Response to comments of DPhil examination report

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This document presents a point-by-point response to the comments from the examiners, which are shown in black. My replies are shown in blue and the appropriate changes to the text in the thesis are shown in purple wherever necessary.

1. Introduction and Background

The general problem with the Introduction and Background chapters is that the paragraphs and sections are disconnected, leaving the reader to determine the connection. Chapter 2 lacks a sufficiently detailed description of the global monsoon system and how it differs over America (physical mechanisms), particularly for the South American Monsoon System (SAMS), which is also the focus of this work. In particular, lacking and equally important is a discussion of the orographic effect involved in all monsoon systems (mechanical versus thermodynamical drivers) that will prove vital for the North American Monsoon System (NAMS) in Chap. 6. There is also no motivation and explanation of how the QBO interacts with the troposphere in the tropics (again,an explanation of the physical mechanisms is missing).

The comments of these chapters were addressed primarily by the following:

- Addition of sentences to link paragraphs and sections with each other.
- Details on the role of orography for the AMS.
- Details on the South Atlantic Convergence Zone, South American Low Level Jet and the mechanisms of ENSO teleconnections.
- A clearer discussion on why current global monsoon dynamic theories fail to explain the characteristics of the AMS.

In the revised version, several more paragraphs in section 2.2 now examine the South Atlantic Convergence Zone, the South American Low Level Jet and how these two features influence the mean SAMS and extreme precipitation events. Similarly, the role of the Sierra Madre Occidental (SMO) and the Andes cordillera for the NAMS and SAMS, respectively, is introduced and discussed. Undoubtedly, orography plays a big role for monsoons, however, recent reviews (Geen et al., 2019, Hill, 2019) discuss that orography are a reason why some theories proposed to explain the global monsoons break down for individual monsoons, rather than orography playing an identical role for all monsoon systems. For that reason, the roles of the Sierra Madre Occidental (SMO) and the Andes cordillera are discussed in a tailored manner for the NAMS and SAMS, respectively, rather than discuss a general impact of orography which would lead the discussion away from the main topics of the thesis.

Finally, Chapter 6 investigates mechanisms of the Mesoamerican Midsummer Drought and not the NAMS, in fact the study region of Ch. 6 is found about 2000 km south of the SMO and the core NAMS. Orography in Central America may very well play a role for rainfall variability in the region, but that was not a topic of research of the chapter. Therefore, this comment was addressed by more emphatically highlighting the differences between the NAMS and the MSD as other readers of the thesis may be similarly confused, as well as discussing the importance of mechanical forcing for tropical precipitation in the Discussion section of Chapter 6.

Pages 2-3: No references are given for the SAMS.

Several more references of the SAMS are given in Chapter 1.

Page 3: The statement "Nevertheless, the understanding of the effect of ENSO over South America and the AMS in general is still somewhat limited" is not true at all. There is a vast literature about the impact of ENSO on SAMS and NAMS that considers ENSO variability (diversity), which is not cited in the thesis.

The paragraph mentioned by the reviewer has been reworded entirely.

Page 3: The thesis begins with Chap. 4, consider rephrasing this. Done

Page 3: It is not clear if the focus of the thesis is on southern Mexico and Central America. The first line of the paragraph in question outlines the focus of the thesis and the rest of the paragraph has been reworded for clarity.

Page 9: The paragraphs are not connected. From Walker cell, the text jumps to the ITCZ and then monsoon systems. The part of this section that describes the Hadley and Walker circulations and the ITCZ is now more connected and linked to the monsoon phenomena later on.

Page 15-16: Only three small paragraphs explain the SAMS. They focus on the Amazon region. However, the SAMS extends from the Amazon to the southeast of Brazil and involves several other essential features, such as the South Atlantic Convergence Zone, the South American Low-Level Jet, the reversal of the circulation, etc.

Several paragraphs in this section now provide background on the SACZ/SALLJ and their relevance for the SAMS, and another paragraph details the role of the Andes cordillera and the SAMS and its link between the SACZ/SALLJ.

Page 16: The reference Bombardi and Carvalho (2011) is misplaced.

The reference has been removed.

Page 17: Although this study focuses on North and South American monsoon systems, explanations are only given to the NAMS.

More detail is now given for the SAMS than for the NAMS, as well as a final paragraph on section 2.2 regarding the challenges of current global monsoon theories to explain the NAMS and SAMS. Note that the Midsummer Drought is a feature of Mesoamerica and is not considered a part of the North American monsoon (Adams and Comrie, 1997, Pascale et al., 2019).

Page 21: Disconnect appears again: the text ends with MJO and then jumps to ENSO.

The separation between sections 2.3 and sections 2.4 is now provided by the following line at the end of section 2.3 :

In short, a plethora of hypotheses exist for the causes of the MSD, however, a key aspect of the climate of the AMS, is the effect of ENSO, which is the following section.

Page 21-24: ENSO section describes basics such as where the name comes from that are irrelevant to thisstudy and relevant aspects are left out,such as how ENSO impacts the SAMS. What are the physical mechanisms and the impacts in terms of precipitation? More importantly,references are missing here, particularly the teleconnection patterns that affect both the NAMS and SAMS, such as the Pacific North America (PNA) and Pacific South America (PSA) patterns.

A shorter introduction to the early studies on ENSO is now provided, and more detail on the PNA, PSA and the role of the Atlantic ITCZ migration for ENSO impacts is given in this section.

