Natural Science:
What we know, and what we ought to know

DOUG MACMARTIN: Thank you. We're going to reconvene for the second session on the natural science of geoengineering. And just as a reminder, we didn't talk about this very much in the opening session, but the ideas that you will hear, probably the idea that you hear the most about is stratospheric aerosols, which we have some understanding of from the natural volcanic analog, marine cloud brightening. So adding salt aerosols to low marine clouds to increase their albedo. Those are probably the most frequently talked about options for geoengineering, for solar geoengineering.

There are certainly other ideas that are on the table. So for example, cirrus clouds warm the planet. So if there are ways of thinning cirrus cloud cover, that would provide a small amount of cooling. And I am sure, and I think Scott mentioned this in the previous session, that as the research progresses there will probably be other ideas that are developed and that come out. In principle, you could also do something in space. But that's generally believed to be prohibitively expensive. So I'm going to keep my remarks very, very brief. We have a very august panel here. So solar geoengineering is a potential complement to doing mitigation and carbon dioxide removal, as Kate pointed out this morning, as an additional element in our portfolio of managing climate risks.

And I think it's very important any time one thinks about the scientific uncertainties and the risks associated with doing this, is simply to keep that counter-factual in the back of one's mind and thinking about research and thinking about risks, that we may wind up finding ourselves reaching 1 and 1/2 degrees within the next couple of decades. We've probably already come very close to exhausting the carbon budget for 1 and 1/2 degree target. So that's the context in which everybody is talking about solar geoengineering.

So the purpose of this panel, primarily what do we know about geoengineering from a combination of climate modeling and natural analogs? What are the uncertainties? So what do we need to know in order to get to a point where we could make some informed decisions about this, assuming that that's something we want to do? And how do we do that? What are the research that needs to be done to get there? So my introduction to this field was a meeting at NASA Ames in 2006. I think most of the panel was at that meeting. I don't need to say very much, because the panel members here have all been thinking about this problem for at least that long, and are generally involved in atmospheric science research for much, much longer than that.

So on the far end, Alan Robock is a distinguished professor in environmental sciences at Rutgers University. Joyce Penner, Ralph Cicerone distinguished University Professor of atmospheric science at University of Michigan. David Keith, actually the person that originally introduced me into engineering, Gordon McKay Professor of Applied Physics in the School of Engineering and Applied Science at Harvard, and also professor of public policy at the Harvard Kennedy School. Tom Ackerman is the director for the Joint Institute for the Study of the Atmosphere and Oceans and Professor of Atmospheric Science University of Washington. And then sitting next to me is Dan Schrag, who reminded me that he's a professor of geology as well as professor of environmental science and engineering at Harvard University, and also the director of the Harvard University Center for the Environment and the Director of Science Technology and Public Policy Program at Harvard's Kennedy School.

So everybody will have roughly five minutes for opening remarks. I'll give you a one minute notice. And I'll set the timer, so you'll hear a barking dog at five minutes. That was Rebecca's, my wife's, suggestion last night. And we'll try to leave most of the time for the panel for questions from the audience. So as people are talking, be prepared to come up with questions that you'd like to ask people. So I will, without ado, pass it to Alan.

ALAN ROBOCK: Thanks Doug. Thanks for inviting me here. I'd like to start off with an advertisement. If you haven't had enough geoengineering meetings already, David and Doug and I have organized the first Gordon Research Conference on Climate Engineering, which will be held this summer in Sunday River Maine. And it's time now to submit your applications. And we welcome you to come for a whole week talking about climate engineering. We have funding for students and early career people, and for some other people too. So we hope you'll consider joining us there.

Next, now a little history. You might think that this topic has only been studied since 2006, when Paul Crutzen wrote his famous paper. But David's been working on this longer than that and so has Harry Wexler. I don't I don't know how many of you are familiar with this paper in Science from 60 years ago. The topic was, the title was, modifying weather on a large scale, current proposals are either impractical or likely produce cures that are worse than the ailment. And he said, "when serious proposals for large scale weather modification are advanced, as they inevitably will be, the full resources of general-circulation knowledge and computational meteorology must be brought to bear in predicting the results so as to avoid the unhappy situation of the cure being worse than the ailment." So Harry was the chief scientist for the weather bureau. Very, very prescient. So we've been doing that. Next slide, please.

At this meeting in 2006, I was amazed that people thought how great an idea this was. And I wrote down 20 reasons why it might not be such a good idea. And now I have 27 reasons why it might not be such a good idea, and a few were benefits of outdoor research. And the number one benefit is that if you could do it, you could reduce global warming and all its impacts. And that might be enough for us to live with all the negative aspects. But how can we test these different things? And so with modeling, as Harry Wexler mentioned, this GeoMIP, this Geoengineering Model Intercomparison Project, we can answer the things that are in red here.

