for the first timestep in a day might have given the impression that this is applied to every timewindow. However, this is not the case: it is only applied to the first timestep of the first timewindow of a day. We moved the sentence (6-10,11) about the gradient method to the last paragraph. With this, the preceding description still assumes an arbitrary large dataset. Only the last paragraph now goes into the splitting of a dataset into multiple timewindows. The rest of the last paragraph is restructured

to give a clearer message.

9. "6-17: the size of the time window is discussed here, based on computational costs. But, in fact the choices also imply a decision on which time scales in MLH variation the authors want to resolve (see comment related to line 5-25)."

We agree that an optimised time window might be selected based on theoretical grounds, however, for the demonstration of the method introduced, 15 minutes was a trade-off, mainly based on practical considerations.

10. "7-1: it is unclear which datasets have been used. Only in the results it becomes clear that there are 3 sets: the 12 day campaign in 2008, May 2010 and the entire year of 2010. A clear motivation for using these data is missing. Please include a specification (which dates) of the used data sets in this section."

The text of the manuscript has been changed to clarify these points.

11. "7-18: the ceilometer is mentioned here (and ceilometers were mentioned in the introduction, see remark there), but the data are not actually used (as you mention in line 23). So please remove this."

The sentence appears here since the ceilometer data is an obvious target to apply the algorithm to, so readers might expect to see the ceilometer data appear in this paper since they may be well aware of the presence of a ceilometer at the Cabauw site. We feel that the statement pertaining to this should be part of this section.

12. "7-25: The wind profiler uses very different information on the ML than the backscatter lidar. However, it is unclear whether/why the MLH retrieved from wind profiler data is superior to other methods. And hence it is unclear whether the profiler data are considered as a reference, or rather as an alternative method. Please clarify."

It is correct that it is unknown which method is superior. We do not present either method as a standard, which is also the reason why we have used several methods, none of which we consider standard, to verify the performance of the new Pathfinder method.

13. "8-16: the STRAT2D algorithm appears here at once, without earlier introduction in the methodology section. It is unclear why STRAT2D would be an important reference. Is it the de facto standard to derive MLH? What are the general characteristics of the STRAT2D algorithm? In which way is it similar and in which ways different than the Pathfinder method?"

    This point is acknowledged and the text has been changed to include statements about the status of the STRAT2D method in the introduction of Sec.4, as well as some of its main characteristics. However, we feel that for a detailed description of the STRAT2D method, we can (and should) refer to literature.

14. "8-11: Please note that Vogelezang and Holtslag (1996) only tested the the Richardson number method for stable conditions. So please provide references that show that the method also works for neutral and convective conditions."

    Vogelezang and Holtslag (1996) did include a generalization to near-neutral conditions besides their discussion of stable conditions. However, Seifert et al. (2000) also applied the method to unstable conditions, showing that the method can be applied to all different regimes. We will add a citation to Seifert et al. (2000).

15. "8-18: The manual estimates of MLH have not been introduced and defined before. They should have been defined in the methodology section, rather than at the place where the results are presented."

    The reviewer is correct and even though it is a short description, the manual estimates should be introduced in the methodology section. The definition given

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in paragraph 4.4 (11-29 – 12-1) is moved to a new paragraph 2.1.2 called 'Manual inspection of lidar data'. The content of paragraph 2.1 has been moved to 2.1.1.

New paragraph: 2.1.2 Manual inspection of lidar data As an alternative method, MLH is determined manually. Even today, this remains one of the most powerful ways to determine MLH as the human brain can use knowledge on processes affecting ML development (e.g. time of sunrise and sunset or presence and type of clouds) to distinguish the correct MLH from other gradients. Plots of lidar RCS and gradient fields are visually inspected to determine MLH. Based on the same undelying data, this enables assessment of the performance of the Pathfinder method against another method.

16. "8-19: Linked to an earlier comment on the absence of a table in section 2.3.1: the fact that table 2 is referred to as a table of 'tuning parameters' of the instrument suggests that some of the 'guiding restrictions' would be instrument dependent. However, the values related to the guiding restrictions are not presented as such 2.3.1. In order to show to other researchers (with different instruments) that the method is versatile, it would be good to clarify (in section 2.3.1) which parameters are considered instrument specific, and which could be more general."

This is a valid point. A paragraph at the end of section 2.3.1. has been added to clarify this. Note that our own experience with the application of the algorithm was also limited to a single instrument, so that we cannot indicate for sure that additional parameters may have to be introduced for first introduction of the algorithm to other instruments.

