Reply to the referees of " Perturbations in relative humidity in the boundary layer represent a possible mechanism for the formation of small convective clouds"

We would like to thank the two reviewers for their efforts and for the comments that helped us improve the paper. It is now presented in a clearer and more complete way. We have addressed all of the reviewers' comments and we are confident that with the additional information on the exact type of clouds we study and the new findings the paper is clearer. We will start our response to the reviewers with a general part that is relevant to both of the reviewers followed by point-by-point answers.

General comment

Given the review input, we take the blame for not being clear and for not emphasizing properly the key messages and the novelty of our findings. In the revised version, we made any possible effort to make the messages focused and clearer. Specifically, both of the reviewers referred in their comments to trade cumulus clouds. In the paper, we discuss the formation and dissipation of small clouds that are overlooked in most cases and that are different from trade cumuli. These clouds are warm, thin and short-lived that appear for a few minutes and then dissipate. They were shown previously to be important for both estimations of cloud forcing and for estimations of the properties of cloud free skies. We suggest here that such clouds can be the dominant cloud type in cases where the regular methods of forecast predict no cloud formation at all. In those cases, the LCL, that is used as an estimate for cloud base height, and is calculated for a group of parcels that originate near the surface, is much higher than the inversion layer at the top of the boundary layer. This is in essence completely different from the cases of trade cumulus or any convective cloud type that is typically studied in the cloud physics literature. Moreover, we investigate here the origin and the fuel of this unique and delicate convective motion. We show that the origin of the convective motion that creates those small convective clouds is located aloft in the mixing layer (and not near the surface as commonly considered). Additionally, we study what is the source for the initial perturbation that starts the convective motion (positive buoyancy) and show that the only reasonable perturbation is in the relative humidity (RH), and not in temperature. We are aware of the large body of works presented in the literature on the boundary layer dynamics and on trade cumulus formation. In the revised paper, we discuss better the novelty of this study.

In light of the reviewers' comments, we have taken great efforts to revise the manuscript and to emphasize the above points. First, and in order to emphasize the uniqueness of the studied clouds and their environmental conditions, we changed the title of the manuscript. We now call it: "On the formation of small (<100m) clouds in conditions that predict cloud-free atmosphere".

Second, we have completely revised the abstract of the manuscript:

"Recent observations of small convective (hesitant) clouds suggest a discrepancy between the measured clouds base height and the much higher lifting condensation level, estimated for a parcel rising from near the ground. These clouds are understudied since they are small (~100 m) and their lifetime is only a few minutes. The observations suggest that these clouds appear under conditions of weak updraft with a thermal inversion layer above them. A unique air parcel model, tailored to resolve processes in the haze-to-cloud regime, was developed to study the formation of such clouds. It is common to consider the convective motion as originating by thermals or plumes from near the surface. Considering the atmospheric conditions, the air parcel model shows that these clouds cannot be the result of such classical thermals or plumes. It suggests that the origin of the convective motion is relative humidity perturbations of elevated air pockets aloft in the boundary layer. These results explain the existence of small clouds that standard methods fail to predict and shed light on their formation mechanism."

Third, we have added a complete section to the introduction of the manuscript for describing the clouds we are studying:

"In this paper, we study the formation of a special subset of convective clouds that is usually overlooked. We refer to a sparse cloud field with a typical cloud size of only few 10s of meters and a lifetime of few minutes (previously defined as hesitant clouds, see Koren et al, 2009). Most of the clouds in such fields are much smaller than the common spatial resolution of climate-oriented space borne sensors. These clouds (Figure 1) are characterized by small liquid water path (LWP<~10 g m^{-2}), weak optical signals and short lifetime (minutes).

