Dear Dr. Hansen:

Below please find 3 reviews of your paper "Dangerous human-made interference with climate: a GISS modelE study." The reviewers have suggested revisions to your manuscript. Please take the reviewers' remarks into consideration and adequately address their questions and concerns with a revision of your manuscript.

This decision is late because the reviews were themselves late. This was beyond my control and due to the excess length of you submission. While I accepted at the time of submission that your work may have been better considered as a single work, it is now clear on the basis of the reviewers' comments that I am unable to obtain a comprehensive review of your complete work. Therefore, I request that you follow the recommendations of the reviewers and submit the work as two separate papers with a more selective use of figures, or a single much shorter paper. As I am now unable to obtain objective reviews from the reviewers I used this time, I will send the revised versions to new reviewers. I note further that if you had withdrawn your paper after the first review had been received, as I had suggested, the same situation will have occurred, but possibly a month ahead of the current schedule.

Please submit your revised manuscript and a detailed response to each question and comment of the reviews. The revised manuscript must be returned within one month of receipt of this letter. Failure to meet this deadline may result in the revised manuscript being handled as a new submission. If you feel that you cannot address all comments and revise the paper within one month, please contact me immediately.

Sincerely,

John Austin
Editor, JGR-Atmospheres

Comment on the Hansen et al. paper

I would like to apologize to the lead author, Dr. Jim Hansen, and to each and every one of the co-authors, for the inordinate delay in writing this comment. I label this note as a comment simply because it is not a review. And, thus I issue my second round of apology to all the authors, this one for not being able to do a proper review of their paper. My notes below will explain partly why, the other reason is the tight constraint on my time.

This paper has a provocative title, but this is a sign of where we are now on the issue of human-influenced climate change. The model experiment set-up and simulations performed are undoubtedly significant and, as far as I could see, present insightful perspectives on the topic. The basic science in the paper is sound, the questions addressed are relevant, and the expertise and experiences of the lead author and the team are quite apparent in the paper.

The paper, however, is a daunting one through the vast scope of the areas covered, and thus in its length. Not only daunting, but it is excruciating as well. The length poses a serious barrier in my consideration of whether such a voluminous paper belongs in JGR, no matter the quality of the science that is contained in the paper. It is very important that the decision on publication of a paper examine the presentation aspects thoroughly. But, there is an even more serious problem than length in this paper. And, that is the figures. On this point alone, the paper cleanly fails my test. I simply cannot believe the absence of thought and planning in the presentation of the figures. To see postage stamp-sized panels, and several of them (six or more!) forming one figure, and several such figures in all, is a remarkably unscientific presentation. Just how do the authors hope to convince the readers of their points if it is going to take a microscope to study the results? This is totally inexcusable. I can see that, in most of the figures, not all panels are important, so why should all the panels be there? To me, the presentation of so many panels is self-destructive to the main message/s, and severely distracting for the reader. In a sense, the authors are perpetrating an outrage on the reader – they want the reader to figure out, after scrutinizing all the panels, which ones are important. But, please, why should the reader do this job? Why cannot the authors do some more thinking, and decide themselves as to how to make the figure more succinct and appealing, scratch out the least relevant ones and thus make the main message more transparent? Please do not dump model simulation outputs directly onto the JGR pages. Much more discrimination is required, and this obviously will require considerably more work on the part of the authors.

I probably lack a high moral plane to make such critical remarks given the delay in my comment, but the length+figure issue is going to be a big obstacle for most readers. Every time I tried to pick up the manuscript and work through it, I inevitably stumbled hard in trying to cope with the figures and trying to gage the relevance of each panel. I hope the authors take this as a constructive comment, since I would like the principal

message of this paper to come out in print in JGR. Equally, the authors would desire the reader to actually read through their work.

I recommend that the authors be asked to extensively revise their paper, and focus on a few important points. If necessary, they should make 2 or 3 separate parts of this work, with each part being distinct, self-contained and easily readable. The authors will likely be making their message/s more effective if they had much shorter papers. But, above all, the figures desperately need loving attention.

In summary, JGR should not go ahead with the publication of the paper in its current form.

