

Researching geoengineering: should not or could not?

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Abstract

Is geoengineering a feasible, sensible, or practical stopgap measure for us to have in our arsenal of potential responses to global warming? We do not know at this point and so it seems hardly contentious to claim that we should find out. I evaluate a moral argument that we should not try to find out and a methodological argument that even if we try, we cannot find out. I reject the first but end up as agnostic on the second, outlining the burden of proof that it creates for proponents of geoengineering research.

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Is geoengineering a feasible, sensible, or practical stopgap measure for us to have in our arsenal of potential responses to global warming? We do not know at this point and so it seems hardly contentious to claim that we should find out. ‘Hardly seems contentious?’ Those are fighting words for a philosopher. Here I want to present a moral argument that we should not try to find out and a methodological argument that even if we try, we cannot find out. I reject the first but end up as agnostic on the second outlining the burden of proof that it creates for proponents of geoengineering research.

Of course there is geoengineering and then there is GEOENGINEERING. Nobody gets very wound up about the idea of planting trees or painting roofs white as instances of geoengineering—which is not to say that they will necessarily do much good. The kind of geoengineering that elicits howls of disapproval is grander than this—it is things like space mirrors, sulfur injection into the upper atmosphere, and iron fertilization of the oceans—it is the idea of intervention on a grand scale. I do not think there is much value in getting involved in a semantic squabble here. It is enough that we have at least some proposals on the table that cause disquiet and others that do not. But I will come back to argue that there is an important methodological difference between small ‘g’ proposals and big ‘G’ proposals.

1. Moral arguments against geoengineering

Why might it be the case that we *should not* research geoengineering? If you think that there are no circumstances

under which we ought to ever implement a geoengineering scheme, then there would hardly be much point in researching its feasibility. Many reservations have been raised about implementing geoengineering schemes of one sort or another, but these are usually coupled with a reluctant endorsement of the need for more, not no, research. Robock (2008a) has a litany of concerns about who decides, potential misuse, side effects, uneven effects, and dangerous consequences of sudden cessation of an intervention. Jamieson (1996) offers conditions on implementation that would be hard to satisfy when he proposes that the ‘consequences must be predictably reliable’ and have ‘no irreversible climate consequences’. Still, even if Robock and Jamieson support research, when you read their reservations about, or conditions on, implementation, it is hard to see how research could ever yield a ‘go’ signal. All science and technology faces the kinds of concerns Robock raises about control and consequences. (From the beginning, the scientific enterprise has been a struggle between nurturing and controlling the interests that drive the enterprise.) And no science or technology could be implemented if Jamieson’s condition of predictably reliable consequences was taken seriously. (All science involves a trade-off between risks and returns without the luxury of full knowledge of either.) These kinds of considerations support the suspicion that there is a tendency to treat geoengineering as outside the bounds of normal science and hence subject to a different set of standards. But what is the argument? Is it a moral argument?

There is an undeniable sense of chutzpah that is hard to disentangle from talk about geoengineering at a grand scale.

Gardiner raises the argument that it is more of the same reckless attitude that got us into the problem in the first place when he writes that: ‘we could clean it (our planet) up . . . but so intent are we on continuing our messy habits, that we will pursue any means to avoid that, even those that imposed huge risks on others and involve further alienation from nature’ Gardiner (2009) (forthcoming; p 25). While Jamieson writes that even if geoengineering were successful ‘it would still have a bad effect of reinforcing human arrogance and the view that the proper relationship to nature is one of domination’ (Jamieson 1996) (p 332). And yet even if we embrace such considerations, do they help us resolve the dilemma that we face? Here we are at a fork in the road. Should we turn left or right? Which will do less harm? Maybe we should not have gone down the road that we have traveled as a species. But pointing that out is not of much help. Perhaps it does help though. Jamieson and Gardiner would likely argue that faced with such choices, there is often a short term technological fix. But in the long run, making choices relying on such technological fixes would not solve the underlying problem which is in the end practical not moral—we have to find a place consistent with the limits of nature. Yet, even if we were to swear a solemn collective oath to do that, and had the collective means to follow through on such an oath, we are still where we are, and why demand that we go ‘cold turkey’? We still may need to avail ourselves of technical fixes in the transition for such a transition to even be possible. Why not at least investigate whether geoengineering is available as one of those fixes?

2. The permissibility of research

But the idea of taking on such an investigation needs to be treated with care. For there is a danger of treating it, and all of science, in an idealized way when it comes to research. That picture of science places individual creativity at the center with research as a neutral arbiter. But this picture is strikingly blinkered when it comes to the sociology of science. We are too creative to research *all* of the ideas that we come up with and the process by which choices are made is far from rational. But more important, once research programs are chosen, sociological forces increase the probability that development will take place. This consideration (which both Jamieson and Gardiner raise) is not to be confused with arguments based on moral hazard or the allocation of scarce research dollars. Both of those seem exaggerated worries. Moral hazard is sometimes raised as a problem at the level of not just implementation and research, but even talking about geoengineering. But it is hard to discern why moral hazard should function as a deterrent to action here any more than it does elsewhere. Governments regularly sponsor research and programs to counter the effects of ill advised actions—even their own actions. An amnesty program for illegal immigrants is designed to offset the effects of poor border enforcement and in doing so it encourages more not less challenges to the border. Such programs involve a balancing act of benefits versus costs, despite the name which implies some sort of moral judgment. Moral hazard only arises for geoengineering if you think that research or, if it came to it, implementation would undermine other actions and lead to

more not less greenhouse gas output. That seems far-fetched since, at least among policy makers, nobody believes that geoengineering offers anything but a relatively short stopgap to buy time for other action. Nor are the funds that would be needed for geoengineering research large enough relative to the research budgets of even the United States, let alone the whole developed world, to create an allocation issue. The worry about research is something different—more a matter of inertia. It is the kind of worry that comes to the fore when you look at the history of research programs like the US missile defense shield. But if this is a *general* feature (albeit a dysfunctional one) of large scale scientific research programs, unless you think no such research should take place because of it, you need to produce an argument that geoengineering has some special exceptional features that make this concern relevant here where it is not elsewhere.

