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Interactive Comment

Interactive comment on "The radiative forcing potential of different climate geoengineering options" *by* T. M. Lenton and N. E. Vaughan

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We thank the referee for their initial supportive remarks, and we understand their reservations about the method used to estimate the potential of shortwave geoengineering options. Here we try to explain why we took such a simple approach and clarify some misunderstandings. Then we outline our suggested improvements.

Clarifications of original approach:

We must stress that the original method was only ever intended to give 'ball park figures'. The reasons we took such a simplistic approach were twofold; (i) we wanted to provide estimates that could be analytically derived and were 'transparent' for all readers with basic mathematical ability, and (ii) several other papers seeking to quantify these geoengineering options have taken an even simpler approach. Whilst much

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more accurate numerical calculations are clearly possible the results would not be ‘transparent’ in the sense that few readers would be able to reproduce them. There is already a widespread public discussion happening on geoengineering, and some of it is either under-informed by lack of quantification, or misinformed by pseudo-quantitative statements about the efficacy of various options that are in some cases misleading, as can be shown without the need to use numerical models. Regrettably there is also a rather widespread misunderstanding of numerical climate models - either too much faith is placed in them, or they are treated with suspicion (due to their many often hidden assumptions, and the reproducibility problem). Our original method was not “entirely home-grown” - such a simple two layer model is sometimes used in teaching, and also in the energy-moisture-balance atmosphere model of the GENIE intermediate-complexity Earth system model (Lenton et al., 2006), in turn based on that used in the UVic model (Weaver et al., 2001). The referee says “better simple schemes are used in many energy balance models” but gives no references or pointers to the literature and after a lot of searching we have not found a great deal of help.

Weaknesses:

The referee correctly points out some weaknesses in our original approach and recommends major amendments. After considerable thought and reading the literature we have decided to replace our original approach with some alternative formulae more grounded in the existing literature, which are detailed below. Significant issues with the original calculations were: (a) the estimates of the reduction in solar constant required to achieve a given radiative forcing. (b) The estimates of the changes in stratospheric albedo and low-level marine stratiform cloud albedo required, to achieve a given radiative forcing, because these depend on questionable assumptions about absorption and reflection in the atmosphere, and the order in which they occur (hence we offered some limiting values). (c) Failure to account for the effect of different underlying surface types (specifically, ocean under marine stratiform clouds). However, most of the shortwave geoengineering options evaluated involve changes in land surface albedo, for which we

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had already shown reasonable (first-order, global) agreement with more complex calculations accounting for geographical position and season (Hamwey, 2007). We have chosen to stick with an analytical approach, but to replace our original very simple model with formulae based on results from complex radiation codes. The lead author has some experience with complex radiation schemes and the fact that they too have their weaknesses (Goldblatt et al., 2009). We have looked at the referee's suggested radiation scheme (Lacis and Hansen, 1974) and made use of a simple parameterisation of its results for clear sky (Chen and Ohring, 1984) as well as results from other radiation codes.

Specific corrections:

We agree with the referee that our original two statements that "This is a reasonable approximation" were ill-advised and they will be removed. We did try to acknowledge that we were making a number of sweeping assumptions (but obviously not in enough detail for referee 2, although referee 1 understood the spirit of our approach).

There was a typo (repeated) regarding the total absorption, which is 235 W m-2.

We will remove the statement from the abstract of revealing "significant errors" in prior research.

The statement on p.2589 line 16 that the achievable albedo change of desert regions may have been "grossly" over-estimated has got nothing to do with the radiative transfer calculations (we were simply referring to what albedo change can be achieved by changing surface materials, not its effect on radiative forcing) but we will adjust anyway.

Substantive changes:

The following are our detailed responses to the more general points raised:

Global mean approach:

The referee argues that "it is questionable that the global-mean insolation is appro-

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priate” given the geographical variation of forcing mechanisms. However, Caldeira and Wood (2008) find that a given reduction in top-of-atmosphere solar insolation has roughly the same effect on surface temperature regardless of where it is applied, because of cancelling effects. For example, concentrating a reduction in solar insolation in the high latitudes has a smaller effect on the total radiation budget but because positive feedbacks are regionally stronger the net effect is about the same. This means that our global average approach to considering shortwave geoengineering proposals is not totally unreasonable, even if the intended deployment of a given option is biased to a particular region, e.g. the high latitudes. Furthermore, Hansen et al. (2005) find for a wide range of forcing agents, some spatially uniform, some quite heterogeneous, that surface air temperature changes have a similar pattern for the same global forcing by different agents. Hence we propose to stick with a global mean approach, but to add some justification and qualification by referring to the above studies, and to include discussion of how changes in latitude and season and hence solar zenith angle will affect the estimates in regional geoengineering cases.

