

Reply to Reviewer #1

This study synthesizes observations from the StraToFog field campaign in coastal Atacama to analyze the evolution and surface impact of marine fog that forms in that region. Overall, the campaign methodology is sound and comprehensive. Their multi-site and instrument method of observing the thermodynamic state is foundational to be able to comment on the evolution of these clouds. Their findings that coastal fog is ultimately driven by moisture advection from the diurnal land-sea winds, and the synoptic pressure state, is substantiated by their measurements and consistent with general boundary layer theory. Consequently, their arguments for the advection of the marine boundary layer over the terrain and into the surface desert air mass above is sound. Their key discussion on the fate of the moisture from this airmass interacting with surface fluxes seems plausible. The authors could address specific mechanisms as further evidence for the surface interaction.

We thank the reviewer for his/her positive assessment of our manuscript and for his/her concern about improving the quality of this work. We have carefully answered and corrected every reviewer comment in blue font below, indicating where in the revised manuscript we have introduced changes. We have paid special attention to carefully replying to reviewers' concerns about the stratocumulus cloud vertical structure and have strengthened our argumentation relating to water vapour adsorption as the mechanism of dehumidification of MBL when it is advected inland.

Specific Comments:

Line 135: The methodology of approximating the height of the boundary layer is sound, the calculations of the rate may be too definitive from their measurements. Is this a constant linear rate from the observations? If so, include that and perhaps some error bars.

The subsidence observed during the vertical profiles measurements resulted in a strong thermal inversion, which was very flat in the morning and became a bit tilted towards the afternoon. We estimated the MBL height using the maximum potential temperature gradient ($\Delta\theta/\Delta z$), which gives us one single height value per sounding. The rate of ascent or descent of the MBL was linearly interpolated between every sounding (2 hrs) using $\Delta h/\Delta t$, which resulted in the rate in cm/s shown in the text. Because we have one radiosounding every two hours, we do not have enough measurements to calculate an error bar from MBL height or rate of ascent. However, Taking into account the reviewer's comment, we have added an estimated error ($\pm$) to the total MBL height and the total rate of ascent/descent. The changes are performed between lines 139 and 140 of the revised manuscript.

Line 160: If you're going to use the LCL from a sounding, the assumption is that the parcels that are generating the cloud are starting from the very surface. I think this is a good assumption given it's reasonable to assume these marine clouds are formed from moisture fluxing off the surface, then rising. However, this is still an approximation, given that different layers of the atmosphere may be more moist (although, their measurements indicate the surface is the most moist). It would be good to quantify more of the uncertainty with this technique. Also, it's interesting that they approximate the cloud top with a different technique than the LCL. Why was this the case?

We agree with the reviewer that LCL is an approximation of the cloud base, which by its nature is not a well-defined line. This approximation is only thermodynamic: $LCL(z) = q(z) - q_s(z) = 0$; thus, it does not account for microphysical processes of nucleation, coalescence and droplet concentration, measurements that, unfortunately, we do not have. However, in boundary layer clouds, especially marine stratocumulus, the cloud base can be safely approximated using the LCL, as stated by Dyunkerke et al. (1996), using the adiabatic liquid water content ($q_l > 0$) to estimate the cloud base during aircraft flights. Likewise, Schween et al. (2022), using combined remote-sensing instruments (cloud radar) from the surface in the coastal Atacama, mentioned that LCL is representative of cloud base during the day if there is no drizzle (which was absent during the StraToFog experiment). Finally, Lobos-Roco et al. (2018), using LCL from observations compared with WRF simulations, found similarities between LCL and cloud base. In our case, we estimated the $LCL \sim$ cloud base using the

vertical profiles of radiosondes. Unlike the cloud base, the cloud top was estimated using the MBL height; here, we also use q_l profiles to determine where $q_l = 0$ above the LCL. Unfortunately, we do not have an independent method, other than soundings, to evaluate the method's uncertainty. However, to address the reviewer's comment, we have included the following sentences in section 2.1, lines 92-95 of the revised manuscript:

"From vertical profiles obtained by radiosondes and UAVs, we estimate the Sc cloud base through the calculation of the lifting condensation level (LCL) and the cloud top using the MBL height. The assumption of LCL as the cloud base has been used before in several field observations (Duynerke et al. 1996; Schween et al., 2022), where the uncertainty is reduced during the day and with no drizzle, which was absent during our campaign."

Line 190: The claim that your measurements at the first few meters of the surface are significantly different between site C and site E and therefore an indication of fog deposition and evaporation to me are weak. First, I'm not sure if you're using the same instrument in site C and E, it appears that one may be using a radiosonde, and the other a UAV. This would absolutely impact the results. If one of them is using a UAV, perhaps the drone sitting for a few minutes before launch could impact the first measurement being made. Furthermore, you have flux measurements of the 5 km, do these flux measurements match your claim? I see your point that you are arguing that it is a process that is happening at the very surface, but it's a big jump to your claim of fog deposition and evaporation from one measurement at the surface on one event. Did this happen at other times throughout the campaign?

