Case study of a humidity layer above Arctic stratocumulus using balloon-borne turbulence and radiation measurements and large eddy simulations

by

Egerer, Ehrlich, Gottschalk, Neggers, Siebert & Wendisch

Manuscript # acp-2020-584

Introduction

With an excessively long and tedious title, this study focuses on so-called moist inversions capping Arctic stratocumulus, starting with observations and extending the discussion with both LES and trajectory calculations. The authors are blessed with probably the only extensive observational dataset on particular situation that exist, and this work has potential to become really groundbreaking. Unfortunately, the manuscript does not capitalize on this possibility; instead it gets bogged down by uncertainty and what could not be done because of various difficulties.

I'm quite sure the authors are sitting on a gold mine here; unfortunately, they don't seem to know what tool to use to excavate it. As read on, my thoughts gradually shift from minor to major revision, but with lots of detailed comments. However, having read to the end I find myself recommending that the manuscript is ***rejected***, but with a *strong recommendation* to the authors not to give up, but to explore other avenues of analysis to unlock some of the mysteries behind these cases.

I have many detailed comments that I would love to share with the authors if and when I get a better organized revised manuscript; below are a few overriding concerns.

General comments

Analyzing case-studies like this is difficult; even if you find something, it's hard to generalize, and when you find a technique to analyze the data there isn't much more data to test the generality of your method. This is like learning an art; you need to develop a sense of what is working and what is doomed to fail and you need to be ingenious and imaginative. I've been doing this for many years, and I still learn new tricks and its wonderful when finding that data suddenly makes sense!

Here, the authors seem lost; they don't seem to know – or at least they don't tell me – what they are really looking for, they have no map telling them how to get where they want to be, and they don't know what to do next. There is no clear hypothesis to test and even if there was, they don't seem to know how. The paper reads like a chronological description of how the work was progressing, and it probably is. I want a coherent narrative; a story focusing on the science.

After reading a while, it starts to appear like one goal is to find at least a portion of one flight from which they can get a measure of the downward flux of moisture. I fail to understand why knowing the size of that flux – from one case – is so important. But they have a lot of different data to analyze in different ways, so instead they test one thing after the other and keep running into the same proverbial brick wall at every turn; profile fluxes do not agree with constant altitude flight legs and time series look strange, etc. The text even starts with questioning the very existence of moisture inversions, which is off course fine! However, RH for the descending branch from BELUGA is not consistent with the suggested cloud outline; in the upper 50% of the cloud layer, RH < 80%; I do not believe that is the case.

Remote sensing retrieval software is wonderful and multi-sensor retrievals, like Cloudnet, has many useful features. This is, however, only true when used carefully and from an understanding of limitations and applicability. Here the authors are using Cloudnet retrievals like a very black box and it doesn't help much. So instead they bring in LES, which is perhaps an even larger black box but also doesn't help much; what is needed here is some careful thinking, experience and a new analysis strategy. The LES discussion is quite short, and I don't understand why one case is relegated to an Appendix while the other isn't, and it doesn't help at all. I would suggest to expand the LES study and make it a separate paper; base it on this study, by all means, but do the proper set of simulations to figure out the optimal configuration and then do all the different sensitivity simulations you need to extend and generalize whatever it is you find in the analysis of the observations.

There are so many ways an LES can be useful, but the way it is used here is not one of them. Multiple initial and boundary condition combinations can bring a simulation to appear similar to a single case-study profile, but there is only one that is correct and it is not always evident which one; most *appear* correct for the wrong reasons. There is much else to be said about this but most importantly, you should *never* use an LES to lend credibility to observations; it should be the other way around! If you have doubts about the observations, or how to use them, throwing in an LES does *not* help! Continuing along this line, the trajectory calculations looks intriguing, but the discussion doesn't seem to go anywhere; you need to do more to be convincing, or should just drop this line of inquiry.

What might help is to explore alternative analysis methods and/or looking at more sources of concurrent observations. I suggest looking more at the remote sensing data independently. For example, directly explore the Doppler data from the cloud radar. There are methods described in the literature how to estimate some turbulence statistics directly from the radar data (e.g. σ_w and ε). An upside to this is that you can find levels where the data comes from a constant altitude for well understood portion of time; flipsides are the lack of resolution and that only one parameter can be derived. But do look at the native time resolution; not the Cloudnet-filtered data.

I would also not walk away from the slant profiles just yet, although they take really careful hands-on analysis. There are several old papers where slant profiles by aircraft have been used to tease out profiles of turbulence statistics with realistic magnitudes and shapes. It does require careful filtering, however, I submit that the vertical velocity of the platform should make aircraft profiles harder to work with than the BELUGA data. However, I would, in contrast, advise against filtering data from constant height flight legs. A numerical filter can never provide a signal with a power spectrum looking anywhere near realistic. So just give it up and use Ogive analysis instead, to analyze the magnitudes of fluxes and variances.

A word of warning, however; if the signal looks like in Figure 7a, no filtering in the world will help. The interface between the cloud and the inversion layer is like the surface of a lake and what you see here is the effect of the sensor sometimes being under and sometimes above the "surface". The resulting signal is from two different environments and filtering the signal to make it look smoother will not make those environments the same or even similar; averaging statistics for turbulence over the resulting signal is therefore meaningless, and you need to do something else.

I see no reason to expect the turbulent flux here to be in any other direction than that dictated by the gradient; counter-gradient fluxes appear in deep convective boundary layers, and this is essentially either a near neutral layer close to the upper boundary, in the cloud layer, or a stably stratified environment, in the inversion. So using the flux-gradient approach makes a lot of sense, however, I don't understand the efforts to use parameterizations of the eddy-exchange coefficient, K_q, based on filtered higher-order moments. Why not get it directly from the sensible heat flux and the temperature gradient? If you anyway assume that $K_q = K_H$, this should give you what you want. With the method you use, you can both measure (by eddy-covariance) and calculate (with the flux-gradient method) the sensible heat flux; if the two are different, then you can't trust the parameterized moisture flux either. However, I would say that if the gradient is positive and the flow is turbulent, there's no question in my mind the flux is negative (downward); it just stands to reason, with what we know about turbulent flows. How large it is, is a different question; one that we likely cannot get a useful answer to from one case.

Finally, many are the papers that have tried to explain peculiarities in the results with gravity waves; I have even written at least one myself and that doesn't necessarily make me proud to admit. There are, however, methods to show if what you see are indeed buoyancy waves and not just something that happens to look wavy. So – either show up or let up; either you provide some evidence that there are gravity waves present or drop that line of hand-waiving arguments all together.