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Climate of the Past Discussions

Interactive comment on "Climatic changes between 20th century and pre-industrial times over South America in regional model simulations" *by* S. Wagner et al.

Anonymous Referee #2

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Review of "Climatic changes between 20th centurty and pre-industrial times over South America in regional model simulations" by Wagner et al.

This manuscript presents a comparison of two regional simulations over southern South America of relatively high resolution, 30-years worth of integration each, one for the late 20th century and the other for the pre-industrial period. The simulations were forced by the ECHO-G atmos/ocean coupled global model, and they differ in the amount of greenhouse gases concentration.

Although the objectives of (a) regional model validation and (b) understanding the long-term climate variability / climate change over South America is a quite relevant topic,

this manuscript doesn't reach the level that I'd expect for a publication in Climate of the Past, so I recommend its rejection. My major points are commented below and I hope the authors take them for producing a new paper.

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 section contain several statements that are misleading oversimplifications, not acceptable for a scientific publication on climate.

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

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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.

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).

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....

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

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

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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.

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.

Interactive comment on Clim. Past Discuss., 7, 2981, 2011.