Page 23: ENSO and South America are first mentioned, then the next paragraph describesENSO diversity alone and then goes back to ENSO and South America. ENSO diversity is relevant for both.

This sequence of paragraphs have been reworded to make more clear how ENSO diversity is relevant for South American precipitation variability.

Page 24: This sentence is incomplete.

I couldn't tell which sentence this comment refers to precisely. However, the wording concerning how the QBO is observed I found to be awkward so I rephrased this sentence hoping this was the sentence the examiners refer to.

Page 25: "Wester" should be "Westerly". Done.

Page 25-29: The QBO section does not explain the cause-effect relationship between the QBO phase and temperature and the troposphere in the tropics. The easterly phase of the QBO (QBOE) is associated with a higher and colder tropopause in the tropics. In contrast, the westerly phase (QBOW) is observed with the lower and warmer tropical tropopause. Are the QBO phases independent of the tropopause temperature variability?

My understanding of this comment suggests that section 2.5.2 was not clear enough to indicate that this section presents a review of the mechanisms through which the QBO could be linked to tropical convection.

First, the following line was added at the end of section 2.5.1:

The following section details the observational and modelling evidence that links the QBO to the tropical troposphere, as well as discusses the existing hypotheses that explain how the QBO could impact surface climate in the tropics.

Secondly, three mechanisms are detailed in section 2.5.2, vertical wind shear, tropopause temperatures and tropical circulation feedbacks. This description is now made more evident by the following line at the end of section 2.5.2:

In short, multiple lines of evidence suggest relationships between the QBO and tropical convection, and thereby the QBO could play a role in tropical surface climate variability. The leading hypothesis suggests that the modulation of the UTLS temperature structure influences the upper-level static stability to the extent that the QBO can influence the strength of ascent and vertical transport of moist static energy. The effect of the QBO on vertical shear and feebacks with the tropical overturning circulation have also been suggested as possible explanations for the observed responses to the QBO phase.

In addition, multiple lines were added to section 2.5.2 to more emphatically indicate that those paragraphs are describing the cause-effect links that have been suggested by the literature. Note the discussion on Figure 2.6 for example. Finally and to answer the last question in this comment, the QBO phases are the result of wave-mean flow interactions in the stratosphere (Baldwin et al., 2001, Match and Fueglistaler, 2019). The tropopause temperature variability has little effect on the QBO; however, the mean state of the tropopause temperature has been shown to determine the lower boundary of the QBO (Match and Fueglistaler, 2019).

Page 28: "The authors argue are" should be revised. Done.

2. Chapter 3: Methods and data

This chapter presents the same general problem as previous chapters, i.e., disconnected sections and a lack of detail, e.g. discussing uncertainties of the different data sets. There is no explanation of how data and methods will be used to achieve the study's objectives.

The main revisions to this chapter consist of:

- More detail on uncertainties in the reanalysis and the gridded precipitation datasets.
- Explicit connections between the datasets and methods described in this chapter and the result chapters.

Pages 30-38: Is the vertical resolution of the data sets and model simulations suitable for representing the QBO, tropical stratosphere and troposphere? The text mentions that the Indian monsoon is not well simulated, but is there any evaluation of the performance of the CMIP6 simulations for the SAMS and NAMS?

The text now explicitly mentions how ERA5 and the MOHC models have a suitable vertical resolution for representing the QBO and indeed are amongst the best reanalysis/models at representing the QBO. There were no evaluations of the CMIP6 simulations for the SAMS and NAMS at the time of writing the thesis to my knowledge, except for that of Chapter 4, found in García-Franco et al. (2020), which was partly the value of that contribution.

Table 3.2 is confusing as many simulations are not explained nor further used in the thesis.

All the simulations in the Table are used (see Figure 6.7). However, it is true that three experiments are substantially more important for the thesis in general. For that reason, the table now only indicates the piControl, historical and AMIP experiments, all of which are described in more detail in the text.

Page 38-40: Explanation and interpretation of the moist static energy budget terms missing.

More detail on the use of the MSE budget, its use for the MSD chapter, and how the budget helps in understanding precipitation variability is given.

3. Chapter 4

Chapter 4 is mostly of descriptive nature and, as such, lacks a more informed discussion and interpretation of the results considering the physical mechanisms involved in the various aspects of the monsoon systems analysed here, such as the Atlantic and Pacific ITCZ, precipitation and convection and ENSO teleconnections.

Corrections in this chapter provide more detail on:

- Mechanisms of ITCZ migration.
- Teleconnections pathways of ENSO impacts to South America.

Revisions were made to improve the figures/wording as suggested by the examiners.

Page 44: "near-air surface temperature" should be "near-surface air temperature". Done.

Page 42: The SACZ and SALLJ are mentioned for the first time here, but their mechanisms are not described. This would help pinpoint the models' deficiencies in simulating these features. The background chapter (2.2) now describes the SACZ, the SALLJ and their mechanisms and influence on precipitation for South America.

Table 4.1: This table is of very limited use with a large number of scores. A clear interpretation would help the reader to filter the relevant information. It is also not clear for which region these scores were computed. Units are missing.

The table has been removed as it is of very limited use.

