Changes made to "Young People's Burden: Requirement of Negative CO_2 Emissions" in response to the reviews.

There were very many substantive suggestions and issues raised by the referees – we thank them for doing such a good job on a rather long paper of broad subject matter. We trust you will find that we have been responsive, and we apologize for the time that it took to make the revisions.

We order our discussion as follows. There were several issues raised by more than one of the referees (R1, R2 and R3, where R3 is the additional review sought by Referee R2 and included as the second half of R2's report). We first discuss three topics that were in common to at least two of the referees. Then we respond sequentially to other comments of R1, R2 and R3.

Related Comments of Referees R1, R2 and R3

The clarifications in response to these common issues should be useful to general readers who are not familiar with the full range of climate change literature. The changes occur especially via introductory paragraphs to Section 2 and an improved Section 4 on Climate Sensitivity and Feedbacks, where Slow Feedbacks are now broken out as a subsection.

Comment #1. Referee R1 notes that the choice of 350 ppm as the end-of-century target for CO_2 is subjective. We have a good basis for the <350 ppm portion of this target, but the end-of-century date for achieving it is more subjective. (Evidence supporting the <350 ppm target includes Earth's climate response to paleo CO_2 changes and modern measurements of Earth's energy imbalance. The latter data allow easy calculation, without use of a global climate model, of the CO_2 change needed to restore Earth's energy balance.) Although the end-of-century date chosen for achieving this reduced CO_2 level is somewhat arbitrary, we cite paleo evidence that sea level change lags temperature change by only 1-4 centuries. We also cite evidence that the speed of the climate system response varies inversely with the magnitude of the forcing, thus the response to the human-made forcing will be faster than the response to slow weak paleo forcings. Therefore to leave excessive human-caused forcing in place for centuries, when paleoclimate data tells us that the ultimate response will be large and undesirable, warns us that we had better try to limit the period of the planet's excursion into dangerous territory. Observations of accelerating ice sheet mass loss and improving ice sheet models add to the warning. It would be nice if we could instantly restore the planet's energy balance, but we are forced to choose a conceivable period and end-of-century seems a good choice. We then investigate how difficult that task would be. So it is true that the time scale chosen to aim for getting the planet back in balance is necessarily somewhat subjective, but there is a basis for this approximate time scale.

Comment #2. This comment mainly concerns criticism 2) of referee R3: "A major criticism is that the paper relies heavily on global surface temperature as a benchmark against which past, current and future climate states are compared or deemed safe or "dangerous".

Actually, in other papers, we have focused on alternative benchmarks, especially atmospheric CO_2 abundance and Earth's energy imbalance. Several authors of our present paper, along with several other carbon cycle and paleoclimate researchers (Berner, Pagani, Raymo, Royer and Zachos), made an analysis ("Target Atmospheric CO_2: Where Should Humanity Aim?, Open

Atmos. Sci. J., 2, 217-231, 2008) focused on the CO_2 level, concluding that the target on the century time scale should be no higher than 350 ppm, and possibly lower.

In addition, several of the present co-authors have collaborated in analyses of Earth's energy imbalance. Restoring planetary energy balance provides the principal benchmark of our resulting paper titled Assessing "Dangerous Climate Change" (PLOS One, vol. 8, e81648, 2013).

R3 is correct that our present paper seems to rely heavily on temperature as the benchmark. That emphasis is perhaps a reflection of the fact that the international community overwhelmingly uses global temperature in discussing targets (e.g., 1.5°C or 2°C). However, our thinking is driven by more than the temperature benchmark, and we should have made that clearer. That is one of the things we now do in the introductory paragraphs in section 2. It is useful to make clear the relation and consistency of temperature, CO_2, and energy balance benchmarks.

Comment #3. Referee R2, as one of his two overall comments, and in later specific points, notes that "slow feedbacks" come up multiple times in our paper, but without an overall framing of their role in affecting dangerous climate change.

Indeed, slow feedbacks are an important matter, and it makes sense to discuss them explicitly near the beginning of the paper. Slow feedbacks affect both the time scale issues (Comment #1) and the appropriateness of temperature for defining dangerous climate change (Comment #2). We try to clarify all these matters by revising Section 4 (Climate Sensitivity and Feedbacks) into what we believe is a much clearer discussion of climate sensitivity and fast and slow feedbacks.

