Geoengineering Research Reservations
by

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My interest in these remarks is how to assess the risks inherent in geoengineering.

My argument is that geoengineering is deviant when it comes to the normal process by which science proceeds – perhaps deviant enough to undermine the whole enterprise.

Some proponents of geoengineering advocate limited experimentation to better the technology by which full scale geoengineering might be effectively implemented. But if there is no basis for full scale implementation whether or not the technology is available, the value of researching the technology is thrown into question.

In these remarks, my talk of geoengineering is restricted to solar radiation management and does not include carbon capture. Moreover the kind of solar radiation management I have in mind is planetary wide and (for the purposes of discussion) sulfur insertion into the stratosphere. (This is not an arbitrary focus in that, with others, I take this to be the only currently plausible candidate to achieve a 2 degree centigrade cooling should there be a need to do so.)

Some people have worried that such insertion on a planetary wide basis might produce unforeseen global consequences due to unexpected atmospheric non-linearities. I think these worries are over blown based our general knowledge of atmospheric systems and the record of volcanic eruptions which insert concentrations of SO_2 many times greater than the concentration that would be needed to produce a 2 degree cooling.

The locus of my worry is quite different: our current climate models become progressively weaker in their accuracy the more fine grained you go. At the local level, they are of limited value. As such, assessing the risk of sulfur insertion when it comes to local weather disruption (and attendant agricultural output) is very hard to do. And of these the most notable area of concern is monsoon disruption. Indeed, both theory and modeling raise concerns about sulfur's role in affecting precipitation as opposed to temperature. (See G. C. Hegerl, S. Solomon*, Science* 325, 955 (2009); published online 6 August 2009 (10.1126/science.1178530), and A. Robock, L. Oman, G. L. Stenchikov, *J. Geophys. Res*.113, D16101 (2008).) But even bracketing these considerations, my argument is more abstract: in its applications, most science proceeds from a model, to the laboratory, to field tests, and only finally to wide implementation. Nowhere is that more true than in medicine. At each stage, there is trade off between verisimilitude to the final implementation and the limitation of risk. (Think of the use of animals in experimentation.) What makes that possible is that most of science deals with modular phenomena. You can test a vaccine on one person, putting that person at risk, without putting everyone else at risk. So, even though we have lot of planetary wide goals – like eradicating smallpox – we can test them for untoward effects before full scale implementation. Not so for geoengineering. You can't build a scale model of the atmosphere or tent off part of the atmosphere. As such you are stuck going directly from a model to full scale planetary wide implementation.

It has been argued that sulfur insertion could be usefully implemented in polar regions without effects on the rest of the planet. But modeling to date as well as the volcanic record would seem to undermine this idea by demonstrating that polar insertion does not in fact remain restricted to those latitudes. The idea that one could study the risk of planetary wide insertion with low concentrations might seem plausible. But it has been shown that doing so would require at least a decade to derive enough data to differentiate a signal from noise. (See Robock, Alan , Martin Bunzl, Ben Kravitz, and Georgiy Stenchikov, 2010: A test for geoengineering? *Science*, **327**, 530-531, doi:10.1126/science.1186237.)

Geoengineering is not unique in forcing us to go directly from a model to full scale planetary wide implementation – but it is rare. The only other example I can think of is genetically altered crops. But a difference between the two is that in the case of genetically altered crops, we have rich theoretical knowledge of natural selection as well as a long history of selective breeding.

One response to these concerns is to argue that risk is risk and all risk can always be assimilated into a standard cost-benefit (or expected utility maximization) analysis. On that basis, the risks of climate change may be greater than the risks of geoengineering, especially when understood as a temporary stop gap measure.

But such analyses become less and less coherent as our ignorance of both the likelihood and the magnitude of worst case scenarios goes up. That is as true of our understanding of potential untoward effects of geoengineering interventions as it is for the effects climate change itself.

Now one response to such ignorance is to defend a variety of precautionary principles which come down (in philosophical terms) to adopting what is known as a maximin principle. That is, choose between alternative courses of action so that the worst case outcome is the best of alternative worst case outcomes.

But to even apply such a maximin approach assumes we have some knowledge to characterize these alternatives. My claim is that we do not have such knowledge in hand for geoengineering and I don't see how we can gain it to have a basis on which to make a prudent choice until and unless our climate models undergo considerable improvement. Nor do I think is it plausible to think that international agreement to implement geoengineeering would be likely without such improvement in our models.

As such, a focus on more benign forms of intervention that don't need to be implemented on a planetary wide basis has much to recommend itself, including, especially, ambient carbon capture.

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