Reviewer 2 comments

The authors analyze the impact of a large set of dropsondes over the tropical east Pacific and the Caribbean on ECMWF's integrated forecasting system. Data from two IFS experiments (one making use of the dropsonde data and one not) are compared to study the impact on wind, temperature and humidity distributions. Additionally, the analysis is extended to selected convective parameters which are of relevance for studying developing tropical convection. This impact study is of interest to the community as it makes use of a unique data set and shows a model validation in a region where winds are known to be uncertain. However, it would clearly benefit from additional context and a more thorough discussion of the results. Please see the detailed comments below:

Major comments:

1. The introduction does not provide enough context to the study. It is only hidden between the lines why this work is done and what the questions and hypotheses are. So, I suggest to revise and clarify this section.

*L18: I do not see the distribution of the observations. I recommend that dropsonde locations or frequencies of observations per lat-lon bin (bins shown in Fig. 2) are shown on the map. How many flights were conducted in each box on how many days?
RESPONSE: Thank you for pointing this out. We have produced the figure 1 map with observation frequency. We also reference an overview paper Fuchs-Stone et al. (2020), with such specific details about flights not directly relevant to this study.

*L20ff: The reader would profit from information about the general aims of OTREC (instead of detailed results) and how this study is embedded. How were the flights designed? Did they happen regularly or was there a target process, region or time?
RESPONSE: Thank you for pointing this out. We added a short explanation of the goals of OTREC and the rationale for choosing flights and boxes.

*L25-32: What is the purpose of the paragraph? Please include a clear statement about the scientific aim of the study and why the IFS data assimilation system and the proposed method is suitable to adress it. Was there a plan for a dedicated validation in the specific regions? Was there a bias expected?
RESPONSE: Thank you for your suggestion, we have expanded this paragraph with a more accurate statement of scientific aim. Yes, the original OTREC proposal did propose to test model performance in the region, with an expectation of bias.

*L33ff: I am a bit confused about this paragraph as it covers a broad selection of studies making use of additional sonde data “in other regions of the planet” but it remains unclear what this implies for the presented study. The authors are referred to publications of Majumdar (2016, BAMS) and Parsons et al. (2017, BAMS) that summarize the results of targeted observations during THORPEX and beyond.
RESPONSE: Thank you for your suggestion. We expand the paragraph to be more clear.

2. The data and methods sections would benefit from a more thorough explanation of the model experiments and the used data products. It is confusing to me what type of data is used for what type of analysis:

*L73ff: This paragraph is a surprising start as this 3D-var approach was never mentioned before and it is not stated why it is needed.
RESPONSE: We added the rationale for using the 3D-var in this study. We use it for calculating mass balanced fields derived from the basic observed fields (zonal wind, meridional wind, temperature, and mixing ratio). The derived field which we use to identify convective and non-convective regions, in section 5, is moisture convergence.

*L79-84 (and elsewhere): Please specify “model data” and what is used in the departure calculation. Is it an analysis departure or a background (first guess) departure? Please explain how this can be interpreted? Why do only positive values denote a deficit? Please better explain the two experiments (YDPS and NDPS) and what one expects to learn from comparing them? Were these experiments cycled over the entire month or several single cycle experiments for each mission? Please give details about the blacklisted profiles mentioned in L166. I guess the analysis only considers data points that are used in both experiments, correct?
RESPONSE: I am a bit confused with the reviewer’s comment; the variables used for calculating departures are listed in the same paragraph, a few lines further.
We use analysis departures because those are what end users deal with when they receive the data from ECMWF, that is why we do not look at first guess departures.
The way we defined departures: observation minus model value, means positive values denote model deficit relative to observations. It was not a judgement on the results, it is just so by definition. We made that clear in the revised manuscript.
We better explain the experiments at the expense of being repetitive in the paper.
We also add details you requested; that the model was cycled over two months, and yes, the analysis only considered data points used in both experiments.