Referee R1

Referee #1 General comments:

1. We have changed the punctuation and reference style to those used by Earth System Dynamics. The several papers that were cited in our original manuscript, but were missing from the References, have been added.

2. Regarding the uncertainties in climate sensitivity and aerosol climate forcing: (1) we now discuss the uncertainty in climate sensitivity in the revised Section 4, (2) we have changed Section 10, which had been only on non-CO_2 GHGs, to include a subsection on aerosols, including discussion of likely future aerosols.

In brief, we suggest that uncertainty about equilibrium climate sensitivity is not a major issue for this paper. Our paper is focused on the amount of CO_2 that must be removed from the air to keep global temperature close to the Holocene level. Global temperature is already out of the Holocene range with the present 400 ppm of CO_2 and more warming is in the pipeline. The amount of CO_2 that must be removed to restore planetary energy imbalance depends mainly on the magnitude of future CO_2 emissions, not upon climate sensitivity or upon calculated future global temperature for emission scenarios with high future emissions.

However, our discussion about climate sensitivity is improved, we believe, and our simulations are described well enough that they could be repeated with alternative climate sensitivities.

Aerosols are a significant source of uncertainty, as Referee #1 notes, and a key factor in geoengineering discussion, which is the next topic raised by Referee #1. We have added discussion of aerosols as noted next.

3. Referee #1 asks if we see a role for geoengineering to help keep climate within the Holocene range. This topic can lead to protracted discussion and be a distraction – and the paper is already long. However, humans are already geoengineering the climate by altering GHGs and aerosols, so we need to say how the other forcings alter the CO_2 requirements. Thus Section 10 is now divided into two parts (10.1 Non-CO_2 GHGs and 10.2 Aerosols and Purposeful Climate Intervention), but we minimize the latter discussion.

Referee #1 Specific comments:

L32-33: To account for concern about the susceptibility of soil carbon to human interference, we altered the sentence in the abstract to make it less definitive. Also the relevant text (Section 9) expresses cautions more clearly.

L59: We now note that the CO_2 target is also subject to some adjustment depending on the level of other trace gases and aerosols. We include quantification of this in Section 10 (a permanent reduction of some other forcing by 0.25 W/m^2, e.g., allows the CO_2 target to change from 350 ppm to 365 ppm).

L173-174: Regarding the 12-month running means: with each new month of data we compute a new 12-month running mean, which is simply the average of the most recent 12 monthly temperature anomalies. Thus with the present data, which run through November 2016, the final point is the mean anomaly for December 2015 through November 2016.

However, we agree that it may be confusing that there appears to be a 6 month shift on the x-axis in the way that points are plotted in Fig. 2(a) and 2(b). Therefore we have changed Fig. 2 so that the vertical grid line represents January 1 of the year in question. Thus in Fig. 2(a) the square data point for 1880 is now centered 6 months to the right of the grid line. This location thus now coincides with the location of the 12-month running mean (January 1880 through December 1880). The merit of Fig. 2(b) is that it gives 12 meaningful points each year, among other things providing an early indication of the annual mean and a smoother curve that better defines the temperature anomalies associated with events such as a volcanic eruption or El Nino.

L262-264: Regarding the potential CO_2 released by melting permafrost: this is just one of several examples of why we argue that the target global temperature needs to be in or near the Holocene range. In that case the hypothesized CO_2 injection from permafrost does not occur or is small. It is also a reason why the period of overshoot of global temperature, i.e., the period in which temperature remains well above the Holocene range needs to be minimized. This matter is included in the topic "slow feedbacks", which R3 suggests we should discuss more explicitly. We have done so in the new revised section on climate sensitivity and fast and slow feedbacks, as described in our response to R3.