The next slide. So with using volcanic eruptions as an analog, we can answer some of these things also. So I don't have time to go through all of these. They're in my papers. And then the next slide. But there are some things that can't be answered either with climate modeling or with looking at volcanic eruptions as an analog. And this includes governance, ethics, and aesthetics. And so these are the things which are much harder to do. On the right we have unexpected consequences. On the left we have unexpected benefits. So how can we evaluate those? On the right, we have the moral hazard. If we did it, people would stop doing mitigation. The luck we have, if people find out about this, there will be more of a push to do mitigation. So this sort of balance each other. But how do we evaluate those.

Then there's the issues of military control of the technology. A giant multinational corporation being in charge. The idea that we will continue to do it forever, even if it's not a good idea. We have examples of military budgets in this country as an example of that. So building things that even the military doesn't want. Some of these things are much harder to wrestle with. How do we set the planetary thermostat? How do we agree on that? For you young people, this is how we used to set thermostats. How do we agree on that? And at the [INAUDIBLE] meeting that that Doug mentioned, Rob Socolow went around asking people, what would be the worst consequence of geoengineering? He did a poll on it. At the end of the week, he came up with a presentation. The answer was global nuclear war. So it's extreme. But there would be conflict, because people couldn't agree. So how we deal with these issues is much harder, I think, than whether we do small scale research outdoors, or research indoors. And these are things which are issues we really have to deal with.

So let me just show the last slide. Harry Wexler also said in 1962, "I hope that before we get into large experimentation that not only will the state of meteorological knowledge be more advanced than it is now, but also the state of our sociopolitical affairs as well." These are the issues we're dealing with here.

DOUG MACMARTIN: That was well done. Joyce.

JOYCE PENNER: I would like to, rather than emphasize some of the social issues that I think Alan talked about, talk about what actual things that are possible might or might not work, and why they might or might not work. So we have good knowledge that if we put particles in atmosphere and in particular in the stratosphere, we will cool the climate. Maybe there will be precipitation changes that we don't understand, but we will cool the climate. But I'd like to emphasize that even with the best instrumented volcanic eruption, which was Pinatubo in 1991, there are disputes about what actual temperature changes came from that natural experiment. So there are estimates in the literature that it was somewhere between as small as 0.14 degrees Centigrade, and as large as 0.4. So we have that uncertainty.

But more than that I think, the issues of how efficient a particular strategy might be are of importance. And the largest one, in my mind, is the question of, if you inject SO_2, that has to then convert into sulfuric acid, which then eventually forms particles. And when you form those particles, how large are those particles? If they get too large, then there's not as much reflection of solar radiation, whereas if they say just the right range, they might work quite effectively. So we're talking about field experiments in this panel.

And I think to actually find out the answer to this question not just relying on global models which after all have very huge grid resolutions. So they don't really resolve the microphysics that we need to know. We need to conduct a fairly large experiment. And in my paper I talked about something that is about a tenth of the size of the Pinatubo eruption. That's pulled out of the air. It's between 0 and 1. But I think it has to be a fairly large magnitude to really answer this question of microphysics and whether or not the removal from the stratosphere that is thought to be of order 2 years is actually done correctly in these global models.

Then there are other issues that are important. We know adding particles to the stratosphere results in a change in stratospheric ozone. How big will that be, since stratospheric ozone adds to heat the stratosphere that causes changes in circulation? How will those things respond especially at the larger injections that are sometimes talked about?

And then the final issue that I put down for stratospheric injection is, as these particles descend into the troposphere they have the opportunity to change cirrus clouds. So if you're adding more Cirrus clouds, you're actually heating. Whereas, if you're decreasing cirrus clouds, you can cool. And we don't know the answer to that. Let me go the last slide, because I have only one minute. And that is a marine cloud brightening. There are I think many, many issues around this technique. And those issues are things that we could solve with field experiments I think. Although they may have to get again very large. But every cloud is seemingly different. And what forces it to form or dissipate is different. And that means we have to go into different kinds of clouds and understand how susceptible they might be to adding sea salt particles.

And there are experiments that have been done. The [INAUDIBLE] piece was mentioned. And it showed that some clouds brightened, and some clouds dimmed. So I think that is definite evidence that we need to know more about this issue. I also mentioned the fact that we've done, and I've done particular, comparisons of how general circulation models respond to particles compared to how a cloud resolving model at 15 meter horizontal resolution would respond. And the answer is they can respond differently. So the cloud resolving model can heat while the global model cools. And so I think there are questions about how parametrization are done in these large scale models that make them not correctly predict what's going on in a more highly resolved model.

And then the last question is about cloud fraction, which there's still a lot of open questions about that whole response. So I have a summary, but it mainly, the point, since I'm out of time, is that I think doing these sets of experiments can lead to a slippery slope. Larger experiments are needed, larger experiments are needed. So to get into this whole experimental regime, I think governance agreement of some sort is needed. Thanks.

DOUG MACMARTIN: David.