3.1.1 Sunshades in space:

We failed to recognize in our original approach that changes in shortwave radiative forcing can have lower “efficacy” than changes in longwave radiative forcing, where efficacy refers to the change in surface temperature caused by a given change in radiative forcing (Hansen et al. 2005). This is because globally uniform shortwave forcing (by e.g. reducing the solar constant) is biased to the lower latitudes where positive climate feedbacks are weaker, whereas longwave forcing is more uniform, and in the higher latitudes there are stronger positive climate feedbacks. We thank Ken Caldeira and Jim Hansen for pointing this out. In the model of Hansen et al. (2005) the efficacy of radiative forcing from a 2% reduction in solar constant is only ≈89% of that due to doubling CO2. Hence the required change in solar constant should be ≈12% larger than that estimated from our original formula (1). Thus to counteract a 3.71 W m-2 radiative forcing should require a ≈1.8% decrease in solar constant rather than ≈1.6%,

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in agreement with (Govindasamy and Caldeira, 2000). We will revise the estimates of total area of sunshades and the numbers that need to be added each year accordingly. Different models also differ from the IPCC standard value (3.71 W m-2) in their estimate of the radiative forcing due to doubling CO2. For example, (Hansen et al., 2005) find RF due to 2xCO2 is 4.12 W m-2 rather than 3.71 W m-2 (an 11% increase). Hence in their model the required change in solar constant to counteract a doubling of CO2 is ≈2% (1.6x1.12x1.11). In general, we propose to include adjustments for efficacy for each shortwave geoengineering option according to the efficacy estimates of Hansen et al. (2005).

3.1.2 Stratospheric aerosols:

The efficacy of stratospheric (volcanic) aerosol forcing is estimated to be ≈91% from simulations of the aftermath of the Mt Pinatubo eruption (Hansen et al., 2005). This agrees well with the efficacy of reductions in solar insolation, consistent with both forcing factors operating above the tropopause. Hansen et al. (2005) also provide a simple relationship between aerosol optical depth (τ, at 0.55 μm wavelength) and adjusted radiative forcing (after stratospheric temperatures are allowed to adjust):

$RF \approx -24\tau$ (R1)

This implies that to achieve RF = -3.71 W m-2 requires $\tau \approx 0.155$. However, taking into account the lower efficacy of stratospheric aerosol forcing suggests $\tau \approx 0.17$ is required to counteract the temperature effect of a doubling of CO2. $\tau \approx 0.17$ is comparable to the peak optical depth that occurred after the Pinatubo eruption, although the resulting cooling was only partially realised because of the short lifetime of the aerosol and thermal inertia of the ocean (which gives global temperature response an e-folding timescale of ≈7 years). Geoengineering approaches aim to continually replace the aerosol and thus to eventually fully realise the resulting cooling and maintain it.

Recent work (Lacis et al., submitted) finds that the optimal effective radius for cooling by sulphate aerosol is 0.23 μm and a loading of 0.01 g m-2 (in a single strato-

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spheric layer at 27-29 km altitude) gives a maximum optical depth of τ = 0.038 and corresponding adjusted radiative forcing RF = -1.03 W m-2 (at the tropopause, after stratospheric temperature adjustment). Alternatively, a fixed optical depth of τ = 0.1 (at somewhat sub-optimal effective radius of 0.3 μm, which is comparable to the less effective Pinatubo aerosol with peak $\tau \approx$ 0.17) gives a maximum adjusted RF $\approx$ -2.5 W m-2 (depending on altitude of deployment). However, as Lacis et al. (submitted) note, the tendency of sulphate aerosol particles to coagulate will reduce their radiative effectiveness and hasten their exit from the stratosphere (and if they grow in excess of $\approx$2.2 μm they become net warming agents).