Page 48: An explanation needs to be given as to how the biases are computed for the piControl simulations because for the historical period, they are obtained by subtracting the observed (ERA5) climatology for the same period

The pre-industrial control period used was 1850-2350 and the observed period is 1979-2014, a comparison of the climate of these two periods is indeed not the approach to validate a model as inconsistent forcing could be relevant for these comparisons. The point of comparing the pre-industrial control run with observations after describing the biases in the historical experiments is precisely to show that forcing has little to do with the biases seen in the historical experiment. The fact that low-level wind differences and some temperature differences with respect to observations are very similar in the piControl experiments to the historical biases, suggest that the biases are due to model deficiencies and are less influenced by the historical forcing. In other words, the magnitude of some of these biases is much larger than the impact of forcing; illustrating this point is the aim of these figures. More carefully worded language around the piControl-reanalysis differences is now used to avoid the confusion between the biases with the historical experiments.

Page 51: An explanation needs to be given as to the causes of the seasonal cycle of the ITCZ.

A few lines now introduce the reader to the causes of the seasonal cycle of the ITCZ (solar insolation, SSTs and SST gradients and low-level wind flow).

The definition of the SACZ is only given here. The detection method is briefly described in the Caption of Figure 4.6 but should be described in Chapter 3 (Data and Methods). There is no explanation of the importance of the SACZ for the SAMS. The following work should be cited.

The citations have been added in Chapters 2 and 4, and the method to define the SACZ is now given in 4.2, in the methods section of Chapter 4. The importance of the SACZ is emphasized now in Chapter 2.

Figure 4.9 has too many lines that are difficult to distinguish –reduce to the essential.

Two lines were removed from this Figure.

Page 59: The interpretation of the result described penultimate paragraph, "the GC3 N216-pi shows a better agreement with observations but still underestimates austral summer rainfall by 1 mm/day" needs clarification since "pi" simulations refer to the pre-industrial period. Is it correct to compare those to observations?

The pre-industrial control experiments are not the best experiments to compare to observations, however, highlighting the differences between two resolutions of the piControl experiments is relevant because it points to the role of horizontal resolution. This statement was meant to convey such a message. At the time of writing García-Franco et al. (2020), historical experiments for the medium-resolution version were not available. I have reworded that line to provide more clarity on these comparisons for the reader.

Page 62: ENSO teleconnections are evaluated by other fields, generally in the upper troposphere (divergent wind, streamfunction or geopotential height anomalies).Temperature and precipitation are the impacts of ENSO and not its teleconnection patterns. Also, since the mature ENSO season is DJF (as shown in Fig 4.11 and 4.12), its links to the monsoon season are unclear.

The word *teleconnection* has been replaced by impact where appropriate.

Page 63-64: Cold anomalies are not ENSO teleconnections.

As before, the word *teleconnection* has been replaced by impact where appropriate.

Page 65: Another reference should be used to explain the Pacific North American (PNA)and the North Atlantic Oscillation (NAO) is irrelevant for the tropical North Atlantic (Hastenrath 2006, Taschetto et al. 2016). NAO/ENSO interaction is relevant for extra-tropical Europe. Giannini et al. (2000, 2004) do not evaluate the interaction of NAO and ENSO. So, these citations are misplaced.

A different wording of this line and references are now provided highlighting the importance of simulating the ENSO-PNA connection for reproducing ENSO impacts to Mexico and the Caribbean.

Page 65: This is also not the case. 1) SESA is out of the area of study; 2) The ENSO influence on SESA is made via changes in the South American Low-level Jet (SALLJ) and less so via the subtropical jet; 3) This is not related to the SACZ and SPCZ. ENSO teleconnection to South America is weak during the mature phase of ENSO because local processes and remote intraseasonal phenomena such as MJO dominate.

The impact to SESA is now described as the result of the influence of subsidence on the SALLJ and the associated moisture transport (Montini et al., 2019). The discussion on SESA is short as it is outside of the study region.

Page 66 and Figure 4.13 Caption: Description of left and right panels but not the middle, upper and lower ones.

The Figure caption has been corrected.

Page 66: The explanation given is correct but does not link to the precipitation anomalies. It explains the shift in the Walker cell and ascending branch but does not mention the descent branch over the Amazon that generates the negative anomalies of precipitation. ENSO affect the Amazon in two different ways, triggering Kelvin waves (Matsuno-Gill type of response) leading to the changes in Walker cell and triggering Rossby wave trains to the tropical North Atlantic (which is the PNA). The mechanism associated with the latter is reducing SLP over the tropical North Atlantic, which weakens the trades and evaporative cooling, leading to a warming of the tropical North Atlantic. The ITCZ does not migrate southwards, leading to negative anomalies of precipitation. This whole process is not explained anywhere in the text.

This process is now introduced in the background chapter (Section 2.3), and teleconnection pathways to South America are now discussed in more detail in this Chapter and linked to the precipitation anomalies. The links to the SLP in the tropical North Atlantic are explicitly linked to the anomalies of precipitation through the mechanism mentioned by the examiner.

Figure 4.14.: The quality of the figure is poor and needs to be improved. It is unclear what information can be obtained from the figure. In particular, the non-linearity discussed is difficult/impossible to see.

Apologies, I find the suggestion that the figure is of poor quality confusing, as all the labels, axis and legend were clearly visible. My understanding is that the examiners feel the Figure is not clear enough, and for that reason the new version of the Figure shows fewer simulations. More clarity in the text is also given as to where can the non-linearity be seen in the model simulations and the observations.

Page 68: Part of the methodology for calculating EP and CP ENSO is in the Figure's caption. It should be in the methods.

