We thank the reviewers and the editor for their considerable assistance. We appreciate the efforts required to deal with a long paper, especially one that explicitly addresses implications of the science for the public and governmental policies, we recognize the reviewer's expertise and insights as revealed by their reviews, and we wish to acknowledge the significant improvements in the paper that resulted from these expert reviews.

The first author (JEH) also acknowledges the unusual contributions of his co-authors, who include world-leading experts in several disparate disciplines essential for assessing the broad issues of concern. As discussed in the paper's Introduction, the paper aims to be clear and understandable to a broad audience including the judiciary, which in many nations is better able than other branches of government to help assure that policies are responsible in the long-term. Although assistance of these experts was requested to assure the paper's accuracy in their specific areas of research, in fact they all contributed to the overall content, organization and conclusions of the paper, and I thank them for the extra efforts required by the several iterations of the paper. This paper complements an earlier paper with similar overall objectives [Assessing 'Dangerous Climate Change', PLOS ONE, 8, e81648, 2013], which also included world experts in additional relevant fields (economics, human physical and mental health, glaciology, species extinctions, and coral reefs).

Below we describe the changes made in response to reviewer suggestions, after first noting a few updates in addition to those requested. We are submitting a version of this revised paper that "tracks" all changes from the prior version, as well as a clean version that incorporates all of the changes and locates the figures conveniently.

Updates/Corrections:

1. Global temperature data is updated through April 2017 in several figures and greenhouse gas amounts are updated to the most recently available data.

2. Several references have been updated to their final published version. Recent references on CH_4 were added in Appendix A8 and reference to the recent Etminan et al. paper on CH_4 climate forcing was added in Appendix A10.

3. On page 2 the quotation from the Paris Agreement, which was from a draft of that Agreement, has been replaced by the quote as it appeared in the final Paris Agreement.

4. In Section 8 the description of concern about the rate of emission reduction in the Alec L v. Jackson (2012) case is modified, based on a transcript of the court proceedings.

5. Andrew Lacis, who developed the radiation formulae we are using for climate forcings and who made useful inputs to the current paper, is added as co-author.

Response to reviewer suggestions:

Reviewer #2

The readability of the paper was my main concern on the last round, and this is now much improved. Going through the new draft, I've jotted down a few additional points. I expect these can be easily addressed.

236, 242-245. A perceptive reader may wonder why sea level didn't fall by 3-5m from the early to late Holocene, when temperatures fell by ~0.5C. It would be good to explain why the stability of sea level through the Holocene doesn't undermine the assumed sea level/warming connection.
<u>Response</u>: It takes a little space to address this topic, but we have added the following:

Near stability of sea level in the latter half of the Holocene as global temperature fell about 0.5°C, prior to rapid warming of the Modern Era (Fig. 3), is not inconsistent with that global cooling. Hemispheric solar insolation anomalies in the latter half of the Holocene favored ice sheet growth in the Northern Hemisphere and ice sheet decay in Antarctica (Fig. 27a, Hansen et al., 2016), but the Northern Hemisphere did not become cool enough to reestablish ice sheets on North America or Eurasia. There was a small increase of Greenland ice sheet mass (Larsen et al., 2015), but this was presumably at least balanced by Antarctic ice sheet mass loss (Lambeck et al., 2014).

311: This is reworded from the last draft, but the rewording didn't address my concern. The juxtaposition of this sentence and the sentence starting later on line 329 may help bring the problem into clearer focus: "Climate forcings in Figure 4 are the planetary energy imbalance caused by the preindustrial-to-present change of each atmospheric constituent… Earth's energy imbalance is the portion of the forcing that has not yet been responded to." Two possible fixes: (1) Don't try to define forcing at all, and point to a reference that defines it, or (2) Define forcing rigorously as a hypothetical imbalance that would be obtained if climate is not allowed to change. In this case the distinction between actual and hypothetical imbalance needs to be clearly spelled out.
<u>Response</u>: o.k., we understand the reviewer's concern. The problem is fixed with the addition of half a sentence and a reference, and in so doing we have actually included both of the reviewer's suggested alternatives. The added half-sentence makes clear that the energy imbalance refers to the hypothetical situation with the climate held fixed at its preindustrial state. The reference includes a full discussion of climate forcing and its relation to planetary energy imbalance. The sentence at former line 311 now reads:

Climate forcings in Fig. 4 are the planetary energy imbalance that would be caused by the preindustrial-to-present change of each atmospheric constituent, if the climate were held fixed at its preindustrial state (Hansen et al., 2005).

Figure 8 caption: Need to insert the symbol DeltaFe parenthetically after "Effective climate forcing".
<u>Response</u>: We have made the suggested change.

Figure 9 caption: The main black curve on left is not adequately defined in the legend. Actual emissions until some year ?, then constant until year ?. Okay, reading on, I see that these details are clarified in the text. But the figure should stand better on its own.
<u>Response</u>: We have changed the legend in the figure to define the black curve as actual data.

531. Without any explanation, the focus shifts from combined forcings to CO2 forcing alone.

The reader needs a “sign post” for orientation. Are forcings from all agents other than CO2 going to be neglected in scenarios? Is this reasonable? I suggest that the content from lines 617-619 needs to be moved here, along with a bit more elaboration, to make this transition understandable.
Response: We agree and have modified the paragraph accordingly.

537. The focus now shifts back to combined forcings (Fig. 8). To avoid the double shift, I suggest moving this paragraph ahead of the previous one.
Response: We have switched these two paragraphs, as suggested.

558. Similarly, it’s potentially confusing that the term “scenario” now shifts meaning from emissions scenarios to atmospheric CO2 simulations. I suggest: “Figure 10a shows the simulated changes in atmospheric CO2 for the baseline emission cases (Figure 9a). These cases do not include active CO2 removal. Five additional cases that include CO2 removal are shown …”
Response: We have adopted essentially the suggested rewording.