We thank the reviewer for this observation, as it highlights a lack of clarity in our original description of the measurement sites and instrumentation, particularly in Table 1. The significant differences in the q profiles shown in Fig. 5b correspond to measurements at sites C and F, rather than C and E. We acknowledge that this distinction was not sufficiently clear in the manuscript, which may have led to a misunderstanding. To clarify the reviewer's doubts, at sites C and F (where q profiles show differences), we use the exact same radiosonde sensor (I-Met4) and the same radio to receive the sonde signal. In contrast, at site E we use the UAV sensor (I-MetXQ2), which is from the same manufacturer as the radiosonde and was previously checked; its differences with the radiosonde were very low and more related to the ascent velocity. To minimise them, we flew the drone at the same ascent rate as the sounding (5 m/s) using the ascent profile for comparison. In any case, in Fig. 5 we show that profiles in C and E are quite similar.

Regarding our claim that the difference might be a consequence of fog deposition and water vapour adsorption (no evaporation), this is preliminary at this point in the manuscript. However, this is further complemented by latent heat fluxes (Section 3.2.2), measured at site E (leeward; Fig. 9c), which, even though it might not be exactly comparable with site F, has more arid conditions than E-windward, for example. Such fluxes show a negative latent heat flux during the afternoon (16:00) when relative humidity is below 100% (no fog), which matches the decreases in q profile observed at site F. In addition, our measurements on the high-precision balance (Fig. 8) show an increase in the weight during the afternoon (15:00-16:00), which also corroborates our interpretation of water vapour adsorption and, later on (20:00), fog deposition (when relative humidity reaches 100%). As shown in Fig 8 and 9, which show a compilation of all days of measurements, we can say that this water vapour adsorption is not a single-event phenomenon but happens at least during the entire campaign. In summary, in section 4 (Fig. 10c), we interpreted this reduction of the q profile during the afternoon (Fig. 5b), the negative latent heat flux at site E leeward (Fig. 9c) and the positive change on the high-precision balance (Fig. 8a) as water vapour adsorption.

To take into account the reviewer's concern, we have introduced changes in Table 1 to make it clearer and changes in line 196 to show that this interpretation is, until this point, preliminary:

"This reduction in q when θ remains isothermal within the MBL during the inland advection suggests that atmospheric humidity is likely being retained in the coastal mountains (2 to 5 km inland)"

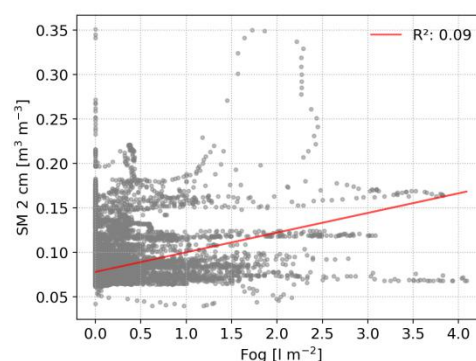
The system that is the fog deposition into the ground, is there some information in the literature of this process? I understand this may be out of the scope of the paper, but given it's a key part of the findings, a few lines arguing it's happening may be helpful. Additionally, is there a saturation and the speed at which this happens? Some comment on this process happening quickly at say the start of fog deposition (the ground is most dry) can help. Overall, some paragraph perhaps on the biology or soil moisture dynamics would help the author's surface interactions claims.

Regarding fog deposition, Alto Patache is a fog oasis composed mainly of bare soil, which is interrupted in some areas by patches of biological soil crust, very sparse cacti, and countable shrubs (as mentioned in the first lines of the introduction). There is a large list of studies showing that such ecosystems are sustained by fog, which is trapped through the foliar area to drip it into the soil. However, as far as we know, the impact of fog droplets on the surface (no deeper soil layers) has never been measured in the hyperarid Atacama before. The fog deposition we describe in our paper is exclusive to the surface and occurred during our campaign from evening to night under high wind speed (>5 m/s); thus, it would be a physical effect of droplet deposition rather than a biological mechanism.

Now, to clarify the reviewer comment, based on our visual observations during the campaign, fog does not infiltrate deeper than the first millimetre of soil (see picture below). The relationship between soil moisture and fog in deserts is also inconclusive in the literature. For example, Li et al. (2018: 10.1016/j.advwatres.2018.01.004) found a weak relationship between fog and soil moisture (4 cm depth) in 1/3 of the analysed sites in the Namib Desert. In our case, during the campaign we did not measure soil moisture. However, posterior measurements in Alto Patache show that fog events measured by a standard fog collector do not influence soil moisture at 2 cm depth (see the plot below). Since this is a paper based exclusively on atmospheric observations, a biological interpretation of fog deposition might be out of scope, and it is something that we cannot prove with our data.



Picture showing a very thin humid surface and dry soil underneath.



Relation between soil moisture at 2 cm and fog collection during 2026 (January to September).

However, with the intention of interpreting the reviewer's comment well and addressing it in the revised manuscript, we have introduced the following changes in lines 261-262 and 361-363:

"This fog deposition occurs under a wind speed > 5 m s⁻¹ (Fig. 7d); thus, it is likely a physical effect of fog droplets impacting the arid surface".

"Since the surface is partially covered by a thin biological soil crust, we cannot discard a biological mechanism affecting actively dewfall, evaporation, water vapour adsorption or fog deposition. However, this remains an open question since we cannot prove it with our measurements".

Technical Corrections:

Given this was an important part of your analysis, I would add to your q mixing ratio a subscript of q_v (for vapor) and q_t (total mixing ratio), similar as you did to q_l , to help the reader keep track of what mixing ratio you're referring to.

We agree, the changes has been introduced.

In the figure captions, it would be beneficial if you included the instruments used to make these measurements, given the complexity of the field campaign, and you use different instruments to make claims about processes happening at the same layers.

We agree, the changes has been introduced.