L270: Regarding the consistency of the radiative forcings (RFs) in Fig. 4 and Table A1: yes, these are consistent, and the GHG forcings are very close to the values used by IPCC. Our GHG

RFs (as stated in the paper) are calculated from formulae in the Hansen et al. (2000) paper using GHG time series that have been used in the GISS (Goddard Institute for Space Studies) global climate models for the past two decades and continually updated. Some of the GHG amounts in the period prior to air measurements of gas amounts differ slightly from IPCC time series, probably because of the use of different ice core data sources, but differences with IPCC are negligible. For example, the GHG forcing in 1850 relative to 1750 with the GISS GHG time series is 0.253 W/m^2. IPCC tables have 0.255 W/m^2. The nearly exact agreement is coincidental, as forcings by individual gases differ by as much as ~0.02 W/m^2, but differences of that magnitude are also negligible compared to today's GHG forcing, which exceeds 3 W/m^2.

Regarding the question of whether we should redo our calculations, starting them at 1750 instead of 1850. That would be a lot of work and would not seem to add anything of significant value. Certainly it does not affect the issues that our paper is aimed at, the burden of CO_2 that must be removed from the air this century if we wish to stabilize climate. A problem with 1750-1850 is the absence of data of useful accuracy, for both the climate forcing and climate response. IPCC attempts to define the climate forcing for 1750-1850, but it is really a guesstimate at a small number. They suggest a net forcing of about 0.1 W/m^2, as it is presumed that the GHG forcing of about 0.25 W/m^2 was offset by increasing atmospheric aerosols during the beginnings of the industrial revolution, but there are no measurements of atmospheric aerosols that would allow the aerosol forcing to be calculated. One might even wonder whether their estimated net small positive climate forcing is not more a reflection of their realization that there seemed to be a slight warming, of order ~0.1°C, over that century. But the temperature change over that period is also very uncertain.

L283: The referee notes that it is too vague to say that the RF used for our global temperature calculation is "similar" to the forcing of IPCC, and that our CO_2 forcing is about 10% greater than in AR5. The IPCC radiative forcings are for 1750-2011, while ours are for 1750-2015. For the same period (1750-2011) and CO_2 change (278 to 390.5 ppm) our formulae yield a forcing 6.7% larger than the IPCC value (1.816 W/m^2). For the same period our formulae yield a forcing 3.03 W/m^2 for the sum of all well-mixed (long-lived, i.e., excluding stratospheric water vapor and ozone), which is 7% larger than the IPCC central value but well within their range (2.54-3.12 W/m^2). The forcing depends not only on the radiative properties of the gases, but also on the global distribution of clouds and other atmospheric complexities. Our formulae are based on calculations with a global climate model with parameterized radiation in which the gaseous absorption has been fit to line-by-line radiation calculations, so we believe they are accurate, but in any case the result is within the range estimated by IPCC. We summarized the comparison to IPCC in a short footnote in Section 3, copied here:

[1] Our GHG forcings, calculated with formulae of Hansen et al. (2000), yield a CO_2 forcing 6.7% larger than the central IPCC estimate [Table 8.2 of Myhre et al. (2013)] for the CO_2 change from 1750-2011. For all well-mixed (long-lived) GHGs we obtain 3.03 W/m^2, which is within the IPCC range 2.83 ±0.29 W/m^2.

L415-425: As suggested by referee #1, we have reorganized the discussion of methane, moving some of the discussion from section 10 to here (section 5), so section 5 now discusses methane observations and interpretations of changing methane amount and section 10 discusses the potential for reducing climate forcing via slowdown of methane emissions. The additional 2016 references noted by the referee were helpful in clarifying the status of knowledge.

L417: Regarding the Turner et al. paper, which suggests that fossil fuel mining is introducing new CH_4 emission sources in the U.S.: we do need to mention this topic, because there is a great deal of concern that "fracking" for natural gas is a new emission source, so it would be odd if we totally ignored the topic. However, as the referee notes, other recent papers show that the total fossil fuel source of CH_4 is not growing, which we have now clarified.

L624: Referee R1 suggests that we provide more information on the economics, e.g., cost comparisons. SC4 (M. Beenstock) also suggests more economics discussion, e.g., reference to the Stern Review. Indeed, the net cost of action or inaction on fossil fuel emissions is an issue of overriding practical importance. We cannot convert our paper into an economics paper, but we can provide a better discussion along the lines suggested by the reviewer and commenter.