606-608. If true, it would help to state that F(t) includes volcanic forcing from a base that assumes zero volcanism.
Response: Equation (1) does not imply assumption of a zero volcanism base. Instead it assumes that the volcanic aerosol forcing over the period prior to 1850 was a constant equal to the average volcanic aerosol forcing in the period 1850-2015 (– 0.3 W/m^2). Volcanoes that occurred prior to 1850 had a constant cooling effect up until 1850 (0.225°C, if climate sensitivity is 0.75°C per W/m^2), but as time goes on the cooling effect of the pre-1850 volcanoes gradually decreases to zero as volcanic cooling is gradually taken over by the time dependent volcanoes after 1850).

For clarification, we have inserted the following sentence at former line 608:
The assumed-constant pre-1850 volcanic aerosols caused a constant cooling up to 1850, which gradually decreases to zero after 1850 and is replaced by post-1850 time-dependent volcanic cooling; note that T(1850) = 0°C.

721. Better to avoid expressing extracted amounts in ppm, which may confuse.
Response: We deleted the extracted amount in ppm.

957.. why “but”? Context would fit better with a “moreover”
Response: We think “but” fits better – probably the sentence was misread by the reviewer.

1080. The year should be 2013.
Response: date has been corrected.

1081. TCR(t) is lacking a definition. As defined in AR5, Chapter 10, TCR doesn’t depend on time, so the formulation appears to differ from AR5. If so, some further explanation of the method is needed.
Response: We have removed the time dependence (t) and defined TCR at doubled CO_2 (1%/year CO_2 increase) and TCRE at 2100 as in AR5, Chapter 10, so this section now reads:

A2. Transient Climate Response to cumulative CO_2 Emissions (TCRE)

The transient climate response (TCR), defined as the global warming at year 70 in response to a 1%/year CO_2 increase, for our simple Green's function climate model is 1.89°C with energy imbalance of 1.52 W/m^2 at that point; this TCR is in the middle of the range reported in the IPCC AR5 report (IPCC, 2013). We calculate the transient climate response to cumulative carbon emissions (TCRE) of our climate plus carbon cycle model as in Section 10.8.4 of IPCC (2013), i.e., TCRE = TCR × CAF/C_0, where C_0 = preindustrial atmospheric CO_2 mass = 590 PgC and CAF (t) = Catm/Csum, Catm = atmospheric CO_2 mass minus C_0 and Csum = cumulative CO_2 emissions (all evaluated at year 2100).

We find TCRE = 1.54°C per 1000 PgC at 2100 with constant emissions (which yields cumulative emissions of 1180 PgC at 2100, which is near the midpoint of the range assessed by IPCC, i.e., 0.8°C to 2.5°C per 1000 PgC (IPCC, 2013). Our two cases with rapidly declining emissions never achieve 1000 PgC emissions, but TCRE can still be computed using the IPCC formulae, yielding TCRE = 1.31 and 1.25°C per 1000 PgC at 2100 for the cases of −3%/year and −6%/year respective emission reductions. As expected, the rapid emission reductions substantially reduce the temperature rise in 2100.

Reviewer #3

Hansen and colleagues have appropriately taken into account my comments and concerns in this revised manuscript. I have no substantial issues to raise.
Below I list a few final comments and clarifications, which could be taken into account by the authors when preparing their final submission. These are so minor that they do not require a further review round from my side.

1) Line 152: Some spaces and a period seemingly without any sense or purpose have crept into this line.
Response: We have fixed this typo.

2) Line 801: "out" seems to be missing
Response: Yes, we have inserted "out".

3) Line 841: It has equally be shown based on historical data that nuclear deployment in the past has shown "negative learning" with costs increasing over time instead of being reduced. It seems reasonable to state both sides of the story:
http://www.sciencedirect.com/science/article/pii/S0301421510003526
Response: Yes, that is a good point. Actually the Lovering et al. (2016) contains the French (rising) cost data (including reference to the Grubler paper) and nuclear construction cost histories for all nations with available data. To include a proper balance we have changed:

" and it has been shown that reactor costs stabilize or decline with repeated construction of the same reactor design (Lovering et al., 2016)." to "Although in some countries reactor costs stabilized or declined with repeated construction of the same reactor design, in others costs have risen for a variety of reasons (Lovering et al., 2016)."

[Note that the main thing the Lovering et al. paper shows is that negative learning (which has occurred in a number of places) is not intrinsic to nuclear energy, but a result of governance, macroeconomic, and other factors. South Korea is a good example of positive learning, and China probably is too, but they have not made adequate data available. Also note that costs in France actually were quite stable, growing less than the general construction index.]

4) Line 1150: A paper, only published two weeks ago, reconciling controversies about the hiatus might be a good additional reference here:
https://www.nature.com/nature/journal/v545/n7652/full/nature22315.html
Response: Yes, it is a very good summary reference – we added it at two points.

5) Comment on Figure 13: With energy accounting method I was hoping to clarify whether the partial substitution method or the physical energy content methods was applied (see: https://www.iea.org/statistics/resources/questionnaires/faq/#one) as this can strongly impact the perceived share of renewable energy in the primary energy mix. It would still be useful to clarify this in the caption. BP seems to use the partial substitution method if I interpret footnote * on page 38 of this document on the BP statistical review correctly:
http://www.bp.com/content/dam/bp/pdf/energy-economics/statistical-review-2016/bp-statistical-review-of-world-energy-2016-renewable-energy.pdf

Response: Agreed, BP uses the substitution method for energy accounting – this has been added to the Fig. 13 caption with reference to Macknick (2011) for explanation of energy accounting.