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Earth System Dynamics Discussions

Interactive comment on "Young People's Burden: Requirement of Negative CO_2 Emissions" *by* James Hansen et al.

Anonymous Referee #2

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This paper is an update to Hansen's evolving narrative about anthropogenic climate change and implications, which in recent years has been put forth mostly in grey literature. It is certainly an unusual paper, with its enormous scope, from detailing observational records to evaluating remedial actions. In effect, it is attempting a synthesis with same breadth as all three working groups of the IPCC but from a personal perspective. Is this useful? Is this acceptable practice? I think the answer to the first question is clearly yes. This paper will make a useful entry point for appreciating the full scope of the problem, including providing mechanistic insight. Certainly the IPCC reports or their summaries don't succeed well in this task. The answer to the second question is "maybe", but only if the paper is reviewed across its full scope. As a scientist, I can review the science aspects. But I'm somewhat concerned that my review – from this perspective alone – might lend credence to conclusions about policy which may not be

sound. To help address the need for broad review, I've taken the liberty of also sending the draft to a colleague with somewhat different expertise than my own, although still from a scientific perspective. I suggest that further review from a policy perspective may be warranted. I've appended this colleague's review to the end of mine, below.

General points: (1) An endpoint of year 2100 is taken for future projections. I guess this is still standard practice, but limits the ability to make key points, such as the extent to which some scenarios commit to further warming "in the pipeline". I would suggest that an endpoint of 2150 or 2200 would better frame the discussions. Too late to change? I'd urge that this be given some thought.

(2) The topic of slow feedbacks crops up at various times in the paper, each time as a bit of an aside. I sense that the issue of the further risk associated with flow feedbacks needs some space in the introductory sections of this paper.

Specific points:

36: "the current generation" Strange term for a science paper. Might be conflated with electricity generation.

160-162: This claim needs support. Might be better to cut because the digression to support the claim would be distracting and the point is anyway of secondary importance.

175-179: The 1998 El Nino set a record that was only barely broken for the next 18 years until this most recent El Nino event in 2015/2016. It seems perfectly reasonable to assume current El Nino will set a record that will last similarly long. If so, then the linear projection assumed here will almost certainly overestimate warming over the next few decades. For balance, a bracketing lower bound fit is needed. The simplest alternative might be to include data only through 2014 when making the linear fit.

193-198. This sentence is too long and clumsy. Also, it sets up an irrelevant straw man involving the claim that the current year might be warmest of the entire Holocene.

198-206: The point of this sentence does not come through for me. In particular "these smoothed temperatures are relevant to important climate features" Which temperatures? Relevant mechanistically or statistically?

207: "because of issues such as discussed" It would be better to spell out briefly what these issues are.

193-212: Underlying this discussion is the need to compare recent decadal temperatures with century-scale averages for the Holocene. To do this rigorously requires a measure of recent warming that has 100-year significance. The need for this has been glossed over in this discussion. Creating such a measure should be doable, e.g. by allowing uncertainty for expected natural decadal variability. At least the need should be laid out to motivate future work with appropriate caveats offered.

233: 0.7C is damn cold! Must be an anomaly, but relative to what?

248. There's too much rounding in this estimate of "not much more than 1 C". Should be computed by combining mean and sigma estimates to 0.1C precision for Eemian relative to Holocene and Holocene relative to 1880-1920.

251: This point appears to be adequately backed by the preceding discussion without reference to the fast vs slow feedback question. Better to make it this way first, and then only add the slow/fast issue as an additional layer, e.g. to be discussed further below.

339-340: Unless I missed it, this is the first mention of a temperature record with El Nino/La Nina removed. Nothing about this is mentioned in the caption to Figure 2, and by eye the ENSO events appear not to have been filtered in the data shown. Needs clarifying.

352-353. Change in vegetation cover should also be included in the laundry list of slow feedbacks.

354-356: This states a plausible theory as fact. Needs rewording, e.g. "they are a

favored explanation for why.."

359-261: Not sure this point about slow feedbacks comes across clearly. I sense this is a framing concept that might better have been developed earlier in the text. See my general comment above.

470: typo: "or"

Figure 9: The y axis introduces the symbol Delta Fe, which has not yet been defined.

510-521 and Figure 11. This is hard to follow and has some inconsistencies. It would help immensely to show the extracted amounts as time series in Figure 10 in Pg/yr or equivalent. Note the community now uses the cgs unit Pg instead of GtC. The text says the extraction starts in 2010 but the caption states 2020. The extracted amount would be better expressed in Pg (or GtC) instead of ppm to avoid seeding unnecessary confusion as to why the extracted amount exceeds the change in atmospheric CO2. It's more useful anyway to express captured amounts in the same units as emissions. Fig. 11 caption needs to clarify what year is used for reporting the cumulative amount captured.

