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Climate of the Past Discussions

Interactive comment on "Climate model boundary conditions for four Cretaceous time slices" *by* J. O. Sewall et al.

J. O. Sewall et al.

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Author Response to Reviewers: CPD-2007-0021

We thank both reviewers for thoughtful and constructive reviews. These boundary conditions were produced as part of a larger study and, thus, with a specific scientific target in mind. This is generally the case with the production of most model boundary conditions. It is rare that the boundary conditions are actually the "end", most often they are the means to an end, and, as reviewer 2 notes, boundary conditions are usually presented in the context of the model results. The change of this practice is a major impetus in the presentation of this manuscript. When boundary conditions are presented in the context of model results they are often relegated to a brief (a page or two at most) mention in the "Methods" section with the majority of the paper addressing the modeling results. In turn, it is then unclear whether it is the results of the model

that are now available to the community to replicate the analyses of the authors or the boundary conditions behind them as well. It is our hope that, in addition to providing our Cretaceous boundary conditions to interested researchers, this manuscript will encourage other researchers to publish more extensively on their boundary conditions, either independently or as significantly expanded sections in papers that highlight modeling results. The reviewer comments have helped, we believe, to make this manuscript a better presentation of our boundary conditions as a general resource for those interested in modeling the middle through Late Cretaceous. Specific reviewer comments are addressed below.

Anonymous Reviewer 2

Major comments:

1. p. 795. paragraph 1. The authors state that the datasets were "manipulated" and then describe these modifications in very general terms. My guess is that the authors actually used systematic rules to develop their boundary conditions. These rules should be described in detail. For example, the authors list the expansion of narrow ocean gateways as one alteration. To what width were the gateways altered? How exactly was internal drainage modified? What are the exact numerical difficulties with narrow gateways and internal drainage?

The text has been expanded to provide more detailed, systematic rules for the alterations of the original DEM's. This text is in the second paragraph of the Methods section.

These boundary conditions should be reproducible. As written, this is not possible.

The reviewer is correct that the boundary conditions were not reproducible as written. Hopefully the details added to the text will make the process more transparent. However, we would note that one of the challenges to producing boundary conditions,

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and one of the reasons to encourage multiple use of the same boundary conditions, is achieving reproducibility. In almost all instances in "deep time", boundary conditions are derived from a limited data set. In addition to sometimes being small in absolute number of data points, those data sets often contain regional biases. Therefore, interpolation is often a large part of the creation of boundary conditions. As the precise details (edges of vegetation zones, lateral extent of high elevations etc.) of boundary conditions will, thus, tend to be rather subjective, the goal of absolute reproducibility is unlikely.

The authors should also reference previous publications that have described the development of boundary conditions (e.g. see Tectonic Boundary Conditions for Climate Reconstructions and references therein).

References from Tectonic Boundary Conditions for Climate Reconstructions as well as other examples have been added in the second paragraph of the introduction.

2. p. 795, paragraph 2. Describe the pole problem. Climate modelers will be familiar with this problem, but the general reader (geologist, oceanographer) may not be.

The pole problem is now described in paragraph 5 of the Methods section.

3. p. 796, paragraph 1. The authors chose to systematically vary the presence/size of the Cretaceous Interior Seaway from the early to late Cretaceous, and suggest that this will allow for sensitivity experiments that capture the response to geographic changes. I don't entirely agree with this logic. To capture the sensitivity to the presence/absence of the Cretaceous Interior Seaway, all other geographic conditions should remain constant and only the seaway should change. I would suggest publishing multiple versions of each time slice within different seaway constructions with some indication of the "best" or "most likely" boundary conditions.

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The reviewer is correct that for a "clean" sensitivity study only one forcing condition at a time should change in a simulation. As we state above and in the manuscript, these boundary conditions were created as part of a specific project. The first phase of that project involves coupled, earth system model simulations of four Cretaceous time slices. In later phases, targeted sensitivity studies will be conducted as appropriate. As the reviewer may be aware, such coupled simulations are computationally and temporally expensive. It is, therefore, impractical for us to produce boundary conditions that are extremely similar. Instead, we choose to investigate time slices with notable differences and, where possible, err on the side of difference. In this way we can, as we state in the text, gain some idea of climate sensitivity to large-scale changes in geography. This information will, in turn, be valuable in assessing which sensitivity studies (as there are obviously multitudes of possibilities) will be most valuable to conduct. We do not, in the text, indicate that the boundary conditions presented here are intended for targeted sensitivity studies. The wording at the end of the 5th paragraph of the Methods section has been altered to avoid any further confusion on this point.