The Stern Review is a useful reference because it frames the discussion in broad terms. In reaching its overall conclusion, that the future cost of inaction on emissions now is likely to exceed the cost of action, the Stern report recommends use of a much smaller "discount" rate in calculating the cost of future climate impacts, which is relevant to our concern that assumptions about future negative emissions can place an enormous and perhaps unrealistic burden on today's young people. A less discussed but important conclusion of the Stern Review is that the cost of emissions reduction, if measured in terms of its effect on GDP, is uncertain even as to its sign, i.e., it is possible that the actions to reduce emissions could increase overall wealth even though they may depress certain economic sectors.

We have revised and clarified Section 9 along the lines that R1 appears to suggest, including an example calculation comparing the cost of energy infrastructure changes with estimated costs of extraction of CO_2 from the air.

L668-676: R1 points out a useful paper by DeCicco et al. that exposes the likely high carbon costs of the U.S. biofuel program for vehicles. That biofuel program is tangential to (and even in opposition to) the potential "improved agricultural practices" that we discuss, and we would be remiss if we did not comment on this, so we added a short paragraph just before Section 9.1.

L715-719: Indeed, for the purpose of showing that the cost of CO_2 extraction will be high if high emissions continue, there is no need to assume that "improved agricultural and forestry practices" will occur to the extent of 100 PgC CO_2 drawdown. To whatever degree they fail to reach that level, it increases the amount of CO_2 that will need to be extracted via "industrial extraction" and thus would increase the cost estimates. We have reworded the text accordingly.

L753-762: We agree with the comments re the methane discussion, and largely moved this discussion to the earlier section, including several new references (see comments re L415-425).

L822-823: Ah, we meant from coal to gas, of course – we have fixed that.

L994: The (relatively small) differences are due to forcing changes between 1750 and 1880, as discussed with regard to the comments on L270 and L283.

Edits: Thanks -- we fixed all of these edits.

Referee R2

General point (1). Referee R2 suggests that the results would be more useful if we had extended the temperature projections beyond the standard endpoint of year 2100. It is not too difficult to extend the global temperature calculations, using simple assumptions for how the radiative forcing scenarios extend beyond 2100, so we have done that, extending the calculations to 2200, as discussed in the text describing Figure 13.

General point (2). Referee R2 observes that the topic of "slow" feedbacks crops up at various points in the paper, often seemingly as an aside.

Indeed, this is an important point. Slow feedbacks are crucial to our analysis, the ice sheet feedback and resulting sea level rise largely leads to our low target for global warming. And the GHG feedback makes the task of reversing GHG buildup all the more difficult. As R2 suggests, the slow feedback topic needs to be more explicit and written more clearly up front.

We now include mention of slow feedbacks in our outline of the paper in [1. Introduction]. Full discussion of slow feedbacks, while still "up front," needs to occur somewhat later, because we need to start with [2. Global Temperature], which is the principal metric of climate change, and proceed via [3. Global Climate Forcings and Earth's Energy Imbalance], which are the fundamental mechanisms of global climate change. These sections are a necessary introduction to [4. Climate Sensitivity and Feedbacks]. Revised sections 1-3 are as in the original paper, except that we have addressed several specific issues raised by the three referees. Section 4 is split into subsections [4.1 Fast-Feedback Climate Sensitivity] and [4.2 Slow Feedbacks].

We believe that this explicit organization of section 4 with discussion of slow feedbacks in its own subsection makes the paper clearer, especially for readers who do not have extensive background in climate studies, and we thank the referee for his/her suggestion. Slow feedbacks are a very large part of the story, the principal reason that it would be dangerous to allow global temperature to long remain far outside the Holocene temperature range.

L36: Agreed, "current generation" is ambiguous. We have restated to eliminate that terminology.

L160-162: Now we have 11 months of data for 2016, rather than 8 months, so it is now clear that the 2016 global temperature is easily the highest in the period of instrumental data.

L175-179: Yes, we agree that future temperatures can be expected to fall both below and above the linear trend line. We can also say with confidence (this relates more to a global temperature issue raised by R3) that the approximate warming trend will continue, because Earth's energy imbalance is the immediate drive for continued warming (rather than annual growth of climate forcings, although the annual growth has some effect on the imbalance). We agree that we should not let extreme endpoint values cause a misleading trend; when we exclude the final year the calculated trend (0.176°C/decade) still rounds to 0.18°C/decade).