Reviewer #2 Evaluations:
Assessment: Category 3
Ranking: Fair
Annotated Manuscript: No

Reviewer #2(Comments):

This paper provides a very comprehensive model study of the potential effects of human-induced perturbations on past and future climate. It summarizes model runs based on IPCC scenarios, and it documents the simulations submitted for the IPCC AR4.

It is important that these scenario runs be documented to a certain extent in the peer-reviewed literature. The paper includes a lot of information. It is rich, well written and includes comprehensive results and detailed figures.

The paper appears, however, to be too long and too detailed for a JGR paper. Many figures include so many small and almost unreadable panels that they are useless for the JGR readership.

I would suggest to redesign the paper, to be less comprehensive/detailed or to divide the paper in several more specific articles with fewer and more readable figures. I also suggest that the authors make use of appendices or 'auxiliary material/electronic supplements'. If the authors wish to publish the submitted paper in its present form, I would suggest to produce a technical note and to submit a smaller and more focused paper to JGR.

Review of "Dangerous Human-made Interference with Climate: A GISS ModelE Study" by Hansen et al. (manuscript 2005JD007032 submitted to the Journal of Geophysical Research - Atmospheres)

RECOMMENDATION:

Publish after revisions that shorten the paper and / or break it up into two or more papers.

SUMMARY:

Hansen et al. present simulations of past (1880-2000) and future (21st century) climate change from a three-dimensional model including the atmosphere, ocean, sea-ice and land surface. The relevance of the subject is clear. "Dangerous Human-made Interference with Climate" is a paraphrase from the Framework Convention on Climate Change. The FCCC was signed and ratified by many nations including the USA and -- unlike its Kyoto Protocol extension -- has been endorsed by the present Administration. It is thus an established goal in the USA and around the world to avoid dangerous human-made interference with climate. The question is how.

Hansen has become famous for speaking his mind. He managed somehow to offend not only the elder and younger Bush Administrations, but also the Clinton Administration in between, by his statements on global warming. This paper sticks to the science and is appropriate for a geophysical journal, except for remarks made at the end (see comments below re Section 8).

COMMENTS:

Computer limitations have traditionally constrained GISS models. I was not surprised to read that ModelE's grid-point spacing is 4 degrees latitude by 5 degrees longitude in the atmosphere (and ocean?), rather coarse resolution by today's standards. On the other hand, this resolution was the state of the art 20 years ago, and 20 years from now today's state of the art will look pretty crude, yet we do not reject the basic conclusions of past climate models or expect our current ones to be invalidated on this basis. In short, I don't consider resolution or other "model deficiencies" the authors discuss (lack of ENSO and weak stratospheric variability) as show stoppers. It would nevertheless be helpful to see more comparison with other climate models, as recommended below.

The model simulations reported in this paper include not just greenhouse gases and aerosols, but essentially all possible climate-forcing factors that have been recognized to date. The authors describe them in enough detail for other modelers to try replicating their results. This might seem routine but is actually infrequent in climate modeling papers (a comment that also applies to the authors' use of alternate data sets to gauge observational uncertainty). I don't like to see "aerosol

indirect effect" defined as a forcing, since it's actually a model-simulated response of the clouds, but doing so is conventional.

The simulations achieve pretty close agreement with the observed time history of temperature at the surface and in the atmosphere. This includes reasonable agreement with data from Microwave Sounding Units aboard satellites since 1979. The authors compare their simulations with various and sometimes-contradictory interpretations of MSU data. Model-simulated warming in the lower to middle troposphere agrees well with the most believable of the MSU interpretations (and everyone working with MSU data now gets a fair degree of warming below the stratosphere, consistent with model). For the global mean surface warming over the period 1880-2003, the authors' simulations obtain 0.5-0.8 K depending on input assumptions, which nicely brackets the numbers 0.6 and 0.7 K obtained from two different analyses of surface observations.

Any model attempting to simulate weather and climate around the world will have errors that are nontrivial and difficult to quantify. The authors' main discrepancy with observations is that their model (like most others) misses the peak temperatures c. 1940 and subsequent cooling observed at higher northern latitudes during the middle of the 20th century. The authors suggest this was mainly an unforced natural fluctuation in the climate system. One would then expect the model to reproduce it only if initial ocean conditions precisely matched reality, an unreasonable requirement considering the limited observations available from the late 19th century. These limitations also "excuse" the disagreement between modeled and observed ocean temperature after the Krakatau volcano eruption of 1883. In any case, overall global warming observed during the last quarter-century -- and the brief cooling after Pinatubo's 1991 eruption -- are both simulated accurately by the model.