The the numerical thresholds for clouds and positive and negative gradients are instrument specific because the backscatter intensity depends on the type of laser used and accompanying specifications. The strength of smoothing applied to the data is related to the signal-to-noise ratio of the lidar. The minimum altitude to search for MLH is determined by the lidar overlapfunction and is also instrument (and possibly even serial numer) specific.

The convective delay depends on the location of the measurements. Likewise, the maximum altitude during night and day are determined by the environment and referred to as 'climatology' in the text. The parameter values listed under graph are discussed in paragraph 2.3.2 and also more general

17. "10-28: The value of R^2 is probably dominated by the strong diurnal cycle in MLH (see major comment (b)). Therefore, I find the remark that the agreement is within 100 m 'almost for the complete period' more informative. Subsequently, the most interesting question is in which cases the deviation is more than 100 m. Apart from the type of statistic used, it is important to know whether the authors consider the wind profiler derived MLH as a reference against which the lidar results are judged."

Windprofiler measurements used in the 12-day period in this study are used to judge the other methods against. However, these windprofiler measurements were thoroughly checked because the automated output is not necessarily better than lidar derived MLH. Qualitative statement of "within 100m for 'almost the complete period'" replaced by a more quantitative statement that "90% of the Pathfinder MLH estimates fall within a range of 250m from the windprofiler estimate".

18. "11-2: The R^2 mentioned here is more meaningful (than those based on the full diurnal cycle), since now the full range of possible MLH's is only determined by the variation of 16 UTC MLH between the 12 days. This range is probably much smaller than the range of values occurring during the full diurnal cycle and hence the R^2 is more meaningful. The 5 UTC values probably suffer from the fact that the spread in MLH between days is small early in the morning. Hence, it is more difficult to obtain a meaningful coefficient of determination. The success for the 16 UTC data is due to the fact that the MLH is accumulating possible small differences between the days (in terms of insolation, partitioning between

sensible and latent heat flux, subsidence). Due this accumulation effect small differences can lead to significant variation in MLH by the end of the day."

Like the reviewer pointed out, it is hard to determine a common statistic which justifies all different comparisons in this study. The choice for R^2 alone was inadequate to judge the performance of the algorithm in all situations. As stated under general comment b) more statistics are added to better describe the results.

19. "11-24: The reason mentioned here for the failure of the Richardson method is related to the dilution of a rising parcel. But the Richardson method is not a parcel method: it only looks at the local Richardson number."

The Richardson bulk method is not a parcel method per se. Looking at the definition of the Richardson bulk method (Eq 9 from Seifert et al (2000)):

$$Ri_b(h) = \frac{gh}{\Theta_{v,1}} \frac{\Theta_v(h) - \Theta_{v,1}}{U(h)^2 + V(h)^2} \tag{1}$$

We can see that this method compares local and surface virtual potential temperature (Tv) everywhere and adds a correction for wind shear. Using a critical value Rc close to zero (Rc = 0.25, similar to Seifert et al., 2000) can only be exceeded when the local Tv is higher than at the surface. The combination of Rc close to zero and the sharp increase of Tv at the inversion the influence of the wind shear does not substantially alter the results in convective boundary layers. Except for the wind shear correction this is similar to a parcel method and therefore we argue that the Richardson bulk method can be called a 'de facto' parcel method.

20. "11-26: 'that the radiosonde ...': clarify here that you are talking about the De Bilt sondes."

Done

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21. "11-29: please stress here that the underlying data for the manual estimate and the Pathfinder are identical. Although one could consider this a weakness (there is no real independent check), you could also consider it a strength (you only check the algorithm, not the data)."

   This is exactly right. A sentence has been added: Note that the Pathfinder and visual analysis have been performed on the exact same underlying instrument data. This ensures that only the algorithm is checked, without introducing new independent data.

22. "12-3: I'm a bit puzzled here: is the quality criterion mentioned here part of the algorithm or not? It seems to be separated, since it can be included or excluded. Please clarify."

   We agree that the text is confusing here. The quality criterion (the ratio that is mentioned) is part of the Pathfinder algorithm, and it can be used in post processing of MLH data for selecting data according to the value of the criterion. We have adapted the text to clarify this.

23. "12-13, section 4.5.1: it is unclear here whether the different 15 minute windows of a day were shifted independently, or that the whole train of 15 minute windows for a given day were shifted forward and backward. Furthermore, I think that it is more important to know how sensitive the method is to the phase of ML development it is started at than the location within a 15 minute window (in case of ML development we are looking at shifts of much more than 15 minutes, e.g. an hour)."

