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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Could aerosol emissions be used for regional heat wave mitigation?" *by* D. N. Bernstein et al.

Anonymous Referee #2

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In this study Bernstein et al. explore whether it is possible to mitigate a heat wave in California with aerosols emitted into the lower stratosphere. Among other the paper focuses on the following questions: (1) How much aerosol is required to mitigate the heat wave, (2) is aerosol advection a limiting factor to the effectiveness of the method, and (3) what is its relationship to the geographical dimension of the injection? In order to elucidate these questions the authors carry out a number of simulations with different aerosol injection rates, and on different geographical scales, starting with one about twice the size of California, and then going down to the size of the metropolitan area of Los Angeles. The sensitivity of the aerosol field to the injection height is not directly explored, however an optimal height of injection is evaluated and set to 12'000 m. The authors find the following main effects: (1) with a high enough injected aerosol mass, the heat wave may be effectively mitigated both in the sense that the maximum temper-

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Discussion Paper

ature is considerably reduced and the afternoon time interval of big heat is reduced, (2) the zenithal angle of the sun is a major contributor to the effectiveness of the method, and is responsible for the latter effect, (3) advective effects are negligible provided that the area of injection is large enough, although these tend to be more important closer to the sea; for the small scale experiments the effectiveness is considerably reduced. The authors conclude that regional heat wave mitigation, though physically feasible, is unlikely to be carried out due to the massive amount of sulphur that is required, and the relative non-effectiveness of the small-scale configuration. Also they stress the risks involved with the method and point to their preference of mitigation.

As a reviewer, I would like to make the following major comments:

1) Within the paper the authors make two kinds of remarks that I would qualify as being personal. First, they state that although geoengineering is not the primary focus of their research they may make a significant contribution to the field (page 23'795, line 27 ff.), second they claim that the side effects of geoengineering may be considerable and therefore express their concern about these methods (several instances). I would question the appropriateness of these remarks. Although the author's capability might contribute to the decision making process of geoengineering, a scientific journal is not meant for advertising potential and qualifications but for the publication of research results that contribute to scientific progress only. The author's personal concern about geoengineering, although shared by the reviewer, is of no relevance to a scientific publication. If the authors feel concerned about geoengineering, then they should consider publishing research results on the potential side effects. This study, however, investigates the effectiveness and to some extent the feasibility of regional geoengineering; in doing so the authors could make a substantial push for geoengineering, because their results actually imply that regional climate engineering could be considered as a viable option for heat wave mitigation, as the results of this study point a physical effectiveness of the method investigated. I am now going to elaborate on this below.

2) To my appreciation the authors tend not to recognise to their full extent some key lim-

ACPD

12, C8750–C8755, 2012

itations to the effectiveness of sulphur geoengineering that derive from aerosol growth dynamical properties. In their experiments the aerosol particles are treated as emission (23'799, 23 ff.), in as much as that they are injected directly at a level of 12'000 m. The size discretization comprises 4 bins, the smallest being in excess of 40 nm and the largest being as large as 10 micrometers. In doing so, the authors make several key simplifications to aerosol growth dynamics, that might actually result in unphysical effects. First, the authors put themselves into the assumption that particles formation is going to be effective in the lower troposphere. This assumption, although not a small one, may be correct, depending on technique, however it needs to be mentioned explicitly in the paper. Second, the initial size in excess of 40 nanometers implies that the initial growth phase of the aerosol is not represented. The initial growth phase is time consuming and determines the relationship between the aerosol number and mass concentration. Third, the aerosol size resolution does not make sense from an aerosol growth dynamical point of view. The aerosol particles should not reach particle sizes corresponding to the 4th or even 3rd size bin. Particles reaching that size under model conditions are actually due to the model's numerical diffusion properties. The particles' actual size distribution should be comprised between the first two bins, thus resulting in a spectral resolution that is obviously too low to represent the growth dynamics accurately. Precisely, these restrictions in terms of aerosol growth dynamical properties could cause the close to linear relationship between the injection rate and the particle's radiative forcing that is diagnosed in this study. Global studies tend to show a less than linear relationship between the injection rate and the effectiveness. This is due to the fact that aerosol growth dynamical properties are such that an increase of the injection rate leads to a corresponding increase of particle size rather than particle number. Although I cannot rigorously prove it, I suspect that via the combined effect of direct particle emission, numerical diffusion and low size resolution, the aerosol set up used here biases the growth dynamics to the point that their number concentration is proportional to the injection rate.

