Response

We thank both reviewers for their constructive and helpful comments.

We will first address the main point made by the reviewers, followed by responses to the individual points and some general revision notes.

Both reviewers point out that a modified simulation design is required to investigate how stratocumulus properties would respond to a long-term change in wind speed, and that the current design does not address this question. We agree. The answers and insights we give were obtained by analysis of the short time scale response, and are only a stepping stone for understanding the long-term response of stratocumulus clouds to changes in wind speed. The following changes were made in the manuscript:

Abstract

How do~~might~~ marine stratocumulus clouds and their radiative properties respond to ~~future~~ changes in large scale wind speed?"

"We present an investigation of the dynamical response of non-precipitating, overcast marine stratocumulus clouds to different wind speeds over the course of a diurnal cycle, all else equal."

Introduction

"The simulations are hence a suitable framework for identifying and characterizing the mechanisms by which the stratocumulus-topped marine boundary layer responds over a diurnal cycle to different wind speeds, all else equal. However, they do not represent a stratocumulus-topped marine boundary layer in a future climate at different wind speeds, which would require initial and boundary conditions that are consistent with the chosen climate and wind speeds."

We have added passages to the introduction to highlight the contrast between this study, which focuses on the response of the stratocumulus cloud deck over a diurnal cycle, and works that investigate the steady state response of stratocumulus clouds to climate change parameters (Schubert et al, 1979; Jones et al., 2014; van der Dussen et al., 2015; …).

Conclusions

"We have investigated the response of non-precipitating, overcast marine stratocumulus clouds to changes in large scale wind speed over the course of a diurnal cycle, all else equal."

"Owing to identical initial and boundary conditions, the simulations are suited to identify and characterize the mechanisms by which the stratocumulus-topped marine boundary layer responds on the time scale of a diurnal cycle to different wind speeds. "

We have added the following paragraph at the end of the conclusions section:

"On longer (climatic) time scales, wind speed may act differently on the CRE than in the course of one diurnal cycle. We hypothesize that on longer time scales, a higher wind speed would also render the nighttime, non-precipitating, stratocumulus-topped boundary layer more decoupled, and less decoupled at

lower wind speed. During daytime, the effect of wind speed on decoupling on longer time scales will depend on whether production of turbulence in the sub-cloud layer by shear from large scale wind supports or suppresses vertical moisture transport from the surface to cloud base. The response may depend on local conditions. Key questions are how future changes in large scale wind speed will modify cloud properties and the CRE on longer time scales, and how their effect compares to the effect of changes in sea surface temperature, atmospheric moisture, CO_2 *content, subsidence strength, and inversion stability."*

Response to reviewer 1

1. p 396 lines 2-4, p 398 lines 23-24, p 399 lines 9-10, p 400 line 3, p 400 lines 19-20: The simulations presented cannot answer this question (how might marine stratocumulus clouds and their radiative properties re- spond to future changes in large scale wind speed?) because they are not run to equilibrium.

We agree. In the present manuscript, we only identify and characterize the mechanisms by which the stratocumulus-topped marine boundary layer responds over a diurnal cycle to different wind speeds. We have amended the text to reflect this (see introduction above). This subject has to our knowledge not been investigated previously and represents a stepping stone for understanding the long-term response of stratocumulus clouds to changes in wind speed.

2. p 397 line 15: There are many more recent papers that could be cited. There are many such papers in JAMES, for example.

We added Brient and Bony (2012), Bretherton et al. (2014), Dal Gesso at al. (2015), and Zhou et al. (2013) to the list of references.

3. p 402 lines 18-19: Why use such a large domain size? It might be better to use a smaller domain and smaller grid sizes. The grid sizes are about 3x larger than recommended for LES of Sc

Smaller domain sizes produce a different wind speed response than larger domain sizes. This phenomenon is currently under investigation. We find that the response becomes independent of domain size at domain sizes > 30 km ⨯ 30 km. A 30 km domain gives a very similar wind speed response as a 60 km domain. We have therefore used 30 km as the smallest domain size in the simulations. This has dictated a compromise on grid spacing.