The method section of the chapter (4.2), now describes the methodology to separate ENSO phases.

Pages 70-73: Is there an effect of ENSO on QBO? If so, how the cause/effect can be untangled? The text mentions that the precipitation in the western Pacific is relatively similar during QBOe and QBOw, but not in other parts and conclude that the teleconnection is affected by the QBO. How does that work? Because the teleconnections are triggered by anomalous convection in the western Pacific (precipitation), which is similar in both cases. The differences between QBOe and QBOw in Fig.18 (Page 72) seem very weak in the troposphere and at the surface. Moreover, could the differences in precipitation be just a result of randomly grouping different ENSO events? Mainly because there are no consistent changes for El Niños.

Chapter 7 more directly addresses the posed by the examiners in this comment. The purpose of section 4.6.3 is simply to provide a first piece of evidence that suggests a possible role for the QBO, which is part of the motivation for chapter 7. A concise reply to this comment follows.

There are observational and modelling studies that indicate an effect of ENSO on the descent rates and amplitude of the QBO, however, models struggle to reproduce such relationships and indeed there are no such effects in the HadGEM3 model (Serva et al., 2020). Suggestions of a QBO modulation of the Walker circulation have been made (Liess and Geller, 2012, Hitchman et al., 2021), and indeed this is confirmed in Chapter 7, which could explain the results in this figure by also providing evidence that the streamfunction is significantly affected in the lower troposphere of the Pacific and Indian Oceans.

The vertical velocity and zonal wind anomalies near the surface are weak, yet the precipitation differences are within the same order of magnitude as the ENSO impacts. The arguments presented by the examiners posing the likelihood of chance or the mixing of ENSO events are good points, which is why these possibilities are examined at length in Chapter 7. Note that there is a possibility of a local effect of the QBO, which would modulate the strength of ENSO impacts (perhaps what we see in Fig 4.17), but also the possibility of a (relatively modest) modulation of the large-scale circulation by the QBO (Hitchman et al., 2021) which could in principle affect the strength of convection in the west Pacific and/or modify how ENSO teleconnections propagate.

Page 74 and Figure 4.19: It is not called Bolivian Low-level Jet. It is the South American Low-level Jet (SALLJ).Vera et al.(2006) and Marengo et al.(2012) do use the same terminology andall relevant literature. Labels a) and b) are missing.

The Figure has been revised to use the SALLJ terminology.

Page 77: The mentioning of the land-use change and soil-atmosphere feedbacks are not supported by the analysis and not discussed in sufficient detail.

This sentence was removed.

Chapter 5: A wavelet transform method to determine monsoon onset and retreat

The equations for the Haar wavelet in 5.1 and in the last paragraph on page 83 contradict each other for the singularity t=b.

Done.

Page 89: Figure 5.5 should be Figure 5.5d. Done.

Page 91: The text should mention the sensibility of the methodology to averaging the precipitation over different boxes.

A line in this page now mentions that the performance of the WT method relative to other methodologies has little sensitivity, however, the area chosen will definitely influence the mean values of onset and retreat found by the WT method (as well as for the other methods).

Page 93: The strength of the proposed methodology is that it finds the onset and retreat pentads more consistently across different datasets when compared to the other methods.

This line was reworded to reflect the comment of the reviewers.

Page 94: "The impact of monsoon onset in precipitation is diagnosed to be slightly stronger by A12 compared to WT or G13." Is this because the A12 methodology uses two different thresholds (described on page 90)?

The result using the A12 methodology is surprising because their thresholds frequently lead to false-hits or years where onset/retreat is not defined because the criteria is not met. However, the sligthly stronger precipitation differences could be due to the fact that their parameters are stricter, i.e., their threshold is higher than the G13 threshold and the persistence parameter is also longer, which means that onset/retreat are defined more strictly.

Page 95: The text should explain why the methodology is not applied for the SAMS, which is the focus of the previous Chapter, and instead is applied to the Indian Monsoon.

This explanation is now given in the introduction to the chapter, section 5.1.

Page 98: It would have been better to show the differences in relation to ERA5.

Interesting suggestion, however, the ERA5 results were already shown in the previous figure. The way this figure is presented, the effect of horizontal resolution is quite evident and interesting to understand how the model represents stronger precipitation changes associated with onset/retreat in the medium-resolution configuration. I have chosen to keep the two figures as they were initially for the above reasons because the purpose of these figures were to show the performance of the method across models of various resolutions and not a model-to-observation comparison.

Page 100: What does "... the MSD is part of a regional-scale process on the result of local-scale processes." mean?

This sentence was reworded for clarity.

Page 109: It should be mentioned in the conclusions the potential of the methodology to be used for other purposes, such as for shorter extreme events.

Done.

Chapter 6

Page 115: The text should clarify if "in which the SSTs are 4 K warmer and colder" means everywhere, global oceans. The CFMIP experiments need to be described and discussed.

Done.

Page 115: More details are necessary about the AMIP and CMIP simulations.

Done.

Page 117, section 6.3: Please discuss the interannual variability of the climatology.

Done.

Page 120: An explanation needs to be given as to why the increase in resolution does not translate into a better simulation of precipitation.

Done.

Page 124: Colour bar labels in Figure 6.10 should be given at the same intervals for negative and positive anomalies.

The labels are set at the same intervals; the labels are placed at different points relative to the 0 anomaly mark, yet the intervals are exactly the same and the color bar spacing is symmetrical.