L193-198, L198-206, L207, L193-212: Agreed on both points re L193-198, i.e., the sentence is too long and the point being made in the first half of the sentence can just as well be omitted. R2 raises several other valid points about this paragraph. So we have rewritten it in a way that gets to the point and avoids tangents. Also we explicitly mention the issue (seasonal bias in the proxy data) raised by Liu et al. (2014). The question about the significance of the current (smoothed, via linear trend) temperature is better put not as one of statistics, but as a matter of physics. Earth's present (substantial) energy imbalance makes it implausible for the mean temperature over the next several decades to be less than the present (smoothed) temperature.

L233: Of course we meant a temperature anomaly of +0.7°C (relative to preindustrial temperature), not an absolute temperature – we have corrected that error.

L248: Agreed that Eemian temperature is important for assessing the dangerous level of warming, and although we are relying on analyses made by others, we have now made this discussion clearer and have added one more reference.

L251: Yes, it seems unnecessary to bring up slow feedbacks at this point – so we have eliminated that statement.

L339-340: What we meant was that the present temperature based on the linear fit is more appropriate than the actual current temperature, which is elevated by the 2015-16 El Nino. We have changed the wording so that there is no impression that we may have done something more formal such as subtracting out a standard El Nino signal.

L352-353: We have added vegetation feedback in the new subsection (4.2) on slow feedbacks.

L354-356: Well, we might argue that the changes of GHGs and surface albedo are more than a favored explanation. Given a fast-feedback climate sensitivity ~3C for 2xCO_2 these boundary forcings account for practically the entire glacial-interglacial temperature change. The results fit well the temperature variations over the entire 800,000 year period with ice core data for GHG amounts (and with ice sheet size inferred from sea level information).

However, in responding to R2's earlier suggestion, we revised this section into "fast feedback" and "slow feedback" subsections, and the referenced sentence was eliminated. Slow feedbacks and their significance are now discussed in a more organized way, which we hope is clearer and a major improvement, because of the importance of slow feedbacks.

L359-361: This is fixed via the new subsection on slow feedbacks.

L470: Typo fixed.

Figure 9: ΔFe, the annual increase of the GHG climate forcing, is now indicated in the caption.

L510-521 and Figure 11: O.K., we have switched from GtC to PgC, and everywhere we give extracted amounts in PgC, not ppm. Fig. 11 fixed to show that cumulative amount is in 2100.

L541: This comment raises two points: notation in equation (1) and the way we have treated the volcano/stratospheric aerosol forcing in the Green's function calculation. The notation problem is easily solved: yes, we should use t' for the functions under the integration sign. Also we have added the integration limits (from 1850 to time t); it is hard to make this look right using our "typewriter", so we will need to check what the typesetter does.

How to handle the effect of intermittent volcanic aerosols is a nuisance for modelers. The ocean temperature at the beginning of the run (in the case of a global atmosphere-ocean model) is the difficulty, because the ocean temperature is dependent upon the prior history of stratospheric aerosols. With atmosphere-ocean GCMs the proper (but expensive) procedure is to have a long spin-up run with volcanoes sprinkled in time at a climatologic amount (or a constant mean amount), but this is often not done. If the spin-up does not include volcanoes but they are included in the transient climate experiment, they have an inappropriate long-term cooling effect. The mean volcanic aerosol forcing over 1850-2000 in the Sato et al. data set is about -0.3 W/m^2.

So what referee R2 is implying is correct: by in effect ignoring volcanic aerosols in the time preceding 1850, the volcanoes after 1850 have a long-term cooling effect. As t goes to infinity the average cooling for -0.3 W/m^2 is about 0.2°C, which is not negligible.

It is easy to include a term in the Green's function calculation to avoid an inappropriate long-term aerosol cooling effect, if we assume that volcanic aerosols prior to 1850 had average forcing over time of -0.3 W/m^2. That's an approximation, but there is no way to be exact without knowing the specific history of volcanoes before 1850. The effect is not very obvious in the graphs, but we have added that term and we are glad that the referee forced us to think about this.

L617-618: Yes, we have changed GtC to PgC everywhere (we use PgC, rather than Pg because we want it to be clear that we refer to the mass of C not the mass of CO_2).