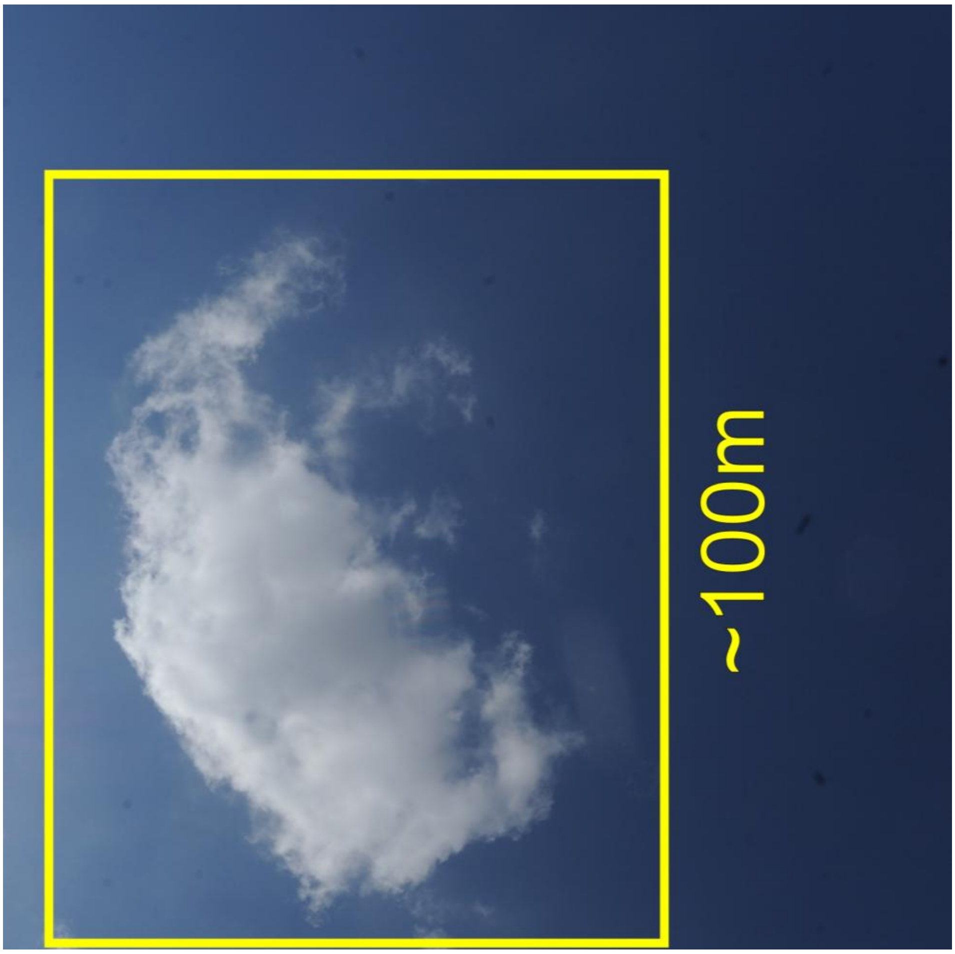

Figure 1 - a typical eastern Mediterranean summer cloud. This image was taken during a field campaign (Nes-Ziona, Israel, at 30 June 2011 15:54 LT), while the camera was pointed to the zenith.

Nevertheless, such clouds are common during the summer in the eastern Mediterranean region. In a recent ground-based field campaign that was conducted in Israel in the summer of 2011, such cloud fields were observed in 27 days out of 92 measurement days. These clouds often appear in cases when the standard forecasting tools would predict no clouds formation. In those cases, the predicted cloud base is above a strong boundary layer inversion. In this study, we examine the mechanism and the environmental conditions that favor such clouds formation. It is important to note that we are not studying clouds that are simply the (left) side of the size distribution of cumulus clouds that appear together with larger ones. Rather we study cloud fields that are composed only of these very small hesitant clouds. ”

Fourth, we have added an opening paragraph to the discussion section:

“This paper studies the formation of a special subset of convective (hesitant) clouds that are often too small to be detected. Sparse cloud fields composed of small (few 10’s m) clouds would often be below any standard detector resolution and would be classified as cloud free regions. We show that these clouds can form under conditions that predict an LCL range that is higher than the base of the boundary layer inversion. We also show that the origin of the convective motion is in elevated RH pockets located aloft in the mixing layer, rather than thermals starting near the surface.“

More additions have been made along the manuscript. These changes are detailed hereafter.

Reply to anonymous reviewer # 1

We thank the reviewer for his useful comments. We found the mentioned references relevant to the topic discussed in the manuscript and we used them to improve the background description. However, as stated above we feel that the main points of the paper were disregarded. To make our points clearer we will refer to the reviewer's comments point by point. The reviewer comments are written in **bold**.

My main concern is that the authors raise an issue - relative humidity, or humidity, perturbations are important for cloud formation - that is widely known in the cloud community.

Authors reply: We agree with the reviewer. No doubt that the importance of relative humidity perturbation in cloud formation processes is known. Our message is much more focused. We present a proof that the very small clouds (those which are overlooked in all previous papers, to the best of our knowledge) that form under the described conditions, can form due to a perturbation in relative humidity (RH) originating higher within the boundary layer and not near the surface. More specifically, we present a mechanism for the formation of this special subset of very small clouds where the origin of the convective motion is specifically treated. It is shown that the origin of the convective motion that creates small convective clouds is in the middle of the mixing layer (and not from near the surface as commonly considered). In addition, it is demonstrated that the initial perturbation that starts the convective motion is in the RH and not in the temperature (as commonly considered). To the best of our knowledge, these messages were never reported in the scientific literature, including in the references that the reviewer pointed, as detailed below. In light of the reviewer's comments, we have revised the abstract of the manuscript and emphasized these messages in the introduction and discussion sections as well.

