Response on comments of reviewer #2 in the interactive Discussion of the manuscript ‚Climatic changes between 20th century and pre-industrial times over South America in regional model simulations'

We thank reviewer #2 for his/her critical but very valuable comments on our manuscript – Please find below a point-by-point response on the comments raised in the review:

1. Major problems in the Introduction section

The Introduction is lengthily, poorly organized section that doesn't help at all in setting the stage for the rest of the paper, and contains fundamental errors in the climate description of South America.
The introduction began with a general description of the South American climate that has fundamental errors. Let's consider lines 24-25 in page 2983 "the westerly wind belt moves in north-south direction, depending on the position of the ITCZ which itself is controlled by the position of the Sun". Of course, the sun-earth geometry is the ultimate driver of the annual march of any climate variable, but I don't see the casual connection between the westerly belt and the ITCZ. Later on (page 2995, last paragraph), the authors attribute the precipitation over the central part of the continent to
the seasonal migration of the ITCZ: : :but the ITCZ is an oceanic feature and climate research over the last 20 years has emphasized the existence of the South American Monsoon System (e.g., Vera et al. 2006, J. of Climate). Another example: the authors attribute the stronger westerlies during summer to the enhanced subtropical – polar themal gradient. It is not clear what the "westerlies" are: the upper-level jet stream? The surface wind maxima? In any case, the tropospheric mean flow in the
SH is also determined by many other factors, including eddy-mean flow interaction and the localized near-surface thermal gradient along the Antarctic perisphery (e.g., Nakamura and Shimpo, Mon. Wea. Rev.,1997). Overall, the introduction and other sectioncontain several statements that are misleading oversimplifications, not acceptable for a scientific publication on climate.

We agree with the reviewer that in the introduction of the basic climatic features of South America we did not provide a deep review. Our intention was not to oversimplify the situation but to introduce the main climatic features in a few words – In the re-worked version of the manuscript we provide a complete new chapter on the more physical caused reasons for the basic climatic characteristics, including the South American Subtropical jet and related phenomena such as the South Atlantic Convergency Zone (Vera et al., 2006a,b; Carvalho et al., 2004). Also for the mid- and high latitude we provide a more thorough introduction into the structure and complexity of the southern westerlies/jet streams (Nakamura and Shimpo, 2003, Gallego et al., 2005; Nakamura et al., 2008). Moreover, the concept of the SAM/AAO (Thompson and Wallace, 2000) is introduced because parts of this concept will be used in a following chapter explaining the wind-induced climatic changes between present-day and pre-industrial times. The paragraph explaining the different meanings of the southern westerlies and the implications for comparisons with proxy data have also been introduced in this context. Moreover, as also raised by the reviewer in one of his following comments, we explicitly stated the hypotheses we tested with the model simulation, i.e. how do CO_2 changes affect the climate of south America and how do these changes compare with empirical studies. Also included in the introduction is motivation on the importance of the sea surface temperature as lower boundary forcing. The analysis of SSTs in terms of changes between

present-day and pre-industrial times is also included now in the new version in the results section of the manuscript.

On page 2984, line 25, the authors state that "one intention among others was to test hypothesis that are based on empirical evidence". This was like music for my ears, since the best use of models (in addition to prediction) is hypothesis testing; without them one can get lost in a sea of numbers. But in the next 6-7 pages I couldn't find those hypotheses. Instead, there is a partial but extensive review of much of the literature on South America's climate and paleo-climate, including use of models by other groups. It is not clear at all the relevance –for this work- of many of your references. The authors should state clearly THEIR hypothesis and use them a guide for the rest of the work.

The hypotheses we wanted to test with the simulations are now outlined separately and more concisely in the introduction. Also in the results section and the conclusion section this issue is picked up again and discussed. For the specific hypotheses we wanted to test what are the impacts of the CO_2 changes between pre-industrial and present-day times on the climate of South America and as a second hypothesis how these changes compare with results based on empirical evidence. In addition to the first version of the manuscript we now also took into account studies in the context of other GHG change simulations, for instance those used by the IPCC, 2007 (Christensen et al., 2007) and studies dealing with changes in the SAM/AAO. The latter was important for explaining the wind-induced changes caused by changes in the atmospheric circulation. Moreover, we also shortened the introduction by motivating only those points that are followed up later in the manuscript and put more emphasis on studies in the context of the last 300 years and those dealing with potential future climatic changes.