4. p. 797, p. 1. The boundary conditions are provided as a 2.8 x 2.8 gridded data set. This may be relatively high resolution today, but doesn't reflect the resolutions that will be used in the future (even the very near future). If this is truly a resource for the paleoclimate community, I would suggest that the authors provide Cretaceous data sets at a higher resolution (1.0 x 1.0) that could then be easily interpolated to lower resolutions. The authors might also indicate what the limit on resolution is. For example, would any geologist believe a data set gridded at 0.1 x 0.1?

2.8° x 2.8° degrees (spectral resolution T42) is, indeed, relatively high resolution for paleoclimate investigations today. It is not clear that investigations of deep time (e.g. the Cretaceous) will necessarily evolve to significantly higher resolutions. The capability to model at T42 has been available for a number of years, yet many paleoclimate investigations continue to be run at coarser resolution. In fact, even a number of the

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models used for modeling the present and future for the IPCC 4th Assessment utilize a resolution coarser than this (e.g. CCCMA CGCM3.1, GISS ER, IPSL CM4, INM CM3). While we do not dispute that models will likely be run at higher resolutions in the future, it is unclear that there is a trend toward resolutions approaching 1° x 1° any time in the "very near future". The primary root behind this tendency to continue with coarser resolutions for paleoclimate is twofold: 1) Garbage in = Garbage out. As the reviewer notes and we note above, most boundary condition datasets have large "interpolated" gaps. Interpolating an already limited dataset to higher resolution only has the effect of increasing the number of grid boxes (and, therefore, the computational cost) but does not increase the amount of actual information coming into the simulation (e.g. a mountain range will not increase in roughness as you create more grid boxes because the information on fine scale peak and valley elevations is not available). For this reason, it is of limited utility to increase horizontal resolution for limited gains in information and potentially debilitating gains in computational requirements. 2) Which is a corollary to the first, is that many paleoclimate problems would benefit from longer simulation times. If model resolution continues to increase in concert with computational power, the same "equilibrium" simulations that involve a single ocean spin up phase will be all that are reasonably possible. Maintenance of lower resolution allows increasingly longer simulations as computation power increases. In fact, recent years have seen an increase in progressively lower resolutions for some paleoclimate simulations with just such long, time transgressive simulations as the goal. Finally, 1° x 1° raw DEM files are available for purchase from the PALEOMAP project if researchers are actually interested in that resolution. For many of the reasons outlined above, we have produced a modest increase over current paleoclimate modeling resolutions and feel this is adequate to address numerous outstanding questions in our understanding of the Cretaceous climate system.

5. p. 798, p. 2. "based on expected, large-scale climate patterns " Again (see comment 1), specific rules should be described in detail. This should be reproducible!

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The final paragraph of the Methods section has been updated to indicate exactly what the "expected, large-scale climate patterns" are.

6. p. 796, p. 2. In this paragraph, the authors describe their decision to limit the Cretaceous Interior Seaway since Blakey and Scotese disagree and their Turonian/Cenomanian reconstruction has an open seaway. I am a bit alarmed by this logic. Shouldn't it be based on geologic evidence (e.g. White et al., 2000)?

While we apologize for any alarm we may have caused the reviewer, we hope that the response to major revision comment 3 above will have shed some light on our desire to enhance differences between our boundary conditions within the bounds of the data. We will further note here that our Albian time slice is an early Albian slice centered on 110 Ma (nominally +/- 5 Ma). The work of White et al., 2000 is focused on the middle to late Albian and, in fact, if one were to look at their Table 1, you would note that of 21 data localities, only six are exclusively dated as "Albian" and of those six, five of them are listed as "Late Albian". The remaining 70% of the data include the Cenomanian or even the Turonian as a date option and six formations are dated exclusively as Cenomanian and/or Turonian. Given these date ranges, this information does not seem relevant to an early Albian time slice. It is, however, precisely these discrepancies in dates and interpretation that make the early Albian extent of the Cretaceous Interior Seaway debatable. As such, we have chosen the configuration that best fits into the overall framework of the four Cretaceous boundary condition sets we have developed. Finally, we thank the reviewer for providing the White et al., 2000 reference and have added it to our manuscript in paragraph seven of the Methods.

In general, I find this contribution to offer little new data or data compilation. Most of the boundary conditions (particularly paleogeography) are based on global data sets that already exist and are currently used and referenced by the paleoclimate community.

While the reviewer is correct that all of the information we have used to create our

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boundary conditions is published or available to the community in some way, the compilation into boundary conditions for climate modeling is our contribution. This is particularly true of the vegetation where even recent studies of the earlier Cretaceous have used modern, average forest cover in the absence of a global, boundary conditions data set. We will further note that the new, 1° x 1° PALEOMAP DEM's on which our paleogeography and elevations are based are not yet in frequent use by the paleoclimate modeling community.

