

General remarks to the editor

We would like to thank the editor and the reviewers for their time and valuable remarks. As described hereafter, we have invested great efforts in order to improve the manuscript in light the reviewers' remarks. Several major additions have been made: the results from the validation experiment were expanded and in addition to the retrieved effective radius, we examine the retrieved LWC as well. A new section was added to the manuscript (section 5), which presents a case study of a single measurement during the passage of a natural thin cloud over our sensors. In addition, an appendix with technical details regarding the main instrumental device was added. Many additional small changes have been made, and we believe the revised manuscript is indeed more scientifically sound.

Authors' reply to Reviewer #1

At first, the authors would like to thank the reviewer for his helpful remarks, and we will address them point by point (reviewer's remarks in bold).

The authors describe an interesting new technique to retrieve droplet effective radius of very thin liquid water clouds from ground-based spectral measurements in the IR window region. The method is based on techniques developed for the detection of aerosol and trace gas plumes. It is described very systematically from theory over method development and sensitivity analysis to experimental validation using a droplet generator. Especially the latter is a most interesting aspect I've hardly ever seen in cloud remote sensing before.

Authors' reply: We thank the reviewer for his good words. Indeed such a "lab" validation of the method was something we wanted to bring to the table despite the limitations of the method. In the proposed method, we retrieve both effective radius of the cloud's droplets, liquid water path, and the optical thickness. Indeed, special emphasize was given in the manuscript to the retrieval of the effective radius for several reasons: Many retrieval algorithms compare the magnitude of the measured signal and some predicted signal, which is produced by a radiative transfer model. The uniqueness of our method is that it first compares the spectral shape of the signal. The spectral analysis serves as a trigger to the retrieval algorithm, since low SAM value indicates that the signal originates from water droplets. This is an important

point, because the result is a standalone passive remote sensing method, which does not depend on any a priori knowledge about an actual passage of a cloud above the sensors. Therefore, we expect the method not to be biased by any specific definition of "what is a cloud". As far as our method is concerned, a cloud is defined simply as an ensemble of water droplets, regardless of their size. As the reviewer noted, the principles of this method originate from the field of remote detection and identification of gaseous and aerosols plumes for environmental application such as pollutants detection. In this application, employing spectral analysis is vital in order to determine the properties of the plume. In our method, the spectral analysis enables first to determine the effective radius of the cloud. Only then, the liquid water path (and therefore the optical thickness) is determined. Since utilization of spectral matching methods is a relatively new technique in the field of atmospheric research, we have spent substantial part of the manuscript to present the theoretical basis and the validation experiment (which was specifically designed to validate the aspect of retrieval of the effective radius).

Weaknesses, in my opinion, exist mainly in the weak discussion of the relevance of the object of study and of the limitations of the method. While the former might have been neglected due to the fact that little attention was payed to the subject of very thin clouds by the scientific community before, the latter is clearly presented in the manuscript, but nonetheless hardly stated in the final discussion section.

I recommend the acceptance of the manuscript to AMT after some changes.

Authors' reply: We accept the reviewer's comment and improved the discussions regarding the relevance of thin clouds, and the limitations of the method.

The relevance of thin warm clouds is discussed in the introduction of the manuscript. We have discussed two main points: first, since most of the measuring techniques focus on well developed clouds, there are knowledge gaps regarding the properties of the thin clouds themselves. Second, the frequency of such clouds is unknown, although studies suggest that thin, small clouds should be numerous. These knowledge gaps imply further studies should be made in order to investigate these poorly monitored clouds. We believe the proposed method can be of great value to gain better understanding of the effect of such clouds. The proposed method is of

merit even in light its limitations for several reasons. First, even when the retrieval of the effective radius is not precise, the information regarding the liquid water path of thin warm clouds is of great value. Second, as described below, there is a known relationship between small LWP values and small effective radii values. This leads to the reasonable assumption that when thin (small LWP) clouds are considered, they are expected to be characterized by small effective radii, which is exactly the region where the proposed method has its best capabilities.