In fact I do think there is such an argument to be made. It is not that I think that big ‘G’ geoengineering is unique, but I do think that it falls into a special class of scientific endeavors that generate a set of methodological challenges, not as to whether research *should* be done but whether it *could* be done.

3. The feasibility of research

On the standard, if idealized model of science, the road to full deployment has a number of way stations each of which offer an opportunity to assess benefit under increasingly realistic conditions. There is of course a trade-off here. The more restrictive the realism, the less the risk but also the less our ability to assess the benefits. Wherever this process starts, be it in the lab or in a computer model, full scale deployment does not take place before testing in more limited circumstances—in scale, strength, or range. So in medicine, even after animal testing, we restrict the number of subjects exposed and increase the strength of the exposure in a series of steps. In medicine we can follow this procedure because of something we take for granted—our object of interest is reasonably modular or encapsulated. That is what makes it possible for us to extrapolate from experimental subjects to the population as a whole with confidence. The experimental subject is a full scale representation of the objects of interest. Not all of our scientific or technical interests allow for full scale representation at the experimental level, and in that case we have to also be able to extrapolate to scale with confidence. But what if the object of your interest is not modular or encapsulated? What do you do then? For that, after all, is the feature that big ‘G’ geoengineering proposals have in common. They call for interventions on systems that lack just this characteristic. You cannot encapsulate part of the atmosphere and it is too complex to be able to build a realistic non-virtual model at scale. As such, it is reasonable to ask whether we could ever have a sound basis for moving to full deployment of any such proposed intervention. And if not, then why bother to even research such proposals in the first place?

Before examining this question, we should pause to note that, at least in some respects, the quandary posed here is not unique to geoengineering. While it is true that most of science and technology does deal with modular or encapsulated systems, some of our interests force us to deal with whole

systems. The most salient examples are instances of what might be termed ‘bioengineering’—deliberate attempts to intervene to change the Earth’s biology. Eliminating smallpox or polio is not possible without operating on a global scale. But there is a difference here—in these cases we can study the effects of eliminating them at a local level even if to reach our goal we have to operate at a global level. Some people would argue that genetically modified crops represent a global intervention despite assurances that these can be treated as local interventions. But here too there is a difference—we have a wealth of experimental (literally, field) studies to fix the probability of such interventions ‘going global’.

There are many things about geoengineering that we could learn from experimentation. But they are restricted to the practicality of such interventions. In the case of sulfur insertion, we would need to learn about how to effectively deploy it as well as its half-life in the atmosphere. But could we ever have a basis for proceeding to deployment at scale with confidence?

In a non-encapsulated system in which no scale model can be built, the only experimental model is to extrapolate from low dosage to dosage at scale. But extrapolate using what mathematical model? At least in the case of sulfur insertion, I think there are two bases for such an answer. One is theoretical, the other empirical. The theoretical one depends on arguing a case based on our general knowledge of atmospheric chemistry. The empirical one depends on our knowledge of volcanic eruptions that have produced a record of short duration high density sulfur output. But the case remains to be convincingly made.

Suppose such answers are forthcoming, and suppose they support the conclusion that even if we cannot assume linearity, there is no reason to think there is a significant risk of discontinuities, let alone runaway states. Could we ever have the confidence to move ahead by slowly increasing the insertion dosage? Here I think there is a clear burden of proof argument for the proponents of geoengineering research. The great philosopher Donald Rumsfeld distinguishes between known unknowns and unknown unknowns. At issue here is not just whether the potential risks of geoengineering involve known unknowns rather than unknown unknowns, but just what the scale of those risks is.

4. Conditions on implementation at scale

Suppose this burden of proof is successfully met. Suppose the research is successfully conducted. Then what? What constraints should govern implementation? Geoengineering

will have uneven benefits because climate change not only has global effects but also has local effects (see Schneider 1996, 2008). Moreover, geoengineering itself may even harm some, making them worse off than they would be with global warming alone. For example, Robock *et al* (2008b) have a model that suggests that sub-Saharan Africa would suffer less cloud cover with geoengineering and thus be hotter and drier than it would be with climate change alone. Even if you prescribe a requirement for collective decision making, what weight should this likely ‘unfairness’ be given—especially if those who are hurt are the most far removed from those that caused and are currently causing the problem? Is this a wrong? If it is, can it be righted by compensation? Here it helps to extrapolate from a more mundane and common case. You live in a village that has voted to take some of your land to build a road. The road will allow crops to get to market with less spoilage and that will benefit the village as a whole. But you will lose land and have less space to plant your crops. The collective good justifies overriding your wishes and even your right to control your own land. But that said, it only does so if it is arrived at democratically, if you can be compensated fairly for your loss and if there is no less disruptive way to achieve the desired good. Here too is a burden of proof on the proponents of geoengineering.

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