Minor comments:

1. p. 793, paragraph 1. The choice of references in the first sentence is peculiar. I would recommend either citing the early pioneers of paleoclimate modeling (especially Barron, Kutzbach, Crowley, but also including Otto-Bliesner, Valdes) or, since this manuscript focuses on the Cretaceous, those researchers that have published Cretaceous paleoclimate studies (Barron, Valdes, Brady, Poulsen, Bice, DeConto, etc.).

In the context of the first sentence we do not find the choice of references peculiar at all. The intent is to demonstrate a trajectory of increasing complexity in climate models over recent decades. While we could have chosen many models to demonstrate this, we chose to use the "family" of models related to the NCAR CCSM (the model with which we are most familiar). The first two references in that list use older, dynamic AGCM only, coarser resolution versions of the NCAR CCM. The second two references in the list use newer, fully coupled, higher resolution versions of the NCAR CCSM/CSM model. The final two references focus on using models to address problems with increased complexity, in the case of DeConto and Pollard (2003), dynamic ice sheet modeling and in the case of Sewall and Sloan (2006), high resolution regional modeling of paleoclimates. The general trend presented by these studies is one of models and studies that require increasingly detailed boundary condition inputs. To make this connection clearer, we have combined the first and second sentences and place the references at the end of this [new] first sentence.

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2. p. 793, paragraph 2. The development of boundary conditions is a non-trivial task for anyone.

This is absolutely true and is the fundamental reason we are publishing this paper and making these boundary conditions available.

The phrasing of this section seems to imply that geological or paleontological training is required. Yet, in this paper, the development of the boundary conditions relies completely on published references. I would edit this section so that it doesn't seem to slight non-geologists.

No one of whom we are aware actually goes into the field, collects a global paleoelevation database and a paleovegetation database before creating climate model boundary conditions. As such, global compilations are almost always based on published references. As we have noted previously, the "global" data sets are often lacking in coverage and interpolation is necessary. Interpolation between and interpretation of published geological and paleontological data is no doubt easier for someone with familiarity in those disciplines than it is for someone with no background in the methods of collection, interpretation, and the associated errors. This section is not intended to slight non-geologists whose expertise we note is critical but simply to point out the reality that certain expertise is more or less applicable to a certain task (a paleontologist, for example, is probably not the best person to update cloud parameterizations). This is in no way a slight, it is simply a fact of studying the integrated earth system, multiple researchers with multiple expertise are necessary. Our hope in publishing these boundary conditions is that more researchers with critical expertise will address the climate of the Cretaceous now that the potential barrier of creating boundary conditions has been removed. This is explained in the Introduction and the Conclusions.

3. p. 796, paragraph 1. The sinking of islands is justified on the basis of computational expense. It would be useful to give the reader (i.e. the non-modeler) an idea of how expensive the islands are and how long simulations usually run.

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Per accurate comments from Reviewer 3 (see comment 9) reference to striking islands due to computational expense has been stricken from the manuscript. Please refer to responses to Reviewer 3 for a more complete discussion.

4. p. 793, paragraph 2. "In the Albian This is a typographical error. Blakey created his boundary conditions in the present day, not the Albian.

The text has been altered to remove this inaccurate phrase.

5. p. 798, paragraph 2. Could the biomes be summarized in a table? The authors might also consider including additional information, such as the albedo, roughness, etc., that they think is appropriate for each biome.

The biomes are now listed in Table 1. As there is no precise albedo roughness length etc. data available from the Cretaceous and all vegetation schemes should provide roughly similar numbers for a given biome (e.g. Deciduous forest, shrubland etc.) we do not think that the addition of the values produced by the particular model we are using (CCSMv3) adds particularly valuable information to the reconstruction. The technical note (Bonan, 1996) from which the basis for our biomes is derived is now referenced in the final paragraph of the Methods section as a resource for those interested in the details of particular plant types.

6. p. 799, paragraph 1. "Expert commentary was then integrated into the final paleovegetation distribution." What does this mean? Please describe the decisions/suggestions made by the experts.

As both reviewers had negative comments associated with this phrasing, we have removed it from the manuscript. In most instances the comments from the paleobotanists were rather general on the order of agreement that, while the type of plant might have been different in the earlier time slices prior to the radiation of angiosperms, the functional niche of the plant was probably consistent with that of the more "modern" angiosperms. In one instance particular reference to unpublished flora was made and

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that is now referenced as personal communication in the last paragraph of the Methods section.

Anonymous Reviewer 3

This under taking might - as intended by the authors - indeed save climate modellers without geological background quite some time and help to ease model intercomparisons. This requires, however, that the generation of these boundary conditions is transparent and represents the state of knowledge. At least the first point is not in all aspects fulfilled.

This is especially true for the reconstruction of the vegetation. It does not become clear from the paper, where data points are available and where the data sets basically represent interpolation/guessing from the authors. There is no discussion at all about uncertainties with respect to these quantities.