Regarding the method's limitations, we have added the following to the discussion: "The theoretical limitations of the methodology have been discussed, and the current estimate is a liquid water path range of 0.065-49.26g/m², and a lower limit of about 0.01 visible optical depth. The method is particularly efficient for clouds with effective radii of up to 4µm." (page 24 lines 16-19 in the revised manuscript)

Major issue 1 - limitations of the method

page 9, line 16 and following: You already state here that even for monodisperse droplets the capability of the retrieval might be limited to size derivation for very small droplets only.

page 11, line 3: Again you state that you only expect to be able to separate the size for droplets smaller than 3 micron with your retrieval method.

page 22, line 16-22, figure 22: Once more, the accuracy of your method seems to be limited. Retrievals larger than 3 micron are mostly off by up to 50%, often they are even not within your range of 10 most likely solutions, and for the largest particles the range of possible solutions covers the whole possible size range. I find your statement somewhat over-positive.

section 5 And again, I find your summary/ discussion overly optimistic given that you encountered some decisive weaknesses of your retrieval (and admitted to it throughout the largest part of your manuscript). Your method is limited to quantitative results for the size range up to 3 or 4 microns. Above that you can only see that droplets are of large typical cloud droplet size. Talking about the "twilight zone" near clouds, this is would be still very interesting information. Please clarify.

Authors' reply: Indeed, as the reviewer noted, and as stated clearly in the manuscript, the proposed method is most suitable for clouds with effective radius smaller than $4\mu\text{m}$. However, due to mixing/entrainment processes there is a strong relationship between the LWP and the effective radius. Thin clouds are characterized by small effective radius. In light the reviewer's comment we have added the following to the manuscript:

“In spite of the knowledge gaps regarding the nature of thin, warm clouds, the relationship between their LWP and effective radius was studied. Liu, et al., (2003) found high correlation between clouds' LWP and their effective radius: Thin, undeveloped clouds, which contain small amounts of LWP, are characterized by small effective radii as well. In fact, Liu, et al., (2003) showed that the majority of clouds with LWP smaller than $25\text{g}/\text{m}^2$ had effective radius smaller than $5\mu\text{m}$. Luckily (and as presented later in this manuscript), our proposed method is at its highest quantitative sensitivity for clouds with small effective radii.” (page 4 lines 18-25 in the revised manuscript)

Major issue 2 - aerosol sensitivity test

page 17, line 28: Now you are applying the 50 m layer method, I was already confused about for water vapor (see later issues), to the aerosol. This, in my opinion, is not a hard test for your retrieval, although you state it repeatedly. In contrast to the water vapor, you do not have a chance to get a good estimate of the aerosol situation in retrieval situations (or do you expect that?). Thus the 50 m thick "dust storm" like aerosol perturbation you test is less critical, and maybe optically less thick, than a usual but unknown summer aerosol situation in the whole boundary layer of say 1500 m. Thus I don't find this "obvious" (page 18/line 2). Please change this test or convince me and other readers that you don't have to.

Authors' reply: Since the proposed method is based on subtraction of a clear sky spectrum, theoretically it is insensitive to the general aerosol loading in the boundary layer, for all practical situations. It is more sensitive to **fluctuations** in the aerosol loading. The proposed analysis (section 3.2.2) introduces a situation where the **fluctuations** in the aerosol loading are severe, and happen in a small layer of 50 meters. The density of the aerosols enhances their radiative effect (mainly due to

effects of multiple scattering), therefore we have written that: “Obviously, this assumption enhances the radiative effect of aerosols on the sky spectrum“.

Major issue 3 - relevance of very thin liquid water clouds

page 4, line 24: I noticed here and somewhere later that you do not give a maximum optical thickness value? See also later issue (page 10). What is the upper optical thickness limit of your method? Could you please add it or comment on it.

Authors' reply: Since our method is aimed to retrieve the properties of thin clouds, we have focused our efforts in stretching its capabilities towards the thinnest clouds possible. Nevertheless, the connection between LWP, effective radius, and optical thickness is straightforward and is given in Eq. 4 in the manuscript. The complete range of parameters (including the maximal optical density) is provided in Table 2 in the manuscript.

page 10, line 5: Miles et al. (2000) show mean effective radius smaller about 2.5 μ only for two out of hundred cases! This leads to the decisive question whether you develop a technique for something worth measuring. I think yes, but you might want to discuss the issue in more detail.

and

page 10, line 6: I would very much prefer to see a 2D probability density function of Miles et al (2000) data and of your selection of forward setups. This could replace Figure 6 completely and add important information to tables 1 and 2.