Page 130, section 6.5: grammatical errors in several sentences. Done.

Page 132, near the end: grammatical error

Done.

Page 136, near the top: grammatical error

Done.

Page 136, near the end: how is LW treated in the models? How does suppression of the coupling work? A critical discussion is needed here.

Done.

Page 145: It would be helpful to provide an interpretation or discussion of these results.

This discussion is given at the end of section 6.7.

Figure 6.29: How are the error bars computed?

The error bars indicate the standard deviation for each month/phase.

Page 151/152: the discussion needs to be made clearer. E.g., it is unclear which evidence suggests that LW heating and CRE and moisture transport may be more important than surface fluxes or the SW heating variations.

Done.

Pages 152-155: A paragraph should be added to the Summary and Discussion Section discussing the results obtained here in the context of recently published work by Boos & Pascale (2021). The latter find that mechanical forcing generated when Mexico's Sierra Madre mountains deflect the extratropical jet stream towards the Equator is the primary driver of convective rainfall for the core North American monsoon and not the thermally forced tropical monsoon. This thesis finds that the most likely mechanism that drives the Midsummer Drought in southern Mexico, Central America and the Caribbean is the transport of moisture by the Caribbean Low-Level Jet, with the thermodynamic aspects being of lesser importance.

The work by Boos and Pascale (2021) analyses the role of orography for the location of the core North

American monsoon precipitation and seems to reconcile why so-called ”established monsoon dynamic” theories cannot explain the band of rainfall on the western coast of Mexico. The study and the examiners use the term ”Sierra Madre” which is ill defined. The Sierra Madre Occidental (SMO) is notably different from la Sierra Madre Oriental, la Sierra Madre de Chiapas, la Sierra Madre de Oaxaca and la Sierra Madre del Sur, so that the term ”Sierra Madre” means very little and has been misused. While this may seem like a harmless mistake about Mexican geography, there are significant scientific consequences of confounding the term ”Sierra Madre”. First, for the cited work the misuse of ”Sierra Madre” is relevant because their simulations do not only locally remove orography in the SMO, but also in the Sierra Madre Oriental, the Faja Volcánica Transmexicana and la Sierra Madre del Sur, which in total span an area greater than the entire UK. The evidence for the argument of Boos and Pascale (2021), which suggests that precipitation in the NAMS is ”due to local orography” may be insufficient, as they have removed a significant amount of orography more than just the SMO, which is the core NAMS. Therefore, disregarding the other cordilleras in Mexico through the use of the term ”Sierra Madre” is not just geographically wrong but covers up a scientifically questionable part of their simulations and leads to conclusions that may not be accurate.

More precisely to the examiners point, the cited work is only tangentially relevant for Chapter 6, which uses a study region 2000 km away from the SMO. For that reason, I have made more evident how the NAMS is separate from the MSD, not just geographically but also in terms of physical mechanisms. Nevertheless, the examiner’s main point, which is to suggest a discussion on dynamic versus radiative mechanisms in the thesis is highly relevant and is directly related to the work of Boos and Pascale (2021) which contradicts the conventional thermal mechanisms proposing instead a big role for mechanical forcing. For the purpose of such a discussion suggested by the examiners, however, the study of Fu et al. (2021), amongst other references of studies of the MSD region, are more relevant, so a new paragraph in the discussion section of the chapter discusses radiative versus dynamical mechanisms. The work of Boos and Pascale (2021) is used to describe the role of orography in Chapter 2.

Section 6.8: The use of the phrase “interannual variability” is wrong – interannual variability described the variations from one year to the next. However, here it is used to talk about intraannual (or seasonal) variability.

This is not wrong. Interannual variability was analysed for ERA5, this was analysed in Figures 6.11, 6.12, 6.22 and 6.28, which use the definition presented by the examiners by investigating what variables are linked to variations of the MSD from one year to the next. This is made more clear for Figure 6.11 but was also clear in Figure 6.28a.

Chapter 7

Chapter 7 lacks a more informed discussion of the results regarding the physical mechanisms by which QBO can affect the tropical and extra-tropical troposphere and monsoon.The methodology used (composite and regression analyses) is not appropriate to investigate the cause-effect between QBO, ENSO and monsoon variability. This is tried in Chapter 8 with nudged experiments. Here, the suggestion is to concatenate Chapters 7 and 8, selecting only the relevant figures to describe and discuss the main findings concisely.

This chapter has been entirely re-written to address the examiner's remarks. For the first part of analysis using CMIP6 experiments, only results from the medium-resolution are given to provide a more concise view of the results and the selection of figures throughout was more strict to present the findings concisely.

Note also that there is no text in the color blue for tracked changes because the whole chapter has undergone revisions.

Page 158: An explanation needs to be given as to why use the pre-industrial control experiments(before 1850) and compare to the observations GPCP extending from 1979 to 2018.

Pre-industrial control experiments are ideal to investigate the internal variability and teleconnections of a GCM without the effect of external forcing. A clearer reasoning for using these experiments as well as several examples of studies that have used this type of runs to examine variability within a GCM are now given in that part of the chapter.

