Response to comments of DPhil examination report

García-Franco, JL.

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.

Specifically, a general depiction of the global monsoon as well as traditional and novel theories to explain the existence of the monsoon were already present in the thesis. However, detail on dynamics and features of the SAMS and the role of orography for the NAMS and the SAMS was missing. In the revised version several more paragraphs 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.

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, I decided to provide detail on the roles of the Sierra Madre Occidental (SMO) and the

Andes cordillera 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, one important remark is needed to clarify a confusion by the examiners, which are understandably unaware of Mexican geography. The North American Monsoon and the Midsummer Drought regions are very different regions which, in my understanding, has caused the confusion leading to the statement "orographic effect... that will prove vital for the North American Monsoon System in Chapter 6" which is not accurate.

Chapter 6 investigates mechanisms of the Mesoamerican Midsummer Drought, which uses a study region about 2000 km south of the NAMS. More specifically, while the orographic effect may certainly be relevant for the NAMS, the results of Chapter 6 do not evaluate/mention/investigate the orographic effect. 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.

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

Several references of the SAMS were given and a clearer link to outstanding questions in the AMS was also revised.

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 as follows:

A key influence on the AMS is the El Niño-Southern Oscillation (ENSO), a coupled-ocean atmosphere phenomena in the Pacific Ocean, that has global climate impacts (McPhaden et al., 2006). Progress has been made in understanding the impacts and pathways of ENSO to South America (Marengo et al., 2012), and current research has found that ENSO variability promotes different impacts to South America (Hill et al., 2009, Tedeschi et al., 2015, Cai et al., 2020). However, how ENSO teleconnections and impacts depend on other modes of variability is still an active topic of research (Cai et al., 2020, Jimenez et al., 2021).

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 clearly.

Page 9: The paragraphs are not connected. From Walker cell, the text jumps to the ITCZ and then monsoon systems. The start of that paragraph now reads:

Another relevant aspect of the tropical circulation is the Inter-tropical Convergence Zone (ITCZ), which

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.

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. Note again, that the Midsummer Drought is a feature of Mesoamerica and not 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.

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 datasets and methods 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).

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.

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.

This chapter was originally reviewed [1] and published (García-Franco et al., 2020) for providing a detailed description of biases in the MOHC models for the AMS, a monsoon "rarely assessed". Corrections in this chapter provide more detail on:

- Mechanisms of ITCZ migration.
- 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.

[1] `https://wcd.copernicus.org/preprints/wcd-2020-8/wcd-2020-8-RC1.pdf`
`https://wcd.copernicus.org/preprints/wcd-2020-8/wcd-2020-8-RC2-supplement.pdf`

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 a first approach to validate any model simulation as forcing could be relevant for these comparisons. After describing the biases in the historical experiments, the differences between the picontrol simulations and the observations are described. The fact that low-level wind differences and some temperature differences 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. 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 North America – i.e., Mexico and the Caribbean.

Page 65: This is also not the case. 1) SESA is out of the area ofstudy; 2) The ENSO influence on SESA is made via changes in the South AmericanLow-level Jet (SALLJ) and less so viathe 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).

Page 66and 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 after the explanation fo the Walker cell shifts, the text in this Chapter now describes the links of the Walker cell to the precipitation anomalies. The links to the SLP in the tropical North Atlantic are also described and 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.

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 reviewers 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 chapte (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?Mainlybecause there areno consistent changes for El Niños.

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.

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 and the mid-to-upper tropospheric zonal wind anomalies are also notable. The arguments presented by the examiners posing the likelihood of chance or the mixing of ENSO events are good points, yet I would argue that both points lose ground in simulations of 500-yr length, but the topic is still open which is why we investigated the topic more closely in Chapter 7 and showed that the differences are not due to chance or the influence of ENSO events, and show a robust link. This discussion of this comment is more readily addressed in Chapter 7.

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. 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 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.

They are given at the same intervals, a closer inspection of the figure will make evident that 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 break down in 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 different to Sierra Madre Oriental and while this may seem like a simple mistake on Mexican geography there scientific reasons that make me question this comment. This error is relevant, first, for the cited work because their simulations do not only locally remove orography in the SMO, but also in the Sierra Madre Oriental, the Faja Volcánica Transmexicana and the southern cordilleras of Mexico, which in total span more than 4000 km just in longitude. I find the argument that the evidence of Boos and Pascale (2021) suggests that precipitation in the NAMS is "due to local orography" 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 wrong but covers up a scientifically questionable part of their simulations.

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, which makes me wonder whether the examiners realize the difference between the two study regions. 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 discussion on dynamic versus radiative mechanisms is relevant and is directly related to the work of Boos and Pascale (2021) which contradicts the conventional thermal mechanisms. For the purpose of such a discussion suggested by the examiners, however, the study of Fu et al. (2021) is a more relevant reference, but both citations are given now in a paragraph that discuses radiative versus dynamical mechanisms at the end of Chapter 6. The work of Boos and Pascale (2021) is also 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.

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 now been largely reworded to include also the results of Chapter 8. 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. The second part of the analysis is also shorter.

Note also that the text in the color blue for tracked changes aims to distinctly point to the examiner's comments, but most the text in the 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 extendingfrom 1979to 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 means that the false-hit case suggested by the examiners is less likely to occur.

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.