3) The authors claim that their study reinforces concerns about the feasibility and sus-

ACPD

12, C8750–C8755, 2012

Interactive Comment

tainability of geoengineering, both global and regional. This claim, as laid down in this paper, appears shaky if not flawed to me for several reasons. First, the results produced imply that regional heat wave mitigation may be effective if the area of injection is large enough. It is then not limited by mixing effects on its border and advection away from the target area as a whole. This view might eventually turn out to be over-simplified, under the lights of the points raised in the previous section. Second the authors claim that the amount of aerosol to inject is too high for a mid range experiment to be realistic. I do not entirely share this view. The amount to be injected for the mid-range experiment is 3'750 tons, which are equivalent to some 120 large cargo jet payloads. Then, it all comes down to the following. If the injection technique is such that large cargo jets may be used (in terms of the height of injection and the release rate), then 120 payloads may not necessarily appear to be prohibitive without further proof. Third, this study does not analyse severe side effects of regional geoengineering that appear prohibitive to its implementation, nor are these similar, and therefore consistent to those of global geoengineering. Testing and optimising the method over remote areas first could easily circumvent considerations for local safety. Global geoengineering concerns do not apply because the method's effect on ozone and precipitation should not be relevant (to the least these are not evaluated within this study). To me the conclusion to this study therefore is that the results seem to imply the method's effectiveness and feasibility, however depending on their confirmation once comprehensive aerosol microphysical effects are taken into account, and on the development of solutions to technical issues. Potential side effects require further investigation and possibly first implementation experiments over remote areas.

4) Graphs: Instead of showing graphs on consecutive days that look rather similar and are therefore not very informative, I would have preferred some graphs showing the vertical distribution of the aerosol and its effect on temperature, the influence on the flow field shown vertically and/or horizontally, horizontal graphs of particle number concentration and size in bin 2. I am just getting the impression that the experiments could yield a lot more effects if these had been investigated.

ACPD
12, C8750–C8755, 2012

In conclusion, I believe that the paper shows two interesting results, namely that regional geoengineering with sulphur particles may be effective, depending on the injection rate, and that heat waves are shortened in time due to the relationship between the optical depth of the aerosol and the zenithal angle of the sun. I would like to leave to the appreciation of the editor if these two results correspond to sufficient scientific progress to merit publication on their own. Personally, I would have preferred a much more thorough investigation of the model results, especially as to the effect on atmospheric dynamics, and the aerosol that is generated in terms of its vertical and horizontal distribution. Some of the set up of the aerosol appears unfortunate to me, and I think that the authors should discuss their results in the framework of these simplifications. Also I cannot follow the authors in their conclusion, and would rather believe that the results show regional geoengineering to be effective and potentially feasible. Side effects are not investigated, for that reason the mere concern of the authors about these does not help. I am sorry not to have reached a more positive review.

Minor comments:

23’795, 25-26: smaller size aerosols scatter more efficiently provided that these are not too small, and that the total mass is conserved (that is that a decrease in size is compensated by an increase in number). 23’796, 24-27: the study of Pierce et al. precisely investigates the role of subscale aerosol microphysical growth processes to the log term global scale properties. The subscale processes act on a very short time scale, of typically a few seconds to a day or two. 23’798, 17-19: revise sentence, probably word missing. 23’799, 22: beginning of sentence, something might be wrong 23’799, 23: please indicate injection altitude here. It is an important set up parameter. I would like to see more details on how this value has been arrived at. 23’799, 26-29: I would very much like to know how the aerosol is distributed among size bins. 23’801, 19: could you please expand a little bit on how the importance of vertical advection is demonstrated, may be even with an extra graph, showing the vertical distribution of the aerosol. 23’803, 14-15: the order of the graphs seems to be inconsistent. 23’804, 1-2:

ACPD

12, C8750–C8755, 2012

as detailed within the major comments, I believe that these bins should be empty. No aerosol grows within half a day to more than micrometer size while the bulk of particle mass is contained in bin 2. The actual particle distribution should be bi-modal. 23'806, 25-27: the relationship between the mass of sulphur and the mass of the aerosol is given by the MOSAIC thermodynamic inorganic scheme. Although a time function, no need to cite Rasch here, but rather give typical values for these experiments. Please do indicate total injected aerosol mass, as sulphur equivalent, in Table 1. 23'807, 10-23: May be too short for a section on its own, please consider integrating into the discussion, which would in turn be split from the conclusion. 23'809, 5-7: the decision maker is not being helped with the injection height, because the choice of it, although motivated qualitatively, appears to be arbitrary as not investigated, but rather just set.

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12, C8750–C8755, 2012

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