541, Eq. (1). There is a notational problem with Eq. (1). The t on the right-hand side cannot be same as the t on the left. The integration needs to be over t', and the limits of integration need to be spelled out. I'd expect kernel to be a function of t' or of t-t', depending on how t' is defined. It should not be a function of t. Figure 1 Why doesn't the long-term average stratospheric aerosol forcing center on zero? The treatment appears to take the natural stratospheric aerosol background as a forcing, which is hard to reconcile with the definition of a forcing as a perturbation from a natural state.

617-618 and Section 9. Again, it would be preferable to express the extracted amounts consistently in Pg C rather than ppm.

818. Better to break the sentence before the "e.g." and remove the "e.g.".

Separate review by colleague:

1) The reviewed article by Hansen and colleagues provides a broad and coherent synthesis of observed and predicted warming of the climate system that is highly relevant to current policy targets and discussions. The paper illustrates (and frames) very well the reality and severity of required CO2 emissions reductions and atmospheric CO2 extractions required to maintain a "safe" climate. Though lengthy, the paper is clearly written and is thus accessible to a broad audience.

2) A major criticism is that the paper relies heavily on global surface mean temperature as a benchmark against which past, current and future climate states are compared or deemed safe or "dangerous". One weakness with this approach is that the paper and its methodology does not address (enough) the uncertainties surrounding temperature changes, whether in the paleo proxies interpretation of Eemian and Holocene climates (error bars in Figure 3b are unsatisfactory), or in future predictions from the simple green function model used therein. A possible way to remedy this, in the context of paleo proxies for instance, is to add various timeseries from different studies overlaying the mean for the Holocene era.

3) On a related note, the paper argument to reduce emissions below certain thresholds (e.g. 350-450ppm or 0.5-1°C) rests on using the holocene range as a benchmark for safe climate. The comparison to the Holocene's temperature variations is well described. The authors, for instance, recognize differences in smoothing (line 195) between the centennial window and interannual/decadal window of modern warming and their implications for the climate system. However, it is not convincing that an additional 0.5°C to 1°C increase from the Holocene range (e.g Paris target of 1.5-2°C by 2100) will lead to catastrophic consequences without detailed analysis of climate models's predictions, which this paper does not address.

4) The authors frequently reference paleo temperature changes to infer climate sensitivity and to warn against catastrophic climate change. The authors, for instance, use

the Eemian as an analog for a warmer world, its feedbacks, and associated sea level changes. While interglacial periods and deglaciations can be used to understand the climate system and its slower feedbacks, I am not convinced of their use as analogs for modern and future warming or to infer climate sensitivity. First, the forcing is different: one involve short-wave radiative forcing changes (precession, obliquity, eccentricity), whereas the other involves changes in atmospheric GHG composition and consequent changes in long-wave radiative forcing. The type of forcing and governing timescales may thus involve different sets and contributions of feedbacks (fast or slow), with potentially unique time and forcing dependent climate feedback parameters for natural vs anthropogenic perturbations (e.g. for short term natural variability, Xie et al 2016). I thus find the use of the Eemian as an analog for future warming incomplete without a discussion of the limitations of using such comparisons.

5) The choice of 1970 as the starting year to determine the secular warming trend (0.18°C/decade) in Figure 2b is not well justified in the paper, besides from referring to it as "the present global warming rate". The authors elsewhere refer to the longterm warming of 1°C since 1880-1920 (e.g. Lines 172, 220), but specifically chose 1970 to interpolate warming values for 2040 and 2060 (Line 177). In addition to contributions to the hiatus of the 2000's, Meehl et al (2016) show potential contributions of decadal variability during the warming phase as well. While the authors recognize the effects of decadal variability in driving the hiatus in surface warming (Line 170), they do not do the same for the enhanced warming period. Since the warming response is likely non-linear, using a linear trend since 1900 is probably not appropriate, and perhaps the authors may want to use the IPCC model mean instead for assessing future values.