Pages 158-159: An explanation needs to be given about the caveats of using the chosen methodology for defining ENSO events. Using the EN3.4 index with a threshold of ± 0.65 to determine positive or negative events is appropriate for the boreal winter, during the mature phase of the ENSO events. However, when the requirement of 5 consecutive months above the thresholds is lifted, this methodology can lead to the erroneous selection of ENSO events compromising the results and interpretations. For instance, if E3.4 is above 0.65 only in August, this month is included in the composites, but cannot be considered an El Niño event.

A 5-month running-mean index was now used for all the results, without significantly changing the Figures and results of the Chapter. This new methodology now avoids the possibility of the false-hit cases suggested by the examiners.

Page 159: An explanation needs to be given as to how the cause-effect between QBO, ENSO and monsoon are untangled using solely composite and regression analyses. In my opinion, the only way to do that is by performing idealised model experiments forced by QBO like variability (without SST variability) and analysing the precipitation response. This is the objective of Chapter 8 and should be presented together concisely.

A review of the proposed mechanisms for the interaction of the QBO, ENSO and tropical convection is now given in Chapter 2, as well as in the introduction of the chapter. In addition, the robustness of the relationships found is a valuable contribution in itself because relatively little evidence exists that there is a relationship between the QBO and tropical convection in a GCM.

As suggested by the examiners, the two chapters are presented together.

Figure 7.1 and following: The colour bars are too small to recognise the values of specific colours.

Please improve.

The labels and extent of the colours are now larger.

Pages 160-161, Figures 7.2 and 73: An explanation needs to be given as to why the model results in DJF & MAM have the opposite response in the equatorial Atlantic and Indian oceans.In addition, difference plots including significance between GPCP and the models would be useful.

If I understood correctly, this comment refers to the zonally asymmetric impacts, i.e., precipitation anomalies of different signs at different longitudes (Atlantic versus Indian Oceans). These features are now linked to the hypothesis of a Walker circulation modulation by the QBO in the text.

Page 162: "Caribbean Sea anomalies are likely related to the northward shift of the Atlantic ITCZ observed in the same season particularly in UKESM-pi", but not in the observations.

That sentence is now removed in the new version of the Chapter.

Page 162: If QBO affects the troposphere (and not the other way around), why would we only have a significant response overland? It seems that SST affects the troposphere and then QBO.

These sentences have been reworded. There is no implication that only over land there should be a response. The leading mechanism in the literature suggests that the changes to the upper-level static stability induced by the QBO causes these relationships. However, this effect should be present over all regions with active deep convection, over land and over the ocean, however, impacts over the oceans are much stronger than over land in the simulations. The new version of the chapter provides evidence that ENSO SSTs do not modulate the QBO in the model, as found in previous studies (Serva et al., 2020). Furthermore, the evidence of zonally asymmetric impacts, as pointed out by the examiner, is now discussed further in the context of the static stability hypothesis in the discussion section of the chapter. Finally, the new version of the chapter now examines the possibility that ENSO SST variability influences the QBO, finding no evidence of an ENSO modulation of the QBO.

Pages 164-165, Figure 7.8: The biases are the same magnitude as the observed convective precipitation. This challenges the validity of the results.

The biases are large relative to the climatological values which limits the validity of the results as a 'true QBO response'. A paragraph in the method section now mentions that the GC3 N216-pi model has been top ranked amongst CMIP5/6 assessments of the Pacific and Atlantic ITCZ which means that these biases are found in state-of-the-art models. However, there is still value in finding a response in a model, because there is little evidence that the QBO has a robust effect over the simulation of the ITCZ in GCMs. The text now discusses how model biases in the ITCZ reduce confidence that the simulated response can be similar to the observed responses.

Pages 165: The reason for the lack of robustness of the ERA5 analysis does not seem to be the short span of the dataset (see GPCP results). There is a problem with comparing historical datasets (GPCP and ERA5) with pre-industrial simulations.

The chapter does not intend to assess the mean representation of the tropical climate of the simulations.

Rather, the chapter analyses variability, which is more readily examined in a long simulation that has no external forcing. The advantage of the length in these simulations is that (1) robust statistics can be computed, improving on a shortcoming of observational analyses, (2) there should be no random aliasing with ENSO and (3) decadal variability that may exist in the model is readily sampled and would not affect the results. For that reason, the QBO response in the pre-industrial simulations is statistically more robust and provides more information than an a short simulation with external forcing included. The text now more thoroughly examines what the value of piControl simulations in the method section, and for the Figure in question, a more informed discussion is given regarding the uncertainty in the results due to model biases.

Page 165, Figure 7.10: A meridional dipole of precipitation characterises a meridional shift of the ITCZ. Fig. 7.10a (ERA5)shows only a region of positive anomalies during boreal winter. The enhanced convection in DJF can be associated with ENSO. An El Niño event producesmore convection along the equatorial region because it increases the area of SST conducive todeep tropical convection. The description of meridional shifts of the ITCZ needs to be revised. Having a better understanding of the mechanisms involved in the meridional shift of ITCZ would have helped.

A more precise wording is used to distinguish the model response in the Atlantic during boreal spring, indicative of a northward shift of the ITCZ, and the stronger Pacific ITCZ response seen in models and ERA5.

Page 171: Caption of Figure 7.13 is wrong; descriptions of (c) and (d) are swapped.Page 172: An explanation needs to be given as to why use 36-year samples if the pre-industrial runs are longer.

Done.

Page 172: An explanation needs to be given as to why use 36-year samples if the pre-industrial runs are longer.