2. Validation section

2.1 The authors used only 6 station data to validate the PD simulation and they claim there is a "scarcity of meteorological observations" in South America. While SA has a low station density, there is certainly many (>40) more stations with climate data to validate the model contained, for instance, in the Global Historical Climate Network (the well known GHNC-V2 dataset).

For the validation of precipitation we used the GPCC data set – as outlined in the text the advantage of this data set is that the entering station time series are tested for inhomogeneity and therefore should provide a sound basis for comparisons. We however changed the phrasing of the of 'scarcity' to 'low density'

2.2 More worrying is the use of these 6 stations as representative of regional climate. I was dismayed to see that they use Antofagasta, a coastal station in northern Chile, as representative of the Central Andes. Likewise, Santiago (Puerto Montt) hardly represents the subtropical (southern) Andes. Sorry, this is basic geography....

We agree with the reviewer that six stations do not represent the entire climatic variety of the South American climate. To address this issue we avoided to establish a connection between a station and a climatic zone because this issue was apparently misleading. Our intention was simply to be as close as possible to a meteorological station where data are available for comparisons. In the re-worked version we included twelve instead of six stations.

2.3 The authors satisfy themselves by describing the model biases, but little is done in order to interpret these discrepancies.

We partly agree with the reviewer on this issue. Already in the first version of the manuscript we tried to explain specific model biases based on Figures 3–6, including explanations on the potential physical reasoning (cf. p 14 ll. 1ff; p. 14, ll 28ff). The validation already forms a substantial part of the body of the first version of the manuscript and we carried out the validation in several ways, comparing the observationally-driven simulation (i.e. ERA40) for both single stations (section 3.1) and spatially gridded fields (section 3.2). In addition, we also provide within section 3.2 a comparison between the output of the GCM-driven CCLM simulation and the raw output of the driving ECHO-G simulation to display the differences between the original and downscaled GCM output.
In the re-worked version we put additional emphasis on explaining the discrepancies between the ERA40-driven and the GCM driven CCLM simulations compared to observations, including also studies using the CCLM model for other regions of the world (Jaeger et al., 2008; Anders et al., 2009). We also stressed the point that it is important to take into consideration the differences in bias structure between the observationally (ERA40)-driven and the ECHO-G driven regional simulations.

3. Differences between PD and PI climate This must be the central part of the paper but, in its present form, is just a straightforward description of the difference fields of temperature, precipitation, SLP and winds. In the case of surface air temperature a key question -that I hope the authors could address- is the origin of the widespread warming: is it a local effect due to enhanced radiative effects or it rather depends on the prescribed lateral boundary condition? How important is the bottom boundary condition, that is, the prescribed SST? In the case of the precipitation, the relationship between precipitation and local wind (at the 700 hPa I guess) is shown in an article by Garraud (Journal of Climate, 2008), and it is a strong correlation that changes sign across the southern Andes. Perhaps the authors could further diagnose the precipitation changes and separate the wind-driven contribution from other factors.

This point has been addressed in several ways in the re-worked version of the manuscript:
First, the result section has been extended by the consideration of changes in sea surface temperatures between present-day and pre-industrial times, including a discussion on potential effects these changes exert on climatic differences and also, as for instance evident over the high southern latitudes, that may cause the changes in SSTs. Here important results pertain to the increased SSTs in the southwestern Atlantic high pressure cell and changes in the Ekman pumping induced via an increase of zonal winds over the southern ocean leading to a decrease in SSTs over these regions, dampening the temperature increase.
Second, in an additional analysis section the wind-driven changes in terms of a South American Zonal Index (Wagner et al., 2007) that is closely connected to the SAM/AAO have been investigated on temperature and precipitation in greater detail. The respective regression patterns have been compared and discussed in the context of studies already carried out (Gillet et al., 2006; Garreaud et al., 2007; 2009; Karpechko et al., 2009). Specifically for the study of Garreaud (2007) the similarities/discrepancies in the regression patterns between zonal winds versus SAM/SAZI and precipitation have been outlined and discussed in the light of dynamical reasoning.
An important issue addressed in the re-worked version is the separate analysis for the pre-industrial and present-day conditions in establishing regression patterns between changes in high latitude zonal winds and temperature and precipitation fields for the different seasons

and a discussion on the potential mechanisms responsible for the changes in the structure of the patterns.

*4. The English is acceptable but I found quite a bit typos and grammar mistakes. I am not listing them because I suggest reject *this* paper, but in the new submission the authors should perform a cautiously proofreading.*

In the re-worked version we took care for improved English and carefully checked the text for grammar and spelling.