Authors' reply: This is an important point. Indeed Miles et al. (2000) provides a collection of extensive in-situ measurements but most of the remote sensing and in-situ measurements are biased towards thick, developed clouds. In fact, until lately we could not find much of motivation to study thin/small clouds especially from in-situ measurements. Currently there are more and more papers studying small clouds either for their cloud forcing role or for their mistakenly attributed to the aerosol forcing. For example, there are several papers suggesting that within a cumulus cloud field the clouds distributed in a power-law distribution making the number (and even the overall contribution to albedo) of small clouds significant (see Koren et al., “How

small is a small cloud?”, 2008 for example). Therefore, in the light of these findings we think that there are much more cases of small/thin clouds with small amount of LWP and small effective radius. We use Miles as a reference just to give a sense to the reader that our simulations are reasonable in terms of the selected liquid water content and effective radius. We do not use Miles data to establish our simulated droplets size distributions, therefore we don't see how a 2D probability density function of Miles data and of our selection of forward setups would be informative.

As pointed above, and in light the reviewer's remark, we have added a discussion regarding the connection between LWP and effective radius. This discussion should persuade the reader that we have developed a technique for something worth measuring (page 4 lines 18-25 in the revised manuscript)

Other issues:

Page 4, line 17, "The purpose ...": What are the properties you want to talk about? Please state here. Reading on, I have the impression that you should retrieve optical thickness in addition to the effective radius. Isn't optical thickness the most important information? Can you comment on the problems doing so?

Authors' reply: In light the reviewer's remark, we have added a clear statement regarding the properties: "The purpose of our study is to introduce a new concept to determine and retrieve the effective radius, LWP, and optical depth of very thin warm clouds by using ground based IR spectral measurements". (page 4, lines 25-27 in the revised manuscript)

page 5, line 1: I can not remember that I find this structure - "1. identify physical phenomenon", "2. best solution" - in your later retrieval description. Please correct/comment.

Authors' reply: This structure is the heart of the retrieval method and it is clearly stated in section 2.4 ("Retrieval methodology"): "C. Retrieving the microphysical and optical properties of a cloud whenever a substantial increase in the measured sky radiation occurs: Using a spectral match index (SAM analysis for example) between the measured signal and the spectral library. This spectral analysis will serve as a trigger for the retrieval process by identifying that the source of the measured signal is

indeed water droplets. The second stage is comparing the magnitude of the measured signal with the spectral library signals and choosing a set of possible solutions of liquid water path, effective radius, and optical depth of the cloud.”

page 5, section 2.1: Please state clearly in the beginning of this section, that you first look at monodisperse droplet sizes and only later on realistic size distributions.

Authors’ reply: The seventh sentence in section 2.1 is “Following that, we analyze the effect of thin monodisperse and polydisperse water clouds on the measured sky LWIR radiance”.

page 6, line 24: I have no idea what "standoff detection" is. Might be my personal problem and totally clear to everybody else. But if not, could you add a comment?

Authors’ reply: The reviewer is right. We used this term without explaining it. “Standoff detection” is a common term used in the field of remote detection and identification applications for environmental and pollution monitoring. This means detection of pollutants by remote sensing techniques without a need for in-situ measurement. In light the reviewer’s remark we have rewritten the sentence to be: “Therefore, we have used techniques borrowed from the field of remote detection and identification of gaseous and aerosols plumes for environmental applications” (page 6 line 31 - page 7, line 1 in the revised manuscript)

page 8, line 18: Can you please clarify what "inactive" "stable" weather is. Stable inactive weather could mean weeks without sun and thick fog and stratus clouds. Which is probably not what a Israeli summer is like.