That figure is now a Table and the selection of 36-yr periods is to match the length of the observed period and the length of the nudging simulations. This method aims to investigate how frequently would a QBO W-E difference be found if the picontrol simulations were of the same length as observations. In other words, we evaluate the probability of occurrence of a QBO W-E difference found in a slice of the long pre-industrial control of similar length to the observed period, in this case in the probability of differences in the IOD/ENSO indices, which allows you to estimate how the robustness of the results.

Page 173: Regression analysis is not well explained, which gives no confidence in the conclusion that the QBO impacts tropical precipitation.

The use of the regression analysis is now explained in more detail. While one cannot conclude that QBO causally impacts tropical precipitation from this analysis, the regression does separate QBO and ENSO influences and this figure provides for evidence to the reader that there are robust differences in the spatial distribution of precipitation associated with the QBO that are independent of ENSO. The cause-effect relationships cannot be separated nonetheless.

Figure 7.14: What quantity is shown here?

Convective precipitation.

Pages175-176: The ENSO teleconnection involves the extratropical response (Rossby wave trains) such as the PNA for the ITCZ in the Atlantic; itis not just a direct walker cell response (Matsuno and Gill, Kelvin wave response).

This section focuses on QBO-Walker circulation relationships, not ENSO teleconnections, the text now makes that fact clearer. By analysing the Walker circulation in Neutral ENSO phases (see Figs. 7.12 and 7.13) there is no influence of ENSO teleconnection mechanisms.

Page 177, Figure 7.16: An explanation is needed about the physical mechanisms by which the QBO impacts the troposphere because the anomalies are stronger on the surface and not at the upper troposphere-lower stratosphere (UTLS), particularly for the run with higher vertical resolution.Also, the anomalies are less than 10% of the climatological values. This could be related to other phenomena, such as MJO.

Note that all the simulations have the same vertical resolution. Indeed this figure could be a result of the MJO, but this would necessarily have to point to the MJO-QBO connection (**?**), which is not found in this model (Kim et al., 2020). The colorbar for the anomalies is now exactly 10% of the colourbar for the climatologies, this means that regions where the anomalous streamfunction is coloured with the highest possible value is at least a 10% difference from the climatology.

Page 178: The ENSO Atlantic ITCZ response occurs in MAM (boreal spring) because of the ENSO phase-locking with the seasonal cycle of the Atlantic SST that drives the ITCZ meridional shifts.

This link between ENSO and the ITCZ has been noted in the text, indicating the Atlantic ITCZ shifts may be just be an aliasing of ENSO effects, but the fact that these responses remain under ENSO neutral, and the results from the regression analysis, make this possibility unlikely.

Page 180: An explanation for how the QBO teleconnections work needs to be given. In other words, how does the QBO change the surface state? How well do these pre-industrial runs simulate the current QBO-ENSO-Walker relationship/variability?

A more detailed discussion on mechanisms is given, as well as possible explanations for the observed relationships, uncertainties and how our results fit with those hypotheses. A paragraph in the method section now provides context on how well does the medium-resolution configuration simulate general aspects of tropical climate.

Page 180: The results are inconclusive about the cause/effect of QBO and the tropical troposphere.

A more detailed discussion on mechanisms is given. The value of the results is argued to be due to the fact that the evidence of the CMIP6 experiments robustly demonstrates a link between the QBO and tropical tropospheric climate. However, the cause-effect relationships require targeted model experiments, which is the next section of the chapter.

Chapter 8: Tropical Teleconnections of the QBO in theUM with a nudged stratosphere

The suggestion here is to concatenate this Chapter with the previous chapter selecting only the relevant figures to concisely describe and discuss the main findings to respond to the scientific question of the cause/effect QBO and the tropical troposphere.

The chapter was merged as suggested.

Page 182: The explanation of how the QBO can affect convection is given only here. This should also be discussed in previous chapters.

The explanation is now given in Chapter 2 and at the start of Chapter 7.

Page 183: Figure 8.1 x-axis labels wrong; it should be latitude instead of longitude. And Caption does not describe the panels.

Figure corrected.

Section 8.2: Include a detailed description of the nudging methodology, such as time relaxation parameters,possibly showing equation etc. Clarify the exact domains where the nudging works and what happens at the boundaries of these regions (tapering).

The nudging equation is given, as well as details on tapering and a more obvious description of the nudging region as well. And the nudging region is illustrated in Figure 7.14.

Figure 8.2: The different lines are not clear enough.

This figure has been removed.

Figure 8.3: Are panels b) and c) discussed anywhere? If not, remove it. Page 191: Parentheses missing in "(Figure 8.6". Pages 192 and 193, Figures 8.7 and 8.8: The differences between AMIP CTRL and AMIP nudged are not statistically significant.

This figure has been removed.

Page 191: Parentheses missing in "(Figure8.6".

This figure has been removed.

Pages 192 and 193, Figures 8.7and 8.8: The differences between AMIP CTRL and AMIP nudged are not statistically significant.

The wording is now more precise to reflect those regions where the difference is significant.

Page 197-201: Coupled experiments, results for SST and precipitation (Figures 8.13-8.18) all show a weak impact of the nudging on SST and precipitation. This means that QBO has little effect on SST.

The evidence in this figure shows that the nudged experiments have removed the SST-QBO relationship diagnosed earlier in the chapter and found in the control experiments. Since there seems to be little modulation by ENSO on the QBO descent rates and amplitude in the model, the discussion of the chapter now provides a more informed discussion on what these findings indicate regarding mechanisms and the direction of causality for tropospheric-stratospheric connections.