In Appendix C, we show that the key phenomena investigated in this work are robust to a doubling of resolution in each dimension.

4. p 403 lines 8-9: Omitting sedimentation of cloud droplets will affect the entrainment rate (Bretherton et al. 2007).

This is correct. However, sedimentation and collision-coalescence of cloud drops are disabled only in the spin-up runs, in order to prevent drizzle formation at the start, when dynamics has not established itself yet. In the simulations, sedimentation and collision-coalescence of cloud drops are not disabled. We have added a sentence in the text to clarify this.

5. section 2.1.3: Matching observations is not critical for a sensitivity study like this one. For that reason, this section could be greatly shortened, or moved to the appendixes.

We are frequently asked the question how we know that the model is "right" - even when, as the reviewer points out, that understanding may not be critical. It helps to establish "real-world relevance" by using an observed state as a reference point. This said, we do make a key point in this section, namely the justification of aspect ratio choice in our simulations – something we have been asked to do when presenting this work at conferences.

6. p 412 lines 17-21: The (updraft) mass flux profile might provide a useful measure of the changes in BL circulation. See Krueger et al. 1995a,b for examples. Those studies were the first to document the deepening stage of the stratocumulus to cumulus transition. As far as the BL circulation is concerned, increasing fluxes due to SST increase or wind speed increase are no different because the BL sees only the surface fluxes, not the processes that produce it.

We did analyze the total water (updraft) mass flux profiles in our simulations but did not present the analysis in the text. The reason for not including the discussion of these fluxes is that they do not exhibit a consistent response to wind speed at all altitudes (see Figure C6 and C9). We hypothesize that it is the strongest updrafts which contribute to enhanced TKE production and entrainment as the surface moisture flux increases, while the overall total water updraft mass flux need not to show an increase with wind speed. This is under investigation.

We have added the reference to Krueger et al. (1995a, b).

7. p 412 lines 23-4: One way to quantify the circulation strength is with the mass flux profile.

See previous point.

8. p 412 line 27 and p 413, line 1 “Circulation” is ambiguous. I don't think it is synonymous with TKE however. I think instead of mass flux as being a measure of circulation.

We have changed the passage

"The relative roles of buoyancy- and shear-driven circulation in driving the evolution of the boundary layer will be discussed in Section 3.2, where we show that buoyancy-driven circulation is the fundamental mechanism of enhanced boundary layer growth and entrainment at higher wind speed."

to read

"The role of buoyancy- and shear production of TKE in driving circulation and the evolution of the boundary layer will be discussed in Section 3.2, where we show that buoyant production of TKE is the fundamental mechanism of enhanced boundary layer growth and entrainment at higher wind speed."

9. p 413 line 5-7: Why doesn't the enhanced entrainment counteract the surface flux increase and decrease the LWP? You should explain this.

Enhanced entrainment does suppress LWP, but for this reduction to counteract the surface moisture flux increase caused by a higher wind speed, the LWP reduction would have to sufficiently slow down residual circulation in the boundary layer, to compensate the effect of the higher large scale wind speed.

10. p 412-3 section 3.1.3: The last two paragraphs may be too much detail. Summarize instead.

We have shortened the text; please refer to the revised manuscript with highlighted changes.

11. p 414 lines 6-7: It is the decoupling that increases conditional instability and updraft speed.

Conditional instability involves latent heating which drives the rise of an air parcel that is surrounded by clear air. An example is the cumulus-under-stratus state. In the stratocumulus layer in our simulations, the updrafts that drive additional TKE production by latent heat release are located inside the cloud layer.

12. p 414 lines 11-12: The radiative warming is distributed throughout the BL if the BL is not decoupled. Decoupling restricts the warming to the upper layer, but not because it is cloudy.