L818: We have made the suggested change.

Referee R3

Item 2) [Item 1) did not suggest changes]: Item 2) concerns a major criticism that the paper relies heavily on global mean surface temperature as a benchmark against which past, current and future climate states are compared or deemed safe or dangerous.

This criticism was initially surprising, as the first author has written several papers that explicitly or implicitly make that very criticism. It is true that we superficially reverted here to use of global temperature as the benchmark, but that was because of reliance on that benchmark in all of the international discussions and the great publicity surrounding 2°C or 1.5°C targets, so the broader public and scientific audiences seem to be familiar with it, if not totally reliant on it.

However, this is a very useful criticism, because it reveals that we did not do a good job of defining the rationale of the paper, so some revisions are required. Before defining those let me note two other benchmarks that we have relied on and at times have asserted that they are superior in certain respects.

One of these is atmospheric CO_2 amount, or CO_2 equivalence to include all greenhouse gases (GHGs). Several of the present authors were co-authors on a 2008 paper: "Target atmospheric CO_2: Where should humanity aim?", which provided the scientific basis for the organization 350.org. One big advantage of CO_2, over temperature, is that it only requires measurements at one point to know the relevant global amount. Thus, especially for the last 800,000 years for which precise ice core measurements are available, it provides a very precise benchmark.

Another benchmark on which several of the co-authors have spent a lot effort is Earth's energy imbalance. A great merit of this benchmark is that it integrates over all climate forcings, known and unknown, and tells us something very valuable about where climate is heading in the relatively near-term. If we measured Earth's energy balance at only one point in time, it would be useful, but now it is much more powerful as we have rather accurate data for more than a decade, and useful data for several decades. For example, the data show that since the growth rate of GHG climate forcing jumped up about half a century ago and has remained high (as forcing additions from CFC and CH_4 declined, CO_2 increased) Earth's energy imbalance has remained high, driving continual global warming since ~1970. Thus we can be confident that on decadal time scales global temperature will continue to rise, because there is much more energy coming in than going out.

So why did we choose to focus on global temperature in this paper? One reason is for the sake of communicating with a broader audience, both scientists and non-scientists. A problem with using temperature is that it requires introducing climate sensitivity into the story. However, that can be done in a reasonably simple and transparent way by use of the simple Green's function approach (for converting climate forcing into temperature) with a canonical climate sensitivity of 3°C for doubled CO_2. A second reason for the temperature metric is that our paper emphasizes the effect that "slow feedbacks" have on determination of the dangerous level of human interference with the climate system. For the sake of understanding when slow feedbacks kick in or become substantial, global temperature is a very helpful metric, as we now discuss better in the revised paper.

Nevertheless, we are not really relying on temperature as a sole benchmark – we are actually aware of and using these several benchmarks, and relating them to temperature. That is why, e.g., at one point we insert a "consistency check" to verify whether these different benchmarks are consistent in yielding the same conclusion, i.e., we check whether the changes over the industrial era of climate forcings and temperature are consistent with the energy imbalance and the canonical climate sensitivity.

What we have tried to do in revision is explain our approach better. We much appreciate the criticism, because it is an important matter that needed to be made clearer. If we relied on temperature alone, the uncertainties in temperature measurement would strongly restrict our conclusion. However, we argue, and we believe that we have presented substantive support for the argument, that even a (fast-feedback) 1.5°C warming left in place for centuries would be dangerous, in part because it would drive slow feedbacks.

We have borne the criticism about the temperature metric in mind in revising the paper. Two places where we have tried to explicitly make our approach clearer are (1) an added paragraph in the introduction, as the second paragraph, that discusses the multiple metrics, (2) an added paragraph in temperature section, as the second paragraph of that section.

Item 3): This comment concerns the question of whether we show that 450 ppm (as opposed to 350 ppm) of CO_2 or additional warming of 0.5-1°C constitutes dangerous climate change. This topic relates very much to item 2) above. Perhaps if one considers only the CO_2 metric or only the temperature metric, the case for 450 ppm, which would be consistent with 1°C additional warming, being dangerous, does not seem as clear. We believe that our analysis using multiple metrics strongly makes the case that it would be dangerous.