6) The discussion of the hiatus is very brief and lacks in substance, especially given the paper's focus on global mean surface temperature as a proxy for a changing climate. Furthermore, more precise wording should be used when discussing the hiatus, e.g. in lines 163-167, where the term hiatus in global warming is used vs global surface warming. The recent El Niño warming, while shows a nice visual of an increase in

surface warming, is, in my opinion, not an accurate way to reject the “hiatus” argument. Rather, the continued increase in ocean heat uptake during the hiatus period (Roemmich et al 2015) is much more powerful, and is a scientifically accurate argument to show continued energy imbalance in the climate system. Discussing the ocean heat content trend in the hiatus context can prevent future communication blunders, e.g. if a future decrease in surface mean temperature ensues due to decadal variability or other poorly known aspects of climate.

7) The conclusion that the world has already overshot a “safe” climate target can not be taken without great reservations given the paper’s limited analysis of temperature uncertainties and the relation between global mean temperature and the climate system. Though likely true, this statement seems quite subjective and also likely model dependent. From a communication standpoint, it is a bleak conclusion that may lead to no action rather than the authors’s likely intention of motivating urgent changes.

Minor Clarifications, recommendations, and typos:

1) Figure 4: Radiative forcing plot should have uncertainty error bars, as described in paper (line 289-290).

2) Typo in line 190: “ezaggerated"

3) Typo in line 232: there is an extra “(“

4) Line 315: Since the paper is serving as an overview, other estimates of energy imbalance/OHC changes should be listed, including those from cited and possibly also other non-cited references.

5) Line 345: The math is somewhat confusing to me without an equation. Math doesn’t seem to add up when using the aerosol forcing, or I am not understanding well, hence the need to clarify the equation used, which I assume is the energy balance equation C*dT/dt=F+ lambda*T

6) Line 370: The ice sheets “slow” feedback is brought up several times and is a major

motivation for limiting warming beyond 2100. There are for instance several references to ice sheet feedbacks that can drive sea level rise out of “humanity’s control”, with little description of these feedbacks. However, little is detailed about this feedback (mechanisms, magnitude, etc.). It would serve the paper well to have a more detailed paragraph on the ice sheets feedback.

7) Line 374: SRES is not defined.

8) Line 412: The authors suggest the carbon airborne fraction is not expected to continue with larger growth rate emission scenarios. Is there a reference or detailed basis behind this statement.

1. Figure 9: The contribution from CFCs seems quite high, and the drop is (too?) large in ΔF, matching across all gases (CFCs, CO2 CH4 and N2O) which is perhaps too coincidental?

2. Line 483: The authors suggest RCP2.5 requires negative Forcing growth rate 25 years from now, which is confusing. RCP2.5 requires negative emissions not until $\sim$2075. A negative growth rate is required almost immediately. I am not sure how the 25 years came about in the text and in Figure 9b.

3. Line 642: The authors should look into the UNFCCC’s REDD+ program, and how their proposed carbon uptake rates compare to the REDD+ efforts and proposed/future plans.

4. Carbon removal section: Since atmospheric carbon extractions is a major feature of this paper, the presentation could benefit from a summary table or a “wedge” figure (similar to Pacala and Socolow 2005) to summarize CO2 extractions technologies and methods, including costs and feasibility. Current reading through dense text is quite burdensome.

5. Line 695: Long list of proposed benefits but no mention of possible negative impacts of basal dust use.

6. Line 775: The conclusion that future reductions in methane are unlikely is not justified given the speculative nature of the observed recent increase in methane.

7. Line 785 (& Line 570): N2O source from the ocean is not well known either and its future change due to deoxygenation or changes in ventilation rates is poorly known and may have additional or canceling effects on land emissions and feedbacks (Martinez-Rey et al 2015).

8. Figure 12a: There is little difference in temperature outcomes between the 6% and 3% reduction scenarios, which is interesting but barely discussed in the paper. There seems to be an effective rate of emission reduction for large temperature reductions that is not well exploited in the discussion.

References:

Martinez-Rey, J., Bopp, L., Gehlen, M., Tagliabue, A., and Gruber, N.: Projections of oceanic N2O emissions in the 21st century using the IPSL Earth system model, Biogeosciences, 12, 4133-4148, doi:10.5194/bg-12-4133-2015, 2015.

Meehl, G.A., A. Hu, B. D. Santer, and S.-P. Xie, 2016, Interdecadal Pacific Oscillation contributions to twentieth-century global surface temperature trends, Nature Climate Change, published online Aug. 29, 2016, doi:10.1038.

Roemmich, D. et al. Unabated planetary warming and its ocean structure since 2006. Nature Clim. Change 5, 240–245 (2015).

Xie, S.-P., Y. Kosaka, and Y. M. Okumura (2015), Distinct energy budgets for anthropogenic and natural changes during global warming hiatus, Nat. Geosci., doi:10.1038/ngeo2581.

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