If there is no downward effect of the QBO on tropical convection and ENSO, then one would expect that in a long integration there would be no surface response in the tropics because ENSO events would not be aliased with the QBO phase. However, some effects are seen in the model simulations as well as in the observations. While one cannot determine the cause-effect of these relationships, the findings of Chapter 7 are a detailed description of robust associations between the QBO and aspects of tropical climate. The method section in the referred page now provides a line that links how the nudging experiments aim to disentangle what the regression analysis cannot do.

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 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. Traditional mechanisms would suggest a QBO effect over all regions with active deep convection, over land and over the ocean, however, only oceanic effects are seen in the simulations. This could be because SSTs affect the QBO, through unknown mechanisms, or because QBO-related cloud-SST feedbacks are relevant within the model.

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 new figure is more concise. The biases are large, which limits the validity of the results as a 'true QBO response', however, there is still value in finding a response in the model, because there is still skepticism that the QBO would have any real or simulated effect on the ITCZ. The text now discusses how model biases in the ITCZ reduce confidence that the simulated response can be compared to the observed responses, yet some seasonal mean patterns are strikingly similar.

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.

I disagree that there is a general problem with comparing observational variability with model variability from a pre-industrial control simulation. The chapter does no assess the representation of the mean climate in the simulations and compares it with observations. Rather, the chapter analyses variability, which is more readily examined in a simulation that has no external forcing and that undergoes several cycles of decadal oscillations that may exist in the model. 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 the Figure and analysis gives, and 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, to investigate how frequently would a QBO W-E difference value be found if the picontrol simulations were of the same length as observations. In doing so, we find the probability that a slice of the long pre-industrial control of similar length to the observed period would show a similar QBO W-E difference, in this case in the probability of IOD/ENSO events.

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 the regression, the regression does separate QBO and ENSO influences, 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. One would still need to provide a hypothesis which would have to explain why more active convection is seen in the Caribbean Sea, Indian and Atlantic Oceans in one phase of the QBO knowing this process is unlikely linked to ENSO.

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).

I am afraid I don't follow this comment. Those pages focus on QBO-Walker circulation relationships, the text now makes that fact clearer.

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 otherphenomena, such as MJO.

All the simulations have the same vertical resolution, so I had a hard time following this comment. Indeed this figure could be a result of the MJO, but this would necessarily have to point to the MJO-QBO connection (Martin et al., 2021). 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.

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.

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

A more detailed discussion on mechanisms is given which argues that the evidence of the CMIP6 experiments does not seem to support the primary hypothesis for a downward impact of the QBO, and introduces the need for targeted model experiments.

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 findingsto respond to the scientific question of the cause/effect QBO and the tropical troposphere.

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; itshould be latitude instead of longitude.And Caption doesnot 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.

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, removeit.Page 191: Parentheses missing in "(Figure8.6".Pages192and 193, Figures8.7and 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 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.

I would rephrase this slightly to say that the nudged QBO has no effect over SSTs. One would still need to explain the results in the control simulations, which still has a QBO (weaker than the nudged QBO), by providing a satisfactory mechanism of bottom-up influence or the opposite, i.e., a downward influence from the QBO.

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. (1) The nudging technique was not appropiate, 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. The discussion section now discusses these possibilities further.

Chapter 9: Conclusions

Pages 211-217: Overall,this work started by analysing SAMS and NAMS and links to ENSO QBOin 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.

This is more of a comment on my thesis rather than a constructive suggestion I can use to improve the conclusions chapter.

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.

Cai, W., McPhaden, M., Grimm, A., Rodrigues, R., Taschetto, A., Garreaud, R., Dewitte, B., Poveda, G., Ham, Y.-G., Santoso, A., Ng, B., Anderson, W., Wang, G., Geng, T., Jo, H., Marengo, J., Alves, L., Osman, M., Li, S. and Vera, C. (2020), 'Climate impacts of the El Niño–Southern Oscillation on South America', *Nature Reviews Earth & Environment* **1**(4), 215–231.

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, K., Taschetto, A. and England, M. (2009), 'South american rainfall impacts associated with inter-el niño variations', *Geophysical Research Letters* **36**(19).

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* .

Jimenez, J. C., Marengo, J. A., Alves, L. M., Sulca, J. C., Takahashi, K., Ferrett, S. and Collins, M. (2021), 'The role of enso flavours and tna on recent droughts over amazon forests and the northeast brazil region', *International Journal of Climatology* **41**(7), 3761–3780.

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).

Marengo, J. A., Liebmann, B., Grimm, A. M., Misra, V., Silva Dias, P. L., Cavalcanti, I. F. A., Carvalho, L. M. V., Berbery, E. H., Ambrizzi, T., Vera, C. S., Saulo, A. C., Nogues-Paegle, J., Zipser, E., Seth, A. and Alves, L. M. (2012), 'Recent developments on the South American monsoon system', *International Journal of Climatology* **32**, 1–21.

Martin, Z., Orbe, C., Wang, S. and Sobel, A. (2021), 'The MJO–QBO Relationship in a GCM with Stratospheric Nudging', *Journal of Climate* **34**(11), 4603–4624.

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

McPhaden, M. J., Zebiak, S. E. and Glantz, M. H. (2006), 'Enso as an integrating concept in earth science', *science* **314**(5806), 1740–1745.

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.

Tedeschi, R. G., Grimm, A. M. and Cavalcanti, I. F. (2015), 'Influence of Central and East ENSO on extreme events of precipitation in South America during austral spring and summer', *International Journal of Climatology* **35**(8), 2045–2064.