   The sensitivity analysis is included in the study to give the reader an indication of the stability and consistency of the Pathfinder method. When two possible paths are competing for lowest total cost, including or excluding an additional timestep might tip the outcome to either of the two competing paths and therefore changing the MLH estimate of that time window and possibly subsequent timewindows.

The first sensitivity test shifts the whole train of 15-minute time windows forward and backward. These shifts are limited to 30 timesteps forward or 30 timesteps backward because 30 timesteps is the size of the 15 minute timewindows. So with a shift of 30 timesteps, the timewindows are at the exact same place again and consider the same timesteps and, given they have the same starting point, will produce the same result.

Not starting the Pathfinder method at sunrise, but in another phase of ML development would not test the Pathfinder method but the performance of the gradient method to find the MLH and give the method the right starting point. With this, these are two options. Either the gradient method finds (approximately) the same MLH as the Pathfinder would have, or the gradient method finds a different altitude as MLH. In the first case, as is tested in the shifting of timewindows, small disruptions do not change the outcome of the algorithm. In the last case, when the gradient method finds a different gradient the question indeed is whether or not the Pathfinder method can return to the correct MLH. As pointed out in the text, e.g. paragraph 4.2, Pathfinder can exhibit commitment to a wrong layer and this is a point of concern. However, letting the method start at a time when the ML is shallow prevents this problem.

Extra explanation is added to paragraph 4.5

24. "12-24: I don't see the link between an R^2 of 0.96 and a match with the reference run for 93% of the runs. Furthermore, it is not fully clear to me whether the correlations have been determined per day, and that these correlations have been averaged subsequently (what was the order of processing?). The same remark as made earlier applies here: determining a regression between two variables that share the same diurnal cycle is not very informative. Hence, focusing on e.g. the 16 UTC values only would be a more fair comparison."

This correlation is indeed calculated per shift per day and then averaged to arrive at an monthly average correlation for each sensitivity run / shift. The lowest

average correlation was found between the run with a 10 timestep window and the default run, being 0.96. The other runs had an even higher correlation. At the same time, 93% of the individual MLH estimates exactly matched the default run. We chose to drop the statistic of R^2 and describe the results using mean bias and RMSE.

25. "12-27: I suppose that with an increasing window size the overall maximum growth rate (of 1 m/s) also was still applied to the entire window, see line 5-26."

    Yes, this is the case.

26. "13-1: I assume that the correlation mentioned here is between the MLH with the default window size, and the alternative window sizes. Of course this correlation is below 1, since you changed something in the algorithm. But the real question should be: does the change in window size make the MLH estimates better. Since the analysis with the May 2010 data does not include data from a reference method for MLH, this improvement (or lack thereof) with varying window size cannot be judged."

    Connected to general comment c) we acknowledge that validation of the sensitivity runs would help to understand the settings of the parameters better. However, we did not find independent data with a similar or higher quality (resolution, precision and reliability) to use as a benchmark. We like to stress that we feel that the main contribution of this paper is the introduction of the Pathfinder algorithm based on graph theory.

27. "13-19: are you referring to high spatial resolution or temporal resolution (or both). Please specify what you consider high resolution (this means different things to different people)."

    Indeed, high resolution is a bit arbitrary. However, we are referring to both spatial and temporal. Excluding large parts of extra gradients that might distract the

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shortest path algorithm, prevents the need to apply strong smoothing filters or averaging. This way, resolution similar to the measurements native resolutions can be used to determine MLH on. In terms of lidar measurements this means a resolution of 30 seconds and a vertical spacing of 15 meters. Line 13-19 is changed accordingly.

28. "14-4: 'climatology is used'. I had the impression that for the first window of the day the height with maximum gradient was used. And that for the subsequent windows the result from the previous window was used. If climatology is used, please specify what this climatology is based on."

This line is out of place here. The fact the gradient method is used to start the analysis of a day is discussed in other parts of the paper and as far as the authors are concerned no point of concern to be discussed in the Discussions section. The sentence "When the analysis is started, an initial point is needed. For this, climatology is used" has been removed

29. "14-19 to 26: this short summary of the method is not very clear. I think it would help to verbally draw the picture of a grid with height slots in the vertical and time on the horizontal axis."

The text has been changed: Pathfinder puts a full day of lidar measurements arranged in a time-height matrix and subsequently devides the matrix into time windows of 15 minutes. These 15 minute blocks are then translated into graphs in which each individual data point represents a vertex. To estimate MLH, exactly one point has to be selected for each timestep. For the selection procedure a weight is assigned to each vertex, where the weight is inversely proportional to the gradient at the point in the graph.