We propose to replace the quantification in our original paper with one in terms of aerosol optical depth (at 0.55 μm), based on the formula of Hansen et al. (2005), noting that with a more optimal effective radius a somewhat lower optical depth would be required to achieve a given radiative forcing, but because aggregation would degrade the effect over time, the Hansen et al. (2005) formula for Pinatubo-like aerosol seems a reasonable compromise.

Lacis et al. (submitted) also make calculations for stratospheric soot aerosols (only effective as a cooling agent at high stratospheric altitude) and for aluminium aerosols. For completeness we propose to briefly discuss their results. For soot aerosols of optical depth τ = 0.01 (at 0.55 μm), RF = -1.86 W m-2 can be achieved with deployment at 44-50 km altitude. For aluminium aerosols of optical depth τ = 0.01 (at 0.55 μm), the adjusted forcing ranges from RF $\approx$ -0.5 W m-2 for deployment at the bottom of the stratosphere toward RF $\approx$ -1 W m-2 for deployment at 50 km altitude.

3.1.3 - 3.1.4 Cloud albedo changes:

We acknowledge that “a global mean surface albedo...is inappropriate to the oceans” and that this probably led us to overestimate the change in marine stratocumulus cloud albedo required to produce a given change in planetary albedo (because we overestimated the reduction in reflection to space from the underlying surface). If we replace

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the original global average surface albedo of 0.152 with a representative ocean surface albedo of 0.07 for solar zenith angle 60° then (assuming all atmospheric absorption before reflection) the multiplier in the relationship (6) between changes in atmosphere (cloud) albedo and planetary albedo changes from 0.682 to 0.748, and the required change in cloud albedo becomes 0.083 rather than 0.091 (but still markedly larger than the 0.062 estimated by Latham et al., 2008).

We also acknowledge that "absorption in the troposphere is mostly by water vapour in the solar near-infrared, and yet most of the reflection by clouds and aerosols is in the visible part of the spectrum" and that this probably also contributed to an overestimate of the maximum required change in marine stratocumulus cloud albedo (due to an overestimate of prior absorption in the atmosphere). We did consider the unrealistic assumption of no prior absorption in the atmosphere (perfect transmission), which with the alternative ocean surface albedo would give $\Delta\alpha_p = 0.948\Delta\alpha_a$ (and a required change in cloud albedo of 0.065). However, there is atmospheric absorption prior to, and after, reflection by marine stratocumulus clouds, which can be accounted for by a transmittance factor, T_a. If we ignore any changes in back reflection by the highly absorbing underlying ocean surface then the change in planetary albedo due to changing cloud albedo, $\Delta\alpha_c$, can be approximated (Roberts et al., 2008) by:

$\Delta\alpha_p = f_c {T_a}^2 \Delta\alpha_c$ (R2)

where f_c is the cloud fraction, and for e.g. 1.6 km above sea level T_a = 0.925 so ${T_a}^2$ = 0.856 (giving a required change in albedo of 0.072). The altitude of marine stratocumulus cloud tops is often lower than this and the transmittance correspondingly lower hence the required change in albedo would be higher.

The efficacy of cloud albedo changes cannot be isolated in the same way as those due to stratospheric aerosol or solar insolation changes. However, when using an altered definition of radiative 'forcing' that allows tropospheric and land temperatures to adjust, indirect aerosol effects on cloud albedo appear to have similar or slightly

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greater efficacy than 2xCO2 (Hansen et al., 2005), so there are no clear grounds for an efficacy adjustment.

Having read extensively about the aerosol-cloud albedo effect (also called the ‘first indirect aerosol effect’ or ‘Twomey effect’ (Twomey, 1974)) we have assembled some simple formulae to approximately relate aerosol concentration to cloud albedo.

Aerosol number concentration, N_a, can be related to cloud droplet number density, N_d, by:

$N_d \approx {N_a}^a$ (R3)

where a critical uncertainty is the value of the power a, which depends on the hygroscopicity of the aerosol. IPCC (2007) give a = 0.06-0.48 from the measurements of Feingold et al. (2003) (over land), who themselves suggest a typical a = 0.7.

Cloud droplet number density can be related to effective radius, r_e, by:

$r_e = k{N_d}^{-1/3}$ (R4)

where k is a constant that can be derived from observations, e.g. (Twohy et al., 2005).