Authors’ reply: we have added a short description of a typical Israeli summer day: “During the Israeli summer, there is a steady downwelling warm air settling over the Middle East that originates from the tropics. Therefore, a strong inversion layer is present, and only very shallow clouds can form. The occurrence of precipitation is almost impossible. In addition, the all area is dominated by steady westerly winds which arise from a constant low pressure over the Persian Gulf. This low pressure itself is a branch of the Indian summer monsoon.” (page 8 line 30 - page 9 line 3 in the revised manuscript)

page 9, line 14, Fig 5 and all following "cross SAM" plots: Can you introduce this tool with a few more sentences here. What is a good cross-SAM value? Later on page 13, line 23 you state "10_" and promise more details in section 4 (which are not given there, as far as I remember). Please state it here. You might even add a line to the cross-SAM plots.

Authors' reply: This point is better clarified in the revised manuscript. We have added figure 2 (in the revised manuscript) that visualize the SAM method. In addition, we have extracted subsection 2.2.3 (which introduces the SAM) to include the following: "The process of subtracting the clear sky reference creates a differential spectrum. In order to classify the phenomenon which caused the differential signal, we assess its spectral similarity with a predicted set of library spectra which was pre-calculated. As commonly applied for identification of known reference spectra (Yuhua, et al., 1992, Park, et al., 2007), we have utilized the SAM - Spectral Angle Mapping (Kruse, et al., 1993) analysis on the differential spectral signatures (Figure 2). The SAM considers the spectral signals (each consisting of n wavelengths) as vectors in a n -dimensional space, and calculates the angle between two spectra as a measure of their similarity. The angle between two spectra can be any value between 0° (perfect match), through 90° (orthogonal spectra), to 180° (two spectra pointing to opposite directions). Namely, distinct spectra will produce relatively large SAM values, while similar spectra will produce small values. As in any remote sensing retrieval, a certain threshold must be applied, and its value depends on the application and should be determined empirically. In the proposed method, and as described later, we have applied a SAM threshold of 10° . The SAM metric is especially efficient when small spectral features are present, even though the complete spectral behaviour appears similar. Throughout this study, the SAM method has been applied on spectra which contained 16 wavelengths in the region of $8\mu\text{m}$ - $9\mu\text{m}$ and 51 wavelengths in the region of $10\mu\text{m}$ - $13\mu\text{m}$. These wavelengths were chosen to match with the spectro-radiometer features used in our measurements (Section 4). The spectral region of $9\mu\text{m}$ - $10\mu\text{m}$ was deliberately omitted since it contains the wide ozone absorption line (McCaa and Shaw, 1968), that might induce errors in the analysis."

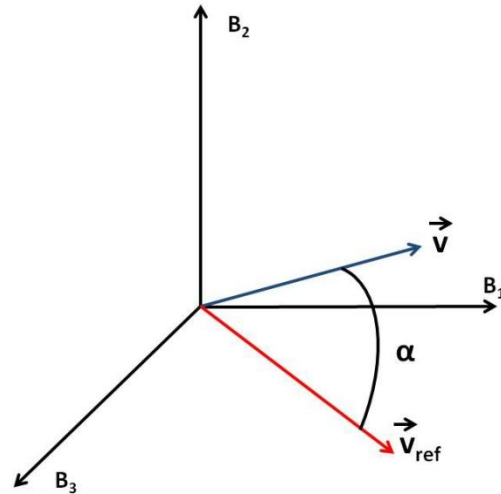


Figure 2 - An example of the SAM (Spectral Angle Mapping) index of two spectra. SAM considers a measured spectrum in n wavelengths ($n=3$ in this example) as a vector in an n dimensional space (B_1, B_2, B_3). The spectral similarity between a measured spectrum (V) and a reference spectrum (V_{ref}) is simply the angle α between these two spectra, calculated to be $\alpha = \cos^{-1}\left(\frac{\vec{v} \cdot \vec{v}_{ref}}{\|\vec{v}\| \cdot \|\vec{v}_{ref}\|}\right)$. Distinct spectra will produce relatively large SAM values, while similar spectra will produce small values.

page 9, line 21: Please avoid the abbreviation of the unit "micro-meter" to just "mu" (mu being the greek single letter). Better write "mu m" as in line 19 on this page.