A more serious problem arises from lack of knowledge about aerosols. "Even with the most optimistic assessment of our understanding" the authors "must conclude ... that the net forcing [responsible for climate change during the period 1880-present] is uncertain by about a factor of three" (Page 21). Furthermore, according to conventional wisdom, another factor-of-three uncertainty applies to a model's response to a given amount of forcing: the canonical 1.5-4.5 K global warming range for doubled atmospheric carbon dioxide. On Page 70 the authors quote (but forget to reference) paleoclimate arguments that constrain this range a bit, to 2-4 K. Still, there is a real possibility that the model is getting the right answers for the wrong reasons when it simulates the climate of the last 120 years. This disturbing situation is no worse for the authors' model than for any other, but it left me feeling uneasy as I starting reading the second half of the paper, which deals with future climate.

Fortunately, the authors concentrate on robust features of their model's simulations. The model predicts global mean surface warming at least as great in this century as it was in the past century no matter what we do, but it implies that whether or not extremes (regional changes 5-10 times natural standard deviations) become the norm depends on human choices. "Business as usual" emissions of greenhouse gases and pollutants would, for example:

(1) Subject ecosystems, wildlife and humanity "to conditions far outside their local range of experience," according to the model's output statistics.

(2) Eventually produce an entirely ice-free Arctic in the summer season.

(3) "Likely" melt ice sheets in Antarctica and Greenland on a time scale of centuries rather than millennia, with "1 m or more of sea level rise in this century." This prediction does not come directly from the model, but it is made plausible by the model's simulated temperatures and some known dynamical features of ice sheets. The authors point out that traditional ice / climate models oversimplify ice sheet dynamics, an appropriate choice for slow time-scale problems like Ice Age cycles but a dubious assumption for the problem at hand.

Any of these developments could reasonably be called dangerous human-made interference with climate. According to the model, most or all of them could be avoided under an "alternative scenario" of emissions, which the authors have published elsewhere. Figure 24 clearly shows that over the past few years the alternative scenario agrees with observed emissions at least as well as the IPCC business-as-usual scenarios (which were developed quite a while ago). On the other hand, the alternative scenario envisions the world's total carbon dioxide emission rate leveling off soon and then declining later in this century. Making this happen would evidently require concerted changes in the energy policies of many governments, above and beyond the present combination of declining responsibility (in China and India), volunteerism (in the USA) and modest, poorly-enforced mandates (in most Kyoto Protocol signatories).

This paper is a stunning achievement and certainly merits publication in some form. However, there is one problem that in my opinion must be dealt with first: the paper is enormously long. I count 46 authors, 94 manuscript text pages, 200-300 references and over a hundred figures. Officially there are only 26 figures, but most have several parts. For example Figure 3 has 20 parts, and Figures 16-17 each have 80-100 latitude-longitude maps arranged like a stamp collection. The length problem could be partly addressed by just dividing the paper into two or more parts -- e.g., forcing uncertainties, simulations of the past, and simulations of the future. Some judicious text cutting would also be in order. I suggest a

close look at Section 5.3, "Other Climate Variables," which seems an overly exhaustive treatment, and 6.2.2, a digression on recent hurricane trends that is out of place in a section about future projections. Relegating many figures to a Web site can also be done without impacting the paper's message.

A related criticism is that the authors largely confine their discussion to a single climate model, despite the well-known phenomenon of different models giving different results. IPCC's 2001 assessment report on the science of climate change lists 34 different global climate models (in its Chapter 8). Now there are more. Perusing the IPCC repository mentioned on Pages 7-8, and focusing on aspects of the simulations that are either (a) shared by most models or (b) simulated better by GISS than by the average GCM according to plausible argument, could shorten the paper.

Finally, Section 8 of this paper is a long "summary" that is overly repetitious (especially in Section 8.2) and ultimately becomes a discussion of economics and technology that I think inappropriate for a geophysical journal.