There is another important point that reinforces the danger of such additional climate forcing: namely the fact that the temperature analysis that we make with a Green's function calculation, as well as the temperature analyses made by the IPCC studies with sophisticated general circulation models, are basically fast-feedback climate models. It is good that Referee R2 suggested that we keep this as a separate point, because it really is additional. If global climate is allowed to reach 1.5-2°C, which we make clear is far above the Holocene range, it means that we (humanity) are unleashing the slow feedbacks that inevitably will occur in response to that temperature elevation above the Holocene range. Those slow feedbacks, which include GHG increases associated with global temperature increase (presumably coming from melting permafrost, warming soil, warming ocean, etc.) as well as melting ice and rising sea level. These slow feedbacks not only make it harder to control atmospheric composition and limit global warming, they (ice sheet melt especially) also carry impacts that could be detrimental.

In our revision we have tried to make clearer the limitation and significance of the fast-feedback temperature analysis, as well as the implications of the slow feedbacks.

Item 4): This comment concerns the applicability of paleoclimate to inferences about climate sensitivity. For sure the (orbital) mechanisms that <u>initiate</u> glacial-interglacial climate change are very different than the human-caused climate forcings. However, as all climate models and paleoclimate analyses show, the orbital (insolation) forcings are not the mechanisms that maintain the quasi-equilibrium paleoclimate states. The two dominant mechanisms are changes of atmospheric GHGs (and, to a lesser extent, atmospheric aerosols) and changes of the planetary reflectivity (due especially to changes of ice sheet area, but also vegetation cover). These mechanisms account for most of the paleo global temperature change, and sufficiently good knowledge of those changes exists to allow useful empirical derivation of fast feedback climate sensitivity. However, we must admit that we did a poor job of describing this topic, with regard to both fast feedbacks and slow feedbacks. In revising the section on feedbacks, we believe that we have much improved the clarity of the fast-feedback portion, as well as adding an explicit subsection on slow feedbacks, as suggested by R2.

Item 5): This comment is also very useful. R3 notes that we have not justified why we calculate the linear trend of global temperature from 1970 to the present, rather than beginning at some other date. On the one hand, the period chosen is the time of steep temperature increase, and it is worth knowing the warming rate in that period. However, there are better reasons, and we should have given those. First, 1970 is when the growth rate of GHG climate forcing reached a rate such that it overwhelmed other variable climate forcings, as we show in a later figure in the paper (Fig. 9). Second, as a result of the rapidly and continuously increasing GHG forcing, it is also the time when Earth became substantially out of energy balance, as revealed by analyses of ocean heat content. It is this energy imbalance that is the immediate cause of continued global warming. As long as the imbalance continues to be large (in the range 0.5-1 W/m^2) we can expect global temperature to continue to rise at a similar rate, even though there is short-term dynamical variability. We have clarified the discussion of the temperature trend accordingly.

Item 6): We agree that Earth's energy imbalance, which is practically determined by the continual growth of ocean heat content, is the more relevant measure of global warming and have clarified the discussion in that regard. Also we changed "global warming" to "global surface warming" to be more technically correct.

Item 7): We believe that the revised paper now makes clear that the conclusions regarding possible overshooting safe targets are based on much more than the temperature. Uncertainties in temperature data are indeed one of the reasons that more metrics are needed.

R3 raises an important point in the last sentence of item 7): the possibility that "bleak" conclusions lead to no actions rather than motivating urgent changes. Our opinion is that the actions needed to rapidly slow fossil fuel emissions actually make sense on a number of grounds, and have multiple co-benefits, so we have tried to make this point clearer. For example, other reviewers asked for some cost estimates of mitigation actions for comparison with the estimated costs of CO_2 extraction, and that comparison is favorable in the sense that costs of slowing down emissions are likely much less than trying to clean the atmosphere later.

Minor clarifications, recommendations, and typos:

1) Figure 4: the relevant uncertainties for this paper are for the net GHG forcing and the aerosol forcing, and the combined GHG + aerosol forcing. It is hard to show those on this diagram (and creates more distraction than relevant illumination), which is intended to provide an indication of the relative contributions of different forcings, but there is an excellent presentation by Myhre et al. (2013), i.e., the radiative forcing chapter of the last IPCC report – so we have added a comment to the figure caption and reference to Myhre et al. (2013).