The discussion presented is negligent of a couple of decades of scientific work on cumulus clouds and far behind what one could call the frontier of science.

Authors reply: We thank the reviewer for the relevant references that were implemented into the background part presented in the revised introduction. Nevertheless, as pointed above, while appreciating the large body of work done on bigger convective clouds, the key messages of our manuscript do not appear in any of the references the reviewer mentioned. We think the scientific content of our study is new and important.

It is well known that the sub-grid variability of both temperature and humidity play a role in controlling cloud formation, and it is also well known that the lifting condensation level should really be viewed as a wide range of lifting condensation levels belonging to parcels with a wide range of temperatures and humidities due to the heterogeneity of the sub-cloud and near-surface layer. This is even mentioned in Stull's book, which the authors reference.

Authors reply: Stull (1988) discusses boundary layer clouds (chapter 13) and we assume the reviewer refers to subsection 13.4.3 which is titled "cumulus onset time and cloud cover". The first paragraph of this subsection presents the range of LCL's and its physical base: "There is a thermal-to-thermal variability in temperature and moisture associated with the variability in land use over which thermals form. Thus, thermals penetrate to a variety of heights, and have a variety of LCLs. The range of penetration heights has already been defined as the entrainment zone. The range of LCLs defines the LCL zone (Wilde, et al., 1985)". Stull considers the source of the convective motion to be near the surface, and explains the LCL zone by variations in the thermals properties. All the scientific models of thermals (see for example Pruppacher and Klett, 1998 and Turner, 1969), consider them to start ascending from near the surface. Unlike the classics of cloud physics, our manuscript suggests that the origin of the convective motion can be from the middle of the mixing layer, and it is not necessarily related to "land use over which thermals form". Pockets of high humidity can result from evaporation of previous clouds for example. We introduced this issue to the discussion: "A possible source for pockets of high humidity can be dissipation of previous clouds." (Page 18, lines 14-15)

Nevertheless, and in light the reviewer's comment we added the following to the introduction of the manuscript (Page 4, lines 21-24):

"It is common to consider a range of LCLs (and not a single height) for specified atmospheric conditions as it represents the variability in the thermals properties that is associated with the variability in land use over which the thermals form (Stull, 1988)."

It is also well known that parcels are not undiluted and instead feel the environmental structure through mixing and entrainment processes. Observations of that date back to the early work of Malkus et al (1953) in her discussion of how thermal models are no accurate representations of real-life cumulus clouds, despite what Scorer and Ludlam based on their laboratory work indicated.

Authors reply**:**

We agree with the reviewer that clouds are not perfectly adiabatic. We know the work by Malkus (1953) that describes how the properties inside a cloud cannot be explained without considering the effects of mixing with the outer air and of the entrainment process.

Our theoretical cloud model treats entrainment processes in a limited but reasonable way for studying small clouds formation in weak updrafts conditions. Equation (4) which iteratively solves the acceleration of the parcel, considers the effect of the entrained air on the updraft, as commonly applied in parcel models. Since all the equations are coupled, it influences all the other processes treated by the model. We added the following to the description of the model in the manuscript (Page 8, line 26 - page 9, line 2): "The following term represents the effect of the entrained air on the updraft. Since all the model's equations are coupled and the updrafts are small, this formalism represents reasonable treatment of the effect of the entrained air on the parcel motion (Pruppacher and Klett, 1998)".

In addition we mention the model's limitations in the discussion (Page 18, lines 15-17): "So even though our theoretical parcel model have some limitations as it does not account for all the implications of entrainment for example, it still points to a possible mechanism for the formation of small convective clouds"

An important paper is written by LeMone and Pennell (1976) on the structure of the sub-cloud layer and trade-wind cumulus formation preceded by the nice piece of work by Malkus (1958), who was the first to observe the so-called transition layer that is often found near the top of the mixed-layer, which the author's observe in their sounding

Authors reply: Thank you for pointing out these references. LeMone and Pennell (1976) describe the relationship of trade wind cumulus distribution to the sub cloud layer fluxes and examine it for different types of Cu clouds. However, the authors did not point to the origin of the convective motion as we do in our study.