*L83: I guess there is no collocation done in the sense that data is interpolated in space and time, but observation space data is used from the data assimilation system? Is the restriction to significant level data a result of the transmission of thinned data to the GTS? Please explain what data was used and clearly separate observation space data

from gridded model data.
RESPONSE: Significant levels: The OTREC radiosonde data are high resolution data, but due to some unfortunate technical issues, the data assimilation system could at the time not assimilate them as high resolution BUFR data, but only on significant levels, resulting in some loss of information.

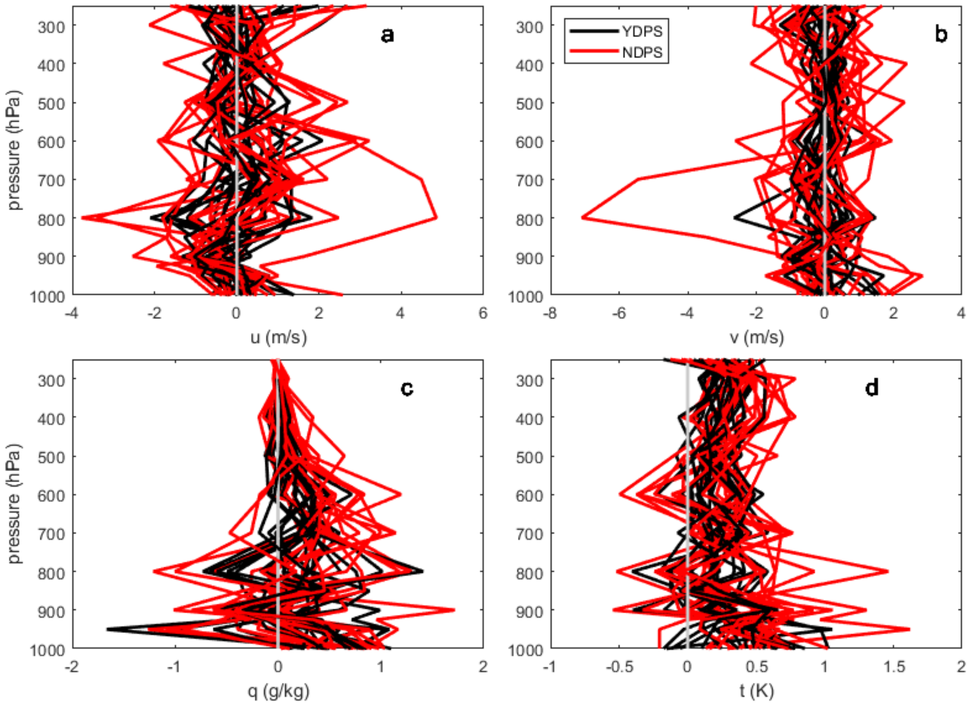
*L102: Here, I guess that “operational data” means gridded model space data? Do the 1x1° cells mean that data is retrieved at this grid resolution? Is the operational analysis data used here? What is the vertical resolution? L108 says that also ERA5 data is used but it is unclear why? Please be more precise about the data.
*RESPONSE: Thank you for pointing this out. We corrected this paragraph with correct data usage. We use the 3d var analysis data to get the locations of convective 1 by 1 degree boxes from observations. The location of these convective events is then used to get diurnal cycle data from operational gridded data from which the diurnal cycle mean is calculated. The 3d var data is calculated on a 0.25 by 0.25 degree grid and then averaged into 1 by 1 degree data.
Thank you for pointing out the ERA5 discrepancy. In an earlier iteration of the paper we used both operational and ERA5 model data, but since the difference was not big we decided to use the operational data and left the ERA5 data in by mistake. We have corrected this error now.*