Shortwave heating can take place exclusively inside the cloud layer even in well-mixed boundary layers, see, e.g., Fig. 2 in http://www.jstor.org/stable/37136?seq=1#page_scan_tab_contents.

13. p 414 lines 22-24: This is a consequence or even a definition of decoupling (actually, not completely decoupled).

Decoupling and moisture flux at cloud base are not equivalent in their information content. Decoupling is related to the difference/ratio of the surface and cloud base moisture flux. The cloud base moisture flux can change independently of decoupling (provided that the surface moisture flux changes accordingly).

14. p 415 lines 7-8: Such a scenario does not occur in simulations of the transition to cumulus as the surface latent heat flux increases due to SST increase, so it should not happen due to wind speed increase.

We have changed the passage:

"This mechanism could potentially ~~result in a runaway breakup of the cloud, or initiate or~~assist in the transition from stratocumulus to shallow cumulus along a gradient in sea surface temperature (Bretherton, 1992; Bretherton and Wyant, 1997; Wyant et al., 1997)."

15. p 417-419, section 3.2.2: There is a lot of detail which makes it hard to follow. Maybe a schematic diagram would be helpful.

This section was shortened in the revised version of the manuscript to improve readability.

16. p 421 lines 3-4: These are the transient responses! For climate change, need the equilibrium responses, as in van der Dussen et al. (2015).

We have amended the text to reflect the focus on the short time scale response of the system to wind speed changes (see introduction above).

17. p 421 lines 22-23: Can this be quantified with a generalized turbulence velocity scale like Moeng and Sullivan proposed for the CBL?

We consulted Moeng and Sullivan (A Comparison of shear- and buoyancy-driven planetary boundary layer flows. J. Atmos. Sci., **51**, 999–1022, 1994) and other pertinent sources for the analysis of the results, and found that the interaction of buoyancy- and shear-driven dynamics and the underlying mechanisms and causal relationships would require investigation beyond the scope of this work. We have therefore limited the discussion to phenomenological aspects of buoyancy and shear effects.

Technical corrections

Please see the annotated manuscript for the technical corrections (highlighted in yellow; red underlines should be ignored). Note that the comments listed above are also included on the annotated manuscript.

1. Fig. 2: Please add uncertainty or std dev to observed values if possible.

We have added uncertainty bars and ellipses to the observational data.

Are the light gray shaded areas supposed to indicate this? If so, it is not clear from the caption.

The meaning of the gray areas is specified in the caption, *"Mean final state (black) and values between the 10th and 90th percentiles (light gray, in plots with observations) of the spin-up run* ***m*** *..."*

We have changed Figure 2 to simplify viewing and removed the shading representing th 10th - 90th percentile range.

2. Fig. 3:

What time period do these cover?

These time series in this figure cover the 24 h duration of the simulations. The simulations start on 11 July 2001 at 04:00:00 UT, and end at 12 July 2001 at 04:00:00 UT. We have added a corresponding statement to the text.

Is the forcing steady in the spin up period?

We use a constant sea surface temperature, a constant large scale wind speed, and a constant large scale divergence (specified in the text), and nudging of mean potential temperature at all levels, and of total water content in the free troposphere as described in App. A1. The nudging is equivalent to forcing by advective tendencies of temperature and moisture that maintain the (mean) potential temperature and moisture profiles.

Why does the wind speed vary in the nocturnal case?

We think that this is the result of an interaction of the Coriolis effect with the residual circulation. This is under investigation. However, since this is only a hypothesis at this time, we have removed the corresponding sentence from the text.

Why aren't the initial values the same?

We have not plotted the initial value in these time series. Not plotting the first value of the time series was pre-defined in the plotting routine. We have corrected this in the revision, and the initial value is plotted.

However, initial values of quantities such as the surface sensible and moisture fluxes will still differ, because they are calculated from the prescribed, different wind speeds.