It is even more noteworthy to mention that papers that cite LeMone and Pennel (1976) use this reference to prove that clouds are the end result of thermals, which originate near the surface. For example, Siebesma et al, (2007) describe a combined eddy-diffusivity mass-flux approach for the convective boundary layer and clearly state: "thermals ultimately can be considered as the invisible roots of the clouds that feed on them (LeMone and Pennell 1976)".

In light the reviewer's comment we have added the following to the introduction of the manuscript (Page 4, line 25 - Page 5, line 5):

"Much has been studied about the structure of the boundary layer in general and of the sub-cloud layer and its relationship with the cloudy layer above in particular.

Lemone and Pennell (1976) showed that the distribution of trade wind cumulus clouds is determined by the structure and moisture fluxes from the sub cloud layer, and noted that suppressed small clouds are the result of moist buoyant air parcels which are collected by the updraft of organized circulations in the subcloud layer. Lemone and Pennell (1976) also reported that momentum and moisture fluxes are enhanced under cloud patches, and that cloud "roots" could be followed 100-300 m below cloud base, into the sub-cloud layer. This coupling between the clouds and the subcloud layer is noted to be stronger for deeper clouds. Nevertheless, it is still widely considered that the surface is the source for the moisture flux that leads to the formation of clouds (Siebesma et al, 2007)."

Parcel models, either run offline or as part of a convection/cloud parameterization in a climate models, consider both temperature and humidity in the parcel updraft, as well as dilution of the parcel even before cloud formation, such that it would feel the temperature and humidity structure of the sub-cloud layer.

<u>Authors reply</u>: We are aware of other parcel and single column models that are currently being used. In fact "Parcel model" is a general and not too informative name for many different models aimed in studying many different aspects of cloud physics. Our theoretical model was developed specifically for this study and it is presented better in the revised manuscript. It follows the vertical motion of a pocket of humidified aerosols; it explicitly solves the water vapor uptake of every aerosol in the parcel; and it determines when the pocket of aerosols can finally be called a "cloud" when the relative humidity exceeds 100%. It is important to emphasize that the model treats the parcel evolution below the cloud base. It treats the thermodynamical and microphysical evolution of an air parcel <u>as it ascends and transforms from a pocket of dry aerosols, through haze, and eventually becomes a cloud</u>. It solves the most fundamental equations: the first law of thermodynamics, diffusional growth of haze and cloud droplets, the temporal behavior of the RH within the parcel, and the driving updraft.

Using this model, we have succeeded to describe the convective motion of small air parcels. We believe that this model (taking into account its limitations) is an adequate tool for studying this problem of small clouds formation under weak updraft conditions.

In light of the reviewer's comment, we added the following to the general description of the model (Page 7, lines 15-17): "The model iteratively solves the equations for the energy budget of the parcel, the growth of the haze droplets (and later of the droplets), and the updraft and RH of the parcel", and later (Page 7, lines 22-27): "Unlike many parcel models in which the parcel's updraft is prescribed, in our theoretical analysis, we developed an air parcel model that is tuned to resolve the initiation of small buoyant air pockets that form due to weak perturbations in temperature or humidity. In order to account for the delicate processes that take place in the transition from haze to activated cloud droplets, the model solves the thermodynamic equations from first principles. "

As stated in the manuscript, our model considers the effect of mixing and entrainment on the updraft (Eq. 4 in our manuscript). Since all the equations in the model are coupled and under conditions of weak updrafts it is a reasonable representation of the effects of mixing on the ascending parcel.
We added the following part before introducing equation (4) (Page 8, line 27 - page 9, line 2): "Since all the model's equations are coupled and the updrafts are small, this formalism represents reasonable treatment of the effect of the entrained air on the parcel motion (Pruppacher and Klett, 1998)"

Even more noteworthy is the entire collection of state-of-the-art cloud schemes that are currently used, which include the sub-grid scale variability of both temperature and humidity, most of which have suggested that observing the variability in humidity is more crucial for the accurate prediction of cloud cover than the variability in temperature is (Sommeria and Deardorff 1977, Mellor 1977 and many after e.g. Neggers 2009). In the methods that the authors present, little reference to such statistical schemes is mentioned, neither how one could deal with the heterogeneity of the sub-cloud layer.
Authors reply: The reviewer refers to papers that describe cloud models which represent the variability in the temperature and humidity in the environment. Nevertheless, and as we already stated above, our manuscript deals with the origin of

the convective motion. None of the mentioned references considers this issue. Furthermore, many of the cloud schemes use the Eddy-Diffusivity Mass-Flux (EDMF) scheme to represent the planetary boundary layer. Such scheme is presented by Siebesma et al. (2007) for example. They consider the convective motion to start near the surface.