Page 204: Even though the nudged experiments improve the UTLS, the impact on convection and SST is negligible. Then the results of this session also show a weak effect of the QBO on the subtropical jets.

But it is argued that the problem is the nudging. Could it not be that the QBO does not affect the tropical troposphere? A more elaborate discussion and interpretation are needed.

A more elaborate discussion is provided.

Pages 208-210: This chapter says that nudging experiments could untangle the cause/effect relationship between QBO/ENSO/IOD,and the results for the coupled and uncoupled experiments do not resolve the problem. Then the conclusion is that nudging might not be appropriate. Or there is no effect of the QBO. The question is still open,and future directions to resolve this problem should be discussed.

Yes, there are several plausible explanations and our results were inconclusive. The final section now discusses the possibilities that (1) other types of nudging could be better suited, as we discuss, our nudging technique removed wave interactions, a zonal-mean nudging technique and not full 3D nudging might provide different results. (2) There is only an upward influence from the troposphere to the stratosphere in the model that we removed by nudging but this possibility was examined for ENSO and we found that this is not a key influence in the model QBO which is parametrised, (3) the main mechanism of interaction is not the static stability mechanism as frequently suggested by the literature (Hitchman et al., 2021) and which is the one the nudging experiments more directly examine.

Chapter 9: Conclusions

Pages 211-217: Overall, this work started by analysing SAMS and NAMS and links to ENSO QBO in climate models, then developed the wavelet transform methodology and applied it for the midsummer drought of southern Mexico and Central America. Then it analyses the influence of the QBO on the tropical troposphere. Our overall assessment is that when the work was more focused, such as in Chapters 5 and 6, the quality of the work/results is better. The QBO work and in particular the nudging seemed rushed and lacked the required attention to detail.

Noted.

Pages 211-217: The overall main contributions of the thesis need to be discussed, stating the significant advances in our knowledge on the American Monsoon System: variability and teleconnections. Suggestions for the next steps should also be addressed.

The contributions from each chapter and advances are given in the summary section, whereas an improved future work section now provides more detail on future steps.

Page 214: The grammar is wrong with an incomplete sentence (verb missing). Corrected.

References

Adams, D. K. and Comrie, A. C. (1997), 'The north American monsoon', *Bulletin of the American Meteorological Society* **78**(10), 2197–2214.

Baldwin, M., Gray, L., Dunkerton, T., Hamilton, K., Haynes, P., Randel, W., Holton, J., Alexander, M., Hirota, I., Horinouchi, T. et al. (2001), 'The quasi-biennial oscillation', *Reviews of Geophysics* **39**(2), 179–229.

Boos, W. and Pascale, S. (2021), 'Mechanical forcing of the north american monsoon by orography', **599**, 611–615.

Fu, D., Chang, P., Patricola, C. M., Saravanan, R., Liu, X. and Beck, H. E. (2021), 'Central american mountains inhibit eastern north pacific seasonal tropical cyclone activity', *Nature communications* **12**(1), 1–11.

García-Franco, J. L., Gray, L. J. and Osprey, S. (2020), 'The American monsoon system in HadGEM3 and UKESM1', *Weather and Climate Dynamics* **1**(2), 349–371.
URL: *https://wcd.copernicus.org/articles/1/349/2020/*

Geen, R., Lambert, F. H. and Vallis, G. K. (2019), 'Processes and Timescales in Onset and Withdrawal of "Aquaplanet Monsoons"', *Journal of the Atmospheric Sciences* **76**(8), 2357–2373.

Hill, S. A. (2019), 'Theories for past and future monsoon rainfall changes', *Current Climate Change Reports* **5**(3), 160–171.

Hitchman, M. H., Yoden, S., Haynes, P. H., Kumar, V. and Tegtmeier, S. (2021), 'An observational history of the direct influence of the stratospheric quasi-biennial oscillation on the tropical and subtropical upper troposphere and lower stratosphere', *Journal of the Meteorological Society of Japan. Ser. II* .

Kim, H., Caron, J. M., Richter, J. H. and Simpson, I. R. (2020), 'The lack of QBO-MJO Connection in CMIP6 Models', *Geophysical Research Letters* **47**(11), e2020GL087295. e2020GL087295 2020GL087295.

Liess, S. and Geller, M. A. (2012), 'On the relationship between qbo and distribution of tropical deep convection', *Journal of Geophysical Research: Atmospheres* **117**(D3).

Match, A. and Fueglistaler, S. (2019), 'The buffer zone of the quasi-biennial oscillation', *Journal of the Atmospheric Sciences* **76**(11), 3553–3567.

Montini, T. L., Jones, C. and Carvalho, L. M. (2019), 'The south american low-level jet: A new climatology, variability, and changes', *Journal of Geophysical Research: Atmospheres* **124**(3), 1200–1218.

Pascale, S., Carvalho, L. M., Adams, D. K., Castro, C. L. and Cavalcanti, I. F. (2019), 'Current and Future Variations of the Monsoons of the Americas in a Warming Climate', *Current Climate Change Reports* pp. 1–20.

Serva, F., Cagnazzo, C., Christiansen, B. and Yang, S. (2020), 'The influence of enso events on the stratospheric qbo in a multi-model ensemble', *Climate Dynamics* **54**(3), 2561–2575.