3. The section “model analysis” that contains the main results to assess the impact of the dropsondes is well-written and good to follow. The authors identify one particular flight on Aug 18, 2019 providing increased departures which is very interesting. However, it remains unclear whether this is a special case in terms of the observed profiles (stronger wind, more humidity or warmer?) compared to the other flights and also whether the observed pre-tropical storm environment differs from other flights.
*RESPONSE: Thank you for the suggestion. We expand this section with more information about how this special case differs from other cases. Please see below (*L125 comment) on how we chose the special case.*

I would like to suggest that the authors enhance this discussion. Difference maps between the analyses of both experiments (or analysis and first guess) could help to understand how the information contained in the dropsondes was spread in the domain. Additionally, the largest departures could be illustrated with respect to the synoptic situation. Did the assimilation also improve the later development of the tropical storm?
RESPONSE: Looking at the influence of the data assimilation on forecast of the tropical storm is beyond the scope of this paper, but excellent suggestion for future work!

*L118ff: This discussion would profit from showing how many observations were made in each box. How many flights contributed to the result in Fig. 2? See also comment on L18.
RESPONSE: Thank you for the suggestion. We enhance figure 2 with frequency of observations. We also list in the introduction how many flights covered each box.

*L125: I guess "statistically significant" means that in the other boxes, departures from more than one flight are averaged which might have led to reduced departures?
RESPONSE: At that location in the paper we refer to the following figure.



This is a figure 4 equivalent, averages of departures for each individual flight, black YDPS and red for NDPS. The outlier red line obvious in 'u' and 'v' is the NDPS experiment for the research flight in the B3 box. This shows that the B3 box, the special case in the paper, is statistically significantly different from the other flights in the wind field.

*L128f: "Lack of… " I do not understand this sentence. What does this study tell about a negative impact on the prediction? Does that mean that other obs types are not able to constrain the analysis correctly?
RESPONSE: Thank you for pointing this out, we endeavor to make the explanation more clear. We infer that the prediction might suffer in such a case as this because, in absence of a OTREC field campaign, the initial condition would predict a storm of different characteristics. We can see how the sentence is confusing and reworded it. Also, yes, the NDPS experiment, which uses other observations to constrain the initial

conditions, does imply that those other types are not able to constrain the analysis correctly when compared to dropsondes.

*L131: “This suggests…” If so, are there other examples or might that be just very case sensitive? This also affects the next argument on the sensitivity to the boxes. Was this flight designed differently or focusing at a different met. situation? Was it the only pre-storm flight? Was this case unique, e.g., in terms of wind speeds? See also comment on L20ff.
RESPONSE: You could be right. The special case might reflect different conditions as the mission there was targeting a pre-tropical storm disturbance environment. Here we plot the average values of the basic variables for comparison. As we explain above, this case was statistically significantly different from other departure cases. We do discuss this in more detail as you suggest above.

*L143: I do not understand this sentence. What is the relative error?
*RESPONSE: Relative error is defined as |(X-Y)/X|*100%, where X is the observed value, and Y is the model value.*

*L156: What does “issues” mean? Were large FG departures leading to a withdrawal of the data? Is the used data rejected during the data assimilation process?
RESPONSE: We rewrote the sentence to be more clear. Actually no, the YDPS experiment did assimilate data so that is proof that the data was not withdrawn or rejected. The small departure in the YDPS experiment for the special case shows that the data had enhanced weight during the assimilation.

*L169: Please explain more carefully how this is done as it remained unclear what kind of data is used. Please specify “point data to a regular grid”. Is this observation space or model space data? Is this related to the methods briefly introduced in the first paragraph of section 2? I guess this method uses observation data and the two analyses from the experiments? A bit more information is needed for readers that are not aware of results shown in Fuchs-Stone et al. (2021). What if the results were compared with gridded model space data? Is it possible to derive reliable vorticity data from the relatively coarse observation grid?
RESPONSE: Thank you your suggestions. We rewrite “point data to a regular grid” to “linearly interpolating the point data to a regular grid”. We do this both on the dropsonde data and on the model blacklisted data, i.e. we treat them as model “dropsondes”. This is not related to the 3d var analysis described in section 2. Since the ECMWF model has parameterizations that influence the divergence and therefore mass flux profile calculations we found it was not possible to compare the 3dvar divergence and vorticity directly. Therefore we resort to estimating the divergence and vorticity that would be ostensibly assimilated in the model.