Furthermore, in time series that were filtered with a low-pass filter to reduce noise, the initial values (values at ½ of the filter length) will also differ.

3. Figures 3, 4, 6, C1, C2, C3: There are too many panels. Are they all necessary? I recommend no more than four panels per figure.

We have split the figures to reduce the numberof panels per figure.

Response to comments in supplement

Specify LW or SW CRE. I think you mean SW.

We added a passage to clarify that we refer to the total cloud radiative effect (SW+LW).

The reviewer points out that we do not investigate equilibrium solutions, which may be more representative of the response of the system to wind speed on longer (climate) time scale.

We agree, and modified the text as descried in the introduction (see above).

The reviewer asks why we only investigate the non-precipitating Sc case.

Precipitation in the simulation adds complexity to the system; we have decided to investigate the most basic case.

The reviewer asks why we assuming $F_{entrainment} = 0$ in the spinup runs.

The entrainment flux is associated with temporal changes in mean inversion height (App. 2). Since in the spin-up runs, the mean potential temperature profile is nudged towards its initial profile, the mean inversion height is constant (following an initial adjustment period of ~ 90 minutes), after which the entrainment flux becomes 0.

Why did you homogenize the surface fluxes?

The surface fluxes carry information on cloud structure (Kazil et al., ACP, 2014). Without homogenization, this information carries over to a cloud deck forming when using prescribed fluxes. The effect is quite small, however. We do not know the underlying mechanism, and therefore decided to eliminate the effect.

In reference to the calculation of the surface sensible heat and moisture fluxes (Eq. 3, 4) - are these local, grid point values?

Yes.

As far as the BL circulation is concerned, it is no different! The BL sees only the surface fluxes, not the components that produce it.

We agree. We have changed the passage in question to reflect this; it reads as follows:

“This mechanism also proceeds in the initial stage of the transition from a stratocumulus to a shallow cumulus cloud state, in which an increasing sea surface temperature drives a stronger surface moisture flux, which in turn increases TKE production by latent heat release at cloud level and thereby entrainment (Bretherton, 1992; Krueger et al., 1995a, b; Bretherton and Wyant, 1997; Wyant et al., 1997).”

The reviewer points out that the following statement in the text is already well known:

“This, together with the concomitant behavior of cloud updraft TKE_w *and ve in the course of the diurnal cycle, in response to wind speed, and to the presence/absence of shear due to geostrophic wind suggests that cloud layer updrafts are a key dynamical driver of boundary layer growth and entrainment.“*

“Stratocumulus clouds“ (Wood, MWR, 2012):

“Broadly speaking, there are two classes of formulations for w_e*: (i) flux-partitioning closures assume that* w_e *adjusts to maintain a constant ratio of some measure of TKE-destroying (negative) buoyancy fluxes to the TKE-producing (positive) buoyancy fluxes (e.g., van Zanten et al. 1999, for a review); (ii)* w_* *closures assume that* w_e** scales with the vertical integral of the buoyancy flux irrespective of how the TKE is produced (Deardorff 1976).”*

We believe that our findings contrast with at least the second of these views, in that they identify a mechanistic connection between a TKE production mechanism and entrainment. This connection has, to our knowledge, not been identified previously.

We have made the text more specific:

“This, together with the concomitant behavior of cloud updraft TKE_w *and* w_e *in the course of the diurnal cycle, in response to wind speed, and to the presence/absence of shear due to geostrophic wind suggests that cloud layer updrafts are a key dynamical driver of boundary layer growth and entrainment* in stratocumulus clouds.*”*

General revision notes

In the revised version, wording has been improved in a number of locations. Together with the changes requested by the reviewers, these changes are highlighted in the revised version by blue (new text) and gray (removed text).

Values in Table 3 of the revised version give the top-of-atmosphere (TOA) total (short-wave + longwave) cloud radiative effect, while the top-of-domain total (short-wave + longwave) cloud radiative effect was given in the original version.