2) and 3): These typos have been corrected.

4): the suggestion to find other references for current estimates of Earth's energy imbalance was useful, as there are three 2016 papers published or in press, including up-to-date corrections of instrumental biases in the ocean heat content measurements. These references are now included; their results are consistent with the range that we stated.

5): No equations are used in this check on the consistency of observed warming with the climate forcing and remaining planetary energy imbalance. The only thing required is the proportionality constant between forcing and temperature change, i.e., climate sensitivity, which is 0.75°C per W/m^2. Nevertheless, it should now be clearer, because of the more explicit discussion of climate sensitivity and fast and slow feedbacks.

6): Yes, consistent with related suggestions of R2, we have discussed ice sheets and other slow feedbacks in their own subsection.

7): The definition of SRES has been added.

8): Although all carbon cycle experts would probably agree with the statement, a definitive statement is not needed for our purposes, and the discussion thereof could be long – so instead we changed "it is not expected to continue" to "it may not continue"

1. Figure 9: In the period in question the gas changes are from accurate in situ measurements and the radiation equations are published and in good agreement with other researchers. The fact that the growth rates of CFCs and CH_4 declined at the same time is coincidental, as the reasons differed. The CO_2 dip seems to be associated with Pinatubo effects, at least a strong case for that has been made in the literature.

2. Line 483: I presume that this refers to RCP2.6, not RCP2.5. We have double-checked our graph, and it is correct. The shift to negative growth of the net (all GHGs) forcing is in 25 years. Fossil fuel emissions do not become negative until about 2070 (see Fig. 10), but atmospheric CO_2 growth becomes negative long before emissions are zero (today uptake of emissions is almost half of emissions). In any case, the data for that specific line in Figure 9 are not based on our calculations, they are taken from IPCC – the IPCC tables are published.

3. Line 642: The REDD+ program has an approach similar to what we are advocating, but it seems to still be in its infancy, and, of more relevance to the question of whether we can use it for quantitative statements about how it relates to our carbon uptake goals, there are major uncertainties regarding how to assess REDD+ carbon uptake potential. We note that Richard Houghton is planning to address this topic at the AAAS meeting in February 2017. As of now, it is still a research matter, which we cannot undertake as part of this paper.

However, in the spirit of R3's question, this topic is addressed by land use (LU) plans outlined in the 189 Intended Nationally Determined Contributions (INDCs) that were provided to the UNFCCC as of 04 April 2016. There is a 2016 Technical Annex to the UNFCCC that gives data for the estimated CO_2 removal rate for the proposed INDCs, although it only extends to 2030. We have added reference and quantitative discussion of this. As may have been expected, the expected drawdown from the INDCs is only a fraction of what is needed to achieve our goal of 100 PgC removal, the annual carbon removal being about one/third of what Smith (2016) estimated to be possible and one-fifth of the rate that would be needed to achieve our goal of 100 PgC this century.

4. Carbon removal section: we have revised the text for clarity, and we believe that it is now easier to read. We doubt that adding another figure helps.

5. L695: R3 suggests that we should include negative impacts of basalt dust, not only benefits. We agree, and thus we have added the following:
Against these benefits, we note the potential negative impacts of increased mining operations including downstream environmental consequences if silicates are washed into rivers and the ocean, causing increased turbidity, sedimentation, and pH, with unknown impacts on biodiversity (Edwards et al., 2016).

6. L775: The methane discussion has been revised and reorganized, based in part on suggestions of R1, and we no longer speculate on a specific methane reduction.

7. L785: Yes, there are a lot of interesting aspects and uncertainties in global climate and biogeochemical cycles, not just the one noted here by R3 but also many others. We can't get into each of these, but note that this point draws attention to the merits of our approach, as (1) we look at the most up-to-date global data on each of the three main GHGs and compare their ongoing changes to their ranges in the IPCC models, and (2) we emphasize the response of each of these gases to large paleoclimate changes. While not perfect, these empirical approaches do include all processes occurring in the real global systems.

8. Figure 12a: Yes, this is an important point. In the real world 6%/year reductions are hard to imagine, but 3%/year is plausible. Rather than just showing this result, we should draw attention to it, which we have done in the revision.