30. "14-27: in the discussion of results it is insufficiently clear what length of period the quoted correlation coefficients refer to (12 day period, May 2010, full year)."

These suggested additions to the text have been made.

31. "14-30: the R^2 = 0.21 is a rather random quote to show the success or failure relative to radiosonde data. You compare a value obtained by regressing values at one time of day (0.21), to the result of a regression based on data that span the full diurnal cycle (0.90). Furthermore, the 16 UTC data gave a better regression. Please paint a complete picture in terms of numbers, or just describe the results in words."

    We have removed the numbers and summarised the results in words as follows: Excellent agreement was found between MLH estimates of the Pathfinder method and from windprofiler during a 12 day period, but comparison to collocated radiosonde data was more problematic, we believe, due to limitations in the Richardson bulk method. Pathfinder results were also checked against manual/visual MLH retrieval applied to the same data, as well as the results from a different algorithm, STRAT2D, also applied to the same data.

32. "20: the figure suggests that in one time step MLH can only move one level up or down. Does that mean that the vertical resolution of the grid is fixed to the 75 meter? I had the impression that the vertical resolution of the data is higher. So in reality more levels can be jumped? Perhaps the reason is that the figure is merely meant to show the logic of the method (which it does very well!). But at least clarify the vertical resolution of the actual grid used (in the text, section 2.3.2)."

    The reviewer is right that the vertical resolution of the data is higher (15 meters for the UV-lidar data), with a timestep-to-timestep restriction of 75 meters this means the algorithm can shift 5 levels up or down between subsequent timesteps. Text has been added to clarify the figure is a simplified view of the algorithm and the actual vertical restriction can span over more than one level.

Very detailed comments

1. "1-24: the link between MLH and the surface energy balance is through the surface fluxes that drive the turbulence in the ML (that is: the link between surface fluxes and turbulent processes was missing in your causality chain)."

   Added this missing link in the explanation to the text.

2. "2-6: here at once the ceilometer occurs, which was not part of your enumeration in the sentence before. If you want to treat ceilometers and backscatter lidars as one type of instruments (the start of line 10 with backscatter lidar suggests that), then better use one term for them."

   We indeed treat ceilometers and backscatter lidars as one type of instruments as their measurements are based on the same principles. We removed the word ceilometer from sentence 2-6.

3. "5-28: 'However', the contrast with the previous sentence is not fully clear. I guess that what you want to say is the 0.278 m/s is valid for the mean growth whereas you intend to follow the turbulent growth."

   Explicitly added this distinction in the text

4. "5-29: 'These': it is not clear to which conditions this refers back."

   Deleted this sentence as it gives the exact same information as the first line of the paragraph.

5. "10-18: to clarify the difference between sections 4.3 and 4.4, I would suggest to change the title to 'Comparison with other methods – 12 day period' (then 4.4 would become 'Full year analysis of midday MLH')."

   The text has been edited to clarify this.

6. "10-24: 'overview lidar' → 'overview of lidar' "

   This change has been applied.

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7. "11-8: extinguished → distinguished "

   This change has been applied.

8. "11-20: remove 'data used in this study' "

   This change has been applied.

9. "12-23: 'correlation . . . of . . . with' → 'correlation . . . between . . . and' "

   This change has been applied.

10. "13-10 to 14: I think the 'This . . . ' in the last sentence refers to the '. . . to jump . . . ' at line 10. But for this connection to be clear, the sentence 'In this particular . . . ' interferes. Perhaps better move this sentence ('In this particular . . . ') to the end of the paragraph (after '. . . the correct solution.'). Furthermore, at line 13-12 I would suggest to insert 'until' between 'exists or' and 'the guiding'. "

    The text has been edited to clarify this. First, the behaviour is described and we removed 'in this particular'-sentence from explanation.

11. "14-6: check sentence 'with if' "

    This change has been applied; 'with' has been removed.

12. " 20, figure 2: please indicate which panel is a, b, c, d. "

    This change has been applied; letters were added in the upper-left corner of the panels.

13. " 21: what does the solid line in the figure mean. Is this the result of the Pathfinder method? "

    Response: The solid black line indeed is the Pathfinder MLH estimate of that day. This was added to figure description for clarification.

14. " 25: figure 10:Too small to interpret. Perhaps you could make it a full page figure with 2 times 6 figures? "

   This point is acknowledged and the figure will be changed to a full-page in the revised version of the paper.