Since the paper Fuchs-Stone et al. (2021) is open source we do not see a problem in leaving the reader to check Table 1 of that manuscript.

4. The section on cloudiness effects is entirely convincing to me, primarily as I missed a hypothesis about what is expected and why this is done. I do not get why x* should be higher in clouds especially for winds. The presented analysis doesn't tell anything about whether a cloud also exists in the model and does not consider the vertical extent of the clouds and cloud layering. Why did the authors not consider a point by point analysis and use something like a dew point depression or RH as an indicator for clouds? Could it be that highest departures occur where no cloud was simulated but observed or vice versa?
RESPONSE: It is not that x* should be higher in clouds, it IS higher in clouds. That is, that correlation is observed, not inferred. It is not the point of this paper to check if there is a could in the model, and also the goal is not to check for the vertical extent of the cloud. We elaborate in the revised manuscript what the goal was; since the ECMWF data assimilation system uses global irradiance values, i.e. satellite data, for assimilation, it seems natural to use satellite data as a measure of presence or absence of clouds. Presence of clouds will clearly affect the assimilation of irradiance data. We have failed to make this clear in the manuscript so we enhance the discussion in this section of the paper to justify our approach.

*L223: I do not understand this sentence as I thought the difference between the experiments is only the information contained in the dropsondes (plus a potential cycling effect that should also be discussed in the final section)? Are there other obs types that do not perform well? What types of wind observations do exist in clouds?
RESPONSE: Thank you for pointing out that this sentence is unclear. We expand the manuscript to be more clear. Indeed, the assimilation system does not only use dropsondes, but in both cases there is other data assimilated. It is beyond the scope of this paper to study details of which other types of data affect the assimilation.

*L232: The Instability Index seems also to show some overestimation in both experiments. Although this is also the case for DCIN it is stated that the convection parameters are well-represented. Later departures are considered "small" and an "excellent agreement" is seen. What is the underlying accuracy requirement for these statements? I guess a 1 g kg^{-1} or a 1 K might strongly impact a process like convection.
RESPONSE: Thank you for pointing this out. High values if Instability Index, and negative values of Instability index are not as important as the middle range; the most important physics of tropical convection happens in that range. We expand our discussion of the impact of the spread of the scatter between observed and modeled values. That is correct, 1 g/kg and 1 K can impact convection significantly as shown in Raymond and Sessions (2007).

5. I asked myself what section 5 has to do with the topic of this paper? What is the reason for adding an analysis of the diurnal variability of these parameters in ERA5. It would be more interesting to know to what extent the assimilation of dropsondes contributed to the diurnal cycle representation in the forecasts, e.g., when using the 12 UTC FCs over 24 h and compare it to the analyses.
RESPONSE: As noted above, I erroneously left "ERA5" where "operational analysis" is supposed to go. We correct that in the manuscript. We do not go into the proposed analysis because of the nature of our experiments. Since we ran a two month cycle instead of running the model 22 times at the moment of each research flight, there is stochastic differences occurring which we would not be able to distinguish from dropsonde influences. Therefore we look at average properties by constructing the diurnal cycle. There is sufficient discrepancy between the observations and model performance in the diurnal cycle to suggest further work necessary to address such questions.

*L236: Why is ERA5 data used here instead of the experiments? Please clarify how observations and simulations are combined and whether this 3D var method is applied to the ERA5 gridded data to calculate a time series of convective parameters.
RESPONSE: Thank you for pointing this out. We clarify this in the manuscript. We do use the operational analysis and not ERA5, as noted in a previous response.