I believe that the general idea is good and that the data have the potential to become widely used, but after my opinion the construction of the data set needs to become more transparent and uncertainties need to be discussed more thoroughly.

As we note in responses to Reviewer 2 above, we have updated and expanded our Methods section in an effort to improve the transparency of the boundary condition creation process and address the uncertainties therein.

Specific comments: 1) If the original database is 1x1, I guess the only reason to go to 2.8x2.8 is that this is the intended model resolution. However, some climate modellers might want to use a higher resolution, thus a topography on 1x1 with a few modifications from the original topography might be much more useful.

Please see the response to Reviewer 2 Major Comment 4 above.

This is especially true for narrow throughflows. Just from the fact that a through-

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flow is narrow and shallow one cannot conclude that it is unimportant (e.g. Gibraltar, Bering, Faroe-Shetland). The minimum representation of the throughflow - even with similar resolution - is grid dependent. In C-grid models 1 open grid point is required, in some other grids 2 or 3. Thus the enlargement/opening of the throughflows should be done in each model separately.

As we note in our general, opening comments, these boundary conditions were created (as most boundary conditions are) as part of a particular study. They are, therefore, presented "as is". Minor modification of a particular throughflow by interested researchers should be a relatively trivial task once the NetCDF files are in hand. We have furthermore updated our Methods section to indicate our approach to dealing with narrow throughflows.

2) p. 793, 23 Are dynamic meteorology and atmospheric dynamics really different disciplines??

This depends on whether you are a "lumper" or a "splitter". In either case, we have removed this potential redundancy from the second paragraph of the Introduction.

3) p. 794, 5 What is the motivation for particularly choosing these time slices?

The last paragraph of the Introduction has been updated to provide this information.

4) p. 795, 25... These paragraphs should be merged with the description (and plots) of the topographies starting in p.799,line 15.

These paragraphs remain in their original position in the text as they describe actions taken in the creation of the boundary conditions and, therefore, "methods". The Boundary Conditions section contains descriptions of the boundary conditions, not the actions taken in creating them.

5) p796 For ocean modellers sill depths might be of interest.

Given the number of sills in these four time slices an exhaustive list would be just that.

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The plots give a general and adequate sense of sill depths and parties interested in a particular sill or sills can request and view the NetCDF files. Ncview is a particularly useful tool for determining a precise value in coordination with a location on a plot.

6) p798 The vegetation reconstruction seems to be somewhat subjective. At least on the plots should be marked, where data are available and which parts are intrapolation/extrapolation. As a person not specialized in land vegetation, a plot of the preindustrial biome vegetation with these biomes would have been helpful. For me it is impossible to judge the uncertainties in this process. A formulation like "The completed paleovegetation distributions were then distributed to members of the paleobotanical community for consultation and presented at an international conference for comment ... Expert commentary was then integrated into the final paleovegetation distribution." (p. 799) is highly unspecific and does not help me at all.

The final paragraph of the Methods section has been revised to provide more information on the rules used in creation of the vegetation distributions as well as the particular references used for each time slice. Furthermore, per the reviewer's request, Figure 6 has been updated to include points indicating data localities gleaned from previous reconstructions (Vakrahmeev, 1991, Saward, 1992, Otto-Bliesner and Upchurch, 1997) and asterixes indicating other points added in the creation of these reconstructions. The reference to "expert commentary" has been removed. See response to Reviewer 2, minor comment 6 above as well.

7) Fig. 6 Some structures of the reconstructed vegetation are somewhat surprising. The equatorial belt seems to dominated in A,B and D by dry vegetation (dry shrubland and savanna). Is this a robust finding or just the consequence of having only data points in the subtropics and subsequent interpolation???

As Figure 6 now indicates, all of the time slices have some equatorial data points on which the vegetation distributions are based.

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8) Fig. 6 Land ice is surrounded by high latitude mixed forest. Is this reliable? Even if the ice seems to rest in higher altitude?

While in reality the ice would be surrounded by some sort of periglacial environment and a gradation through alpine tundra and high latitude evergreen forest, our resolution is insufficient to capture this and so all of the tundra/periglacial/glacial environments are lumped into the few grid cells labeled as "ice". Thus ice abuts mixed forest.

9) p.796 3 Islands are not computationally expensive per se in ocean models! In many models they don't cause problems other than being difficult to resolve. In some solvers for the barotropic stream function/ barotropic velocities, however, they are expensive. This should be made clear in the text.

The reviewer is correct and we have removed this phrasing from the manuscript as the main reason for removing or combining islands (as explained in the revised Methods section) in these boundary conditions is to allow for flow and to remove narrow throughflows as in our ocean component (based on LANL's POP model), islands are not problematic.

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