Author's reply: We have changed all the "μ" to "μm".

page 11, section 2.4, Figure 8: Do you need figure 8, if you repeat the same things in the text "A" ... "C" here?

Author's reply: We agree figure 9 (in the revised manuscript) repeats some of the information in section 2.4, but we believe it visualizes the essence of the retrieval algorithm.

page 15, section 3.2.1: I first wondered why you chose the very exotic variation of water vapor in a 50 m layer just beneath your cloud and not situations with vapor content changing over the whole sub-cloud column. Later I understood that your general requirement is that you need to know the background vapor

profile anyway. Thus you only check the impact of fluctuations in water vapor between your local situation and nearby soundings/ pre-calculated soundings. Right? (1) This all does not become very clear here. Please clarify. (2) The systematic check how your retrieval might be impacted by vapor profiles which are not part of your forward data set is only discussed in 3.2. Think about moving section 3.3 in front of 3.1.

Author's reply: The general logic behind this analysis appears at the beginning of subsection 3.2: "The purpose of the proposed methodology is to extract the properties of water clouds with very small optical depth, by analyzing the spectrum and the magnitude of the signal. Fluctuations in relative humidity, aerosol loading, and haze are also characterized by small optical depths. Since the proposed method is based on subtraction of clear and cloudy zenith sky spectrum, it is essential to examine whether such fluctuations and thin water clouds can alter the sky radiance in a similar way (in terms of spectrum and magnitude). If such similarities exist, the proposed method might falsely interpret such fluctuations as thin water clouds. In addition, when thin water clouds do exist in the sensor's FOV, fluctuations in water vapours and aerosols might induce bias to the retrieved parameters". (page 16 ,lines 5-13 in the revised manuscript)

In general, section 3 deals with the sensitivity of the method. We start with the general limitations of the method due to signal to noise, inherent spectral features in the sensor, and number of bands (subsection 3.1). We follow with sensitivity analysis regarding fluctuations in the atmospheric profile (subsection 3.2). We conclude with analysis regarding the robustness and efficacy of the method for global use (under wide range of different atmospheric profiles - subsection 3.3). We believe this structure is the most reasonable one, as every subsection expands the scope of the sensitivity analysis. Moving subsection 3.3 in front of 3.1 would lead the reader to believe the method is robust before he even knows what its basic limitations are.

page 16, line 22 ... and figure 13: Might be better to give the liquid water path values instead of a arbitrary LWC in your 50 m layer.

Author's reply: The mentioned LWC are the liquid content inside the examined cloud. These values are not arbitrary and are not in the 50 meters layer below the cloud. In

the caption of figure 13, it is clearly stated “The horizontal scale is the LWC of the simulated clouds.”

section 4, page 21, 2nd paragraph: From this discussion the question arises whether there should not be a section 3.4 "Sensitivity to droplet size distribution shape". Please think about it. At least show something on the size of errors caused when you apply a mod-gamma distribution derived retrieval on the admittedly unusual spray distributions.

Author’s reply: To the best of our knowledge, all remote sensing algorithms, which are based on droplets size distributions, assume clouds follow modified gamma distribution. However, as stated in section 4, comparing Figures 22 and 8 (in the revised manuscript) enables to get an impression of what is the result of applying the wrong size distribution: “Figure 22 presents the cross SAM matrix of the differential signals which were created for the validation experiment. Comparing Figure 22 and Figure 8 reveals the differences between the validation experiment and the theoretical capabilities of the method regarding natural clouds. Two main distinct areas exist in the cross SAM matrix in Figure 22: spray with effective radius of less than 4 μ m differs from spray with higher effective radius. The ability to distinguish between sprays within the two areas is quite small.” (page 22 ,lines 10-16 in the revised manuscript)

Technical corrections:

page 12, line 14, wording: "method’s range" should maybe be something like "method’s range of application".

page 17, line 2: "Apart the" -> "Apart from the"

page 18, line 9: "that" -> "than"

page 21, line 6: "above" -> "over"

Authors’ reply: all corrections have been made.