Effective radius can be related to cloud optical depth, τ, by:

$\tau = 3lh/(2\rho_w r_e)$ (R5)

where l is cloud liquid water content, h is cloud height, and ρ_w is density of water. The Twomey effect assumes no change in cloud liquid water path (lh) with changes in N_d. Therefore:

$\tau \approx K{N_a}^{a/3}$ (R6)

where K is an amalgamated constant. Finally, cloud optical depth can be related to cloud albedo, α_c, using e.g. the Eddington approximation:

$\alpha_c = 0.75(1-g)\tau/(1+0.75(1-g)\tau)$ (R7)

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where g is an asymmetry factor and g = 0.85 is used elsewhere (Twohy et al., 2005). (Other relations between optical depth and cloud albedo based on the two-stream approximation could be adopted, e.g. Roberts et al., 2008.) From differentiating (R7):

$\Delta\alpha_c \approx \Delta\tau 0.75(1-g)/(1+0.75(1-g)\tau)^2$ (R8)

If we assume as a representative case τ = 10 and α_c = 0.53 then to achieve $\Delta\alpha_c$ = 0.062 requires $\Delta\tau$ = 2.5. Taking a = 0.5 this demands a factor of 3.8 increase in N_a, or for a = 0.7 a factor of 2.6 increase in N_a. Correcting for atmospheric transmission, to achieve $\Delta\alpha_c$ = 0.072 demands a factor of 4.6 increase in N_a for a = 0.5 or a factor of 3.0 increase in N_a for a = 0.7.

There are caveats as to whether cloud liquid water path does in fact remain unchanged. Observations have found “no correlation between calculated cloud optical thickness or albedo and particle concentration” because cloud liquid water path varied in a counter-acting way across the dataset (Twohy et al., 2005). Where changing liquid water path could be factored out of the data, an aerosol-cloud albedo effect was observed (Twohy et al., 2005). However, the IPCC (2007, pages 171-173) assessment is sceptical about an effect over the ocean, indicating that current observations better support the effect in stratocumulus over land.

Alternatively, as the referee notes in their minor comment 5, there is scope for a second indirect aerosol effect, whereby smaller cloud droplets form raindrops less effectively, lengthening the lifetime of clouds and thus potentially increasing their liquid water path and/or their fractional coverage (Albrecht, 1989). Such effects would increase the magnitude of negative radiative forcing, following (R2) or (R5) above.

Overall, the radiative forcing effects of geoengineering clouds are arguably the hardest of all to predict or to boil down to a simple formula. We note the large uncertainty range in estimates of the current anthropogenic radiative forcing due to changes in cloud albedo of -0.3 to -1.8 W m-2 (best guess 0.7 W m-2) (IPCC, 2007). This suggests that any estimate of the radiative forcing effect of geoengineering cloud albedo has more

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than a factor of 2 uncertainty in each direction. We propose to revise the method and the relevant sections based on the above.

3.1.5 - 3.1.9 Surface albedo changes:

We have compared our simple derived linear relationship (with gradient ≈0.58) between surface albedo changes and planetary albedo changes with the results of more complex radiative transfer codes and simple formulae derived from them (Lacis and Hansen, 1974; Chen and Ohring, 1984; Li and Garand, 1994). These also show a linear relationship, but the gradient depends on solar zenith angle, on total precipitable water in the column, and on cloud cover. Thus, we generalise our original formula (7) by replacing the constant of proportionality (0.579) with a parameter b,

$\Delta\alpha_p = b\Delta\alpha_s$ (R9)

In a clear-sky, b represents the mean effective 'two-way' transmittance of the atmosphere (hence it can be related to ${T_a}^2$ in (R2) except that the wavelengths relevant to surface and cloud reflection differ). Chen and Ohring (1984) derive such a relationship from the model of Lacis and Hansen (1974) and find b = 0.730 in the annual global mean. Similarly, for a fixed solar zenith angle of 60°, b = 0.729. Two different sets of observational data are used to obtain b = 0.756 and b = 0.676. As solar zenith angle increases, b decreases from a maximum of b = 0.776 at 0° to b = 0.509 at 85° due to the greater absorption along longer path lengths of sunlight through the atmosphere. Results from a different radiative transfer model that additionally includes aerosols and variation in the albedo of some surfaces with solar zenith angle (Li and Garand, 1994), give a formula for b in terms of total precipitable water in the column, p (in cm), and the cosine of the solar zenith angle, c:

$b = (1.16711 + 0.05963p0.5 + (0.07514 + 0.04105p0.5)/c)^{-1}$ (R10)

For sunlight overhead (c = 1) and a dry column (p = 0) the maximum possible value of b = 0.8. The dependence on precipitable water in the column (decreasing b) is

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consistent with the referee's comments that for vegetation changes, the high albedo lies at wavelengths where water vapour absorption is greatest. For a solar zenith angle of 60° (c = 0.5), our original value of b = 0.58 corresponds to a precipitable water content of ≈8 cm, which is high (e.g. representative of the inter-tropical convergence zone), because this formula is for clear skies.

Clear-sky values for b could be used to estimate the maximum potential radiative forcing effect of geoengineering measures that change surface albedo, but this will be an over-estimate as clouds are present some of the time everywhere. In the presence of clouds, radiation reaching the surface is reduced and hence the radiative forcing effect of geoengineered changes in surface albedo will be reduced. (This is somewhat mitigated by the referee's observation that most of the reflection by clouds is in the visible part of the spectrum whereas for vegetation changes the high albedo lies in the solar near-infrared.)

Recent global estimates of total downward shortwave radiation at the Earth's surface of 171.6 W m-2 and net downward (i.e. absorption) of 149.4 W m-2 (Hatzianastassiou et al., 2005) are smaller than those we used (Kiehl and Trenberth, 1997) (and correspond to a lower surface albedo of 0.129). The corresponding global average shortwave flux incident at the surface is only 0.50 of that at the top of the atmosphere, compared to 0.58 that we used originally. However, the original global mean broadband radiation approach does not account for spectral variations in absorption or albedo, and it assumes that all absorption occurs before reflection at the surface with none occurring on the way out of the atmosphere.

Given all this we propose to use a range of values of b = 0.50-0.73 in estimating the maximum effect of geoengineered changes in land surface albedo. We also propose to include some brief discussion of the effect of latitudinal and seasonal biases in particular methods.

As well as the solar zenith angle dependence of atmospheric absorption, surfaces

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generally have higher albedo at higher solar zenith angle, and the effects are generally greater than incorporated by (Li and Garand, 1994). Some surfaces (e.g. grassland, arable land) experiencing a greater change in albedo for a given change in solar zenith angle than others (e.g. forest). A popular formula (Briegleb et al., 1986) is:

$\alpha(c) = \alpha_s(1+d)/(1+2dc)$ (R11)

Where c is still the cosine of the solar zenith angle, α_s is the albedo at 60° solar zenith angle and d is an empirical parameter. For grassland and arable grass, d = 0.4 (Briegleb et al., 1986), for desert d = 0.15 (Wang et al., 2005) and for other surface types d = 0.1 (Briegleb et al., 1986). This effect will matter in geoengineering cases where the solar zenith angle systematically differs from the global mean 60°, but at the same time the mean incident radiation will be varying, and the two effects tend to counteract one another. For geoengineering approaches biased to the low latitudes or summer season, the incident radiation will tend to be higher but the change in surface albedo will tend to be smaller than suggested by the global average approach (especially for e.g. grassland). Conversely for geoengineering approaches biased to the high latitudes or winter season, the incident radiation will tend to be lower but the change in surface albedo will tend to be higher than suggested by the global average approach (especially for e.g. grassland). We propose to put formulae (R10) and (R11) which include the effect of varying solar zenith angle in an Appendix.

Response to minor comments:

1. We acknowledge that radiative forcing can also measure the perturbation due to natural interventions and will clarify this.

2. We will cite IPCC AR4 chapter 2 more extensively.

3. Hamwey's method (Hamwey, 2007) uses the 2 dimensions of the land surface (i.e. accounts for variations in longitude and latitude). However, on further inspection it only estimates changes in radiative forcing at the surface. It does not account for any

absorption or reflection of upwelling (reflected) shortwave radiation and hence is not directly comparable to our revised estimates. It is also a broadband approach so does not consider the spectral character of downward shortwave radiation at the surface or of surface albedo. We will qualify these aspects in revising the paper.

4. We will replace “whopping” with “massive”.

5. The point about marine stratiform clouds is addressed above.

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