*L237: The separation of non-convective vs. convective regions in the domain is done only for the dropsonde releases, right? It would be good to give an overview about when the flights are conducted and dropsondes are released in the overview section. Does the difference in release time lead to an average moisture convergence below 2 kW m^{-2} as shown in the time series? How many flights or individual days did contribute to this analysis?
RESPONSE: The 3d var analysis was done on the days of dropsonde releases, correct. We assume that the differing times of releasing dropsondes minimally influences/distorts the analyzed fields because lawnmower patterns done during the flights were designed to minimize those effects. The moisture convergence should not be affected drastically by the different dropsonde release times. There were 22 individual flights on 22 separate days.

*L260: The decomposition is interesting but to understand the inter-box differences it would be good to know how many days are investigated here.
RESPONSE: In line 244 we list how many cases are used for calculating each average. We do not find it necessary to distinguish the number of days over which the average was taken because the flights were performed at the same time, 12-18UTC each day.

6. The **summary and conclusion** section misses a discussion of the results in the context of previous literature and the aims of this study. Although the study addresses the IFS model performance by comparison of model results with observational data, I wonder why the authors submitted the paper to GMD. Therefore, I suggest that the authors comment on how an NWP center can profit from these results. I would also encourage the authors to discuss their results in the context of the quality of tropical winds.
RESPONSE: Since we discussed the results in the context of each section of the paper, we summarized the discussions in this section. We expand the summary though, to include the main discussion points from each section.
We chose GMD for the manuscript because the paper highlights performance issues of the ECMWF model which the modelers can further explore in development of the model in the East Pacific region. Since there has not been an observational study in the East Pacific region since the field campaign EPIC in 2001. This study offers unprecedented amount of data in the region used in the study.

Minor:

*Title: I suggest using “ECMWF“ instead of “EC”
RESPONSE: Thank you for the suggestion. Suggestion was adopted in title.

*L5 and L59: Please check the abbreviation “EC” as typically ECMWF is used.
RESPONSE: Thank you for the correction.

*L9: Please specify “precursor”
RESPONSE: Thank you for the correction.

*L16-17 and elsewhere: correct citation brackets
RESPONSE: Thank you for the correction. We corrected the brackets throughout the paper!

*L29: reanalyses?
RESPONSE: Thank you for the correction.

*L37: Please specify “affected”
RESPONSE: Thank you for the correction.

*L105: What is the top pressure? The uppermost model level?
RESPONSE: The top pressure we used here is 200 hPa.

*L109: Please find a different and more specific title.
RESPONSE: Thank you for the suggestion.

*L113 i.e. 700 hPa?
RESPONSE: Thank you for the correction.

*L121: “all” seems not to be correct.
RESPONSE: Thank you for the correction.

*L123: Box 3?
RESPONSE: Correct, as noted in the next sentence.

*Fig.4: At what vertical resolution is this analysis performed and what is the data availability? Use Theta symbol for potential temperature instead of t.
RESPONSE: We perform it on standard levels even though data is available on more pressure levels. Since the pressure levels do not coincide between different locations we average data on standard levels and perform the analysis there. We changed t to theta in the figures, thank you.

*L230: Please specify “considering the previous section”
RESPONSE: Thank you. Expanded.

*Fig.8 and Fig.9 would profit from a fit to the data (Fig.9) and some statistical numbers
RESPONSE: Thank you for the suggestion. We added correlation coefficients and other measures of fit.

*L272: “evaluates the performance” isn’t it rather an evaluation of the impact of sondes as stated in the title.
RESPONSE: Thank you for the correction.

*L282: “variable departures” is ambiguous
RESPONSE: Thank you for the correction.

*L284: Please rephrase “benefit by … smaller departures”
RESPONSE: Thank you for the correction.

*L293: Change to “more scatter, i.e. more variable departures”
RESPONSE: Thank you for the correction.

*L297: please mention that this was done using ERA5 data
RESPONSE: Thank you for the correction. We corrected that operational analysis was used.

*L307: “whether improvements could be made”
RESPONSE: Thank you for the correction.