In light the reviewer's remark we have added the following part to the discussion section of the manuscript (Page 17, lines 12-19): Many cloud models treated the variability in temperature and humidity below the clouds in a statistical manner (Sommeria and Deardorff, 1977, Mellor 1977). These studies pointed on the importance of representation of the sub-grid scale variability of the humidity in order to obtain realistic results, and set the stage for modern state-of-the-art cloud models that are used to describe boundary layer convection (Neggers et al, 2009). Nevertheless, due to the statistical nature of these models, none of them pointed explicitly to the origin of the convective motion, and it is widely considered to be originated from the surface (Siebesma et al, 2007)"

Instead the authors present a very detailed description of the equations needed to model the thermodynamic properties of an air parcel after condensation has occurred, which appears to have little reference to the issue raised, namely how variations in temperature and humidity before condensation/cloud base height play a role in determining the location of cloud base height.

Authors reply: As we described above, this is a key point that was not emphasized enough in the first version. The model that was developed for this study treats the parcel evolution carefully below the cloud base. It treats the thermodynamical and microphysical evolution of an air parcel as it ascends and transforms from a pocket of dry aerosols, through haze, and eventually becomes a cloud. It solves the most fundamental equations: the first law of thermodynamics, diffusional growth of haze and cloud droplets, the temporal behavior of the RH within the parcel, and the driving updraft.

The parcel model solves the most basic thermodynamic and microphysics equations of haze and droplets growth, and it provides good agreement with reliable measurements of cloud base height. We believe that this model (taking into account its limitations) is an adequate tool for studying this problem of small clouds formation under weak updraft conditions.

Reply to anonymous reviewer 2

We thank the reviewer for his efforts. As stated above, we have taken great efforts to revise the manuscript and emphasize its main points in a better way. Obviously, we strongly disagree with the reviewer's conclusions. To prove this, we will refer to the review point by point. The reviewer comments are written in **bold**.

This study uses a parcel model to study the formation of convective clouds. The authors claim in the abstract that clouds form in their model well below the lifting condensation level. If this were the case, then they would have clouds forming at subsaturated relative humidities, which seems incorrect. I presume that the authors mean that the clouds form at levels below the level of the mean LCL. This is not surprising either. It has been known for a long time that cumulus clouds tend to form in anomalously humid parcels (often the parcels are humidified by evaporation from a moist surface).

Authors reply: In the manuscript, we show observations (not model) that indicate of many days with clouds base height far lower (~600 meters) than the theoretical height predicted by the LCL. We use two different methods to calculate the LCL: one uses a parcel with the average properties of the lower 500 m of the atmosphere and the second one uses a parcel with the properties of the air near the ground. As the reviewer noted himself, this is puzzling, because one would expect such measurements to indicate "**clouds forming at subsaturated relative humidities, which seems incorrect"**. The whole purpose of our manuscript is to explain these puzzling observations. Later in the manuscript, we show that our parcel model can explain this alleged discrepancy between the measurements and the LCL predictions, and that the only possible explanation is that these clouds are the result of perturbations in the RH in the middle of the mixing layer, and not thermals that originate near the surface.

The authors do not provide a justification for where the high humidity parcels originate, but my guess would be that the surface is the source. If this is the case, then it is difficult for me to see what is really new about this work. A detailed parcel model of the type employed here is not needed to understand the situation. Much work has been carried out since the late 1970s to represent the humidity and temperature in the PBL as stochastic variables (either correlated or uncorrelated). The work of Mellor and Deardorff (both 1977) set the stage for this concept, which has, since the early 1990s found its way into parameterizations used in GCMs.

Authors reply: The reviewer states that "**the authors do not provide a justification for where the high humidity parcels originate but my guess would be that the surface is the source**". The whole essence of our manuscript is to point to the origin of the high humidity parcels. The reviewer's statement that "**my guess would be that the surface is the source"** proves that the ideas which are presented in our manuscript are novel. It is commonly considered that the surface is the source for the convective motion, but our manuscript suggests that the convective motion originates from the middle of the mixing layer. As a matter of fact, we show that surface perturbations (either in the temperature or in the RH) cannot lead to clouds forming at the heights in which they were measured.

As we replied to reviewer 1, the mentioned references deal with cloud models which represent the variability of the temperature and humidity in the environment. Nevertheless, our manuscript deals with the origin of the convective motion. None of the mentioned references considers this issue

In its current state, I cannot recommend this paper for publication because it does not appear to offer significant advances over what has been known for years. The authors will need to work hard to make it clear what is really new here.

Authors reply: As described in details above, we believe the reviewer missed our key messages and we strongly disagree with the reviewers conclusions.

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