DAVID KEITH: So the title of this session was what we know and what we ought to know. But I want to start out with some things that we think we know, or many people think we know in this community and literature, that I think probably just aren't so. So one of them is there's widely made claim that somehow this solar geoengineering could work for temperature but not for precipitation. So the literature is filled with claims that it disrupts precipitation in some way very different from what it does to temperature, or that it doesn't help ocean acidification, another very common claim. Or that it works for the rich, but somehow doesn't help or is systematically less effective for the poor. I'm pretty sure that all of those three claims as stated are just not supported by the literature or are just plain false. That's not to say it works magically, but those claims as I said them I think are really not true. And part of it is just getting serious about what we actually know.

So I'll just talk about the first and last of those. We've just completed some really, or are in the middle of some really interesting research using one of the best models that does tropical cyclones or hurricanes, the GFDL, very high resolution model working a bit with Kerry Emanuel. And what we discovered kind of in accord with Kerry's thinking is that even partial solar geoengineering, where it just is used to reduce some of the warming, has a very strong effect in dampening the strength of tropical cyclones, which are one of the most damaging effects of climate change, especially to the most vulnerable. And that's a clear example where if anything it sort of works better for the precip side in extreme storms than it does for temperature.

Precipitation over land. Part of the misconception has been simply due to poor treatment of statistical confidence in models. So if you ask if you do solar geoengineering to say cut the rate of warming in half, what fraction of the world's land surface has precip that is driven towards pre-industrial versus further away, that is it's made better or worse, if you think pre-industrial is the right answer. And if you throw away the points that aren't statistically significant, it's a very small number of points, if any, depending on the model where you've actually made it. And that really—part of that depends on the significance claim.

We've been looking a little carefully now at and including with some epidemiological models at how extreme temperatures hurt people and how that is connected with their income. And it's pretty clear that the poor are harmed the most, and that by reductions the poor would have the most relative benefit. I also want to talk about field experiments. So we are inching towards a field experiment. We're doing that for a couple years now, published an early version of what we call the Stratospheric Control Perturbation Experiment. And we're now beginning to move towards actually doing this thing over the next few years. I'll say a few words about it.

The goal is to make quantitative measurements of some of the aerosol microphysics and atmospheric chemistry that you need to estimate more about the risks and benefits of solar geoengineering in a large scale model. People often ask, why not just model it? But of course, models are completely dependent on a bunch of parameters which we've got from decades or centuries of observing the world. Don't just make these things in a vacuum. What we would do is use a balloon with a gondola in the middle stratosphere. The balloon would move it a few meters a second, and the propeller wake turns out to be crucial to this. It makes a well-mixed plume of air that would be a few kilometers long, and 100 meters in diameter, into which we can use as a kind of a vessel that we can make quantitative controlled measurements in the stratosphere.

We'll start by just injecting ice crystals, is the thinking. And the idea is that in that well-mixed wake, we can actually measure some of the key parameters to do with the particle-particle interactions that determine the kind of things Joyce was talking about, and their interactions with atmospheric chemistry. At least in our current thinking, the balloon operation will be provided by World View in Tucson, Arizona. So that's where we'll fly out of. I can say a few words about how we'll be governed. I think the first is to say that we, that those of us doing the experiment, and it's really me and Professor Frank Keutsch are the experimental leads. We completely agree that governance of solar geoengineering in general and in experiments is inadequate and contested. And our view is that an experiment like this can actually help to grow governance in a kind of bootstrap way.

We think there are three core kinds of governance we need. One is kind of hard governance of environmental health and safety risks, which has to be by an external agency. And there are a variety of external agencies that can do that. In practice, the amount of material we're talking about releasing, which would actually be less than a kilogram, is not big enough to, for example trigger a EIA under the National Environmental Protection Act, but we're going to look to see if we can get what's called a FONSI, a finding of no significant impact under that act. There are also separate checks and balances at Harvard and World View. But quite separate from that kind of the physical risks, we completely acknowledge that there are risks to do with just talking about or making solar geoengineering more prominent, the risk that it will embolden the fossil fuel industry to fight harder against emissions controls.

And so a core will be to build a scientific advisory committee, not just scientific, an advisory committee that is strong and independent and contains a really diverse set of views, including views that are very critical of proceeding down this road as a way to kind of code develop structures for governance. And part of the mandate of that advisory committee would be to recommend explicitly how governance might be improved. And I would say that to be clear, we are not unalterably committed to doing the experiment. Where we're heading down the road of doing that. And depending on what the advisory committee says, depending on what we learn technically. We are certainly willing to stop, because our long term goal is to build a sustainable effort in solar geoengineering research that allows us to tell more about ways it might actually provide public benefit.

DOUG MACMARTIN: Thank you. Tom.

THOMAS ACKERMAN: Thank you.

I represent a small consortium of scientists who are interested marine cloud brightening. I could take some agreement and some issue with each of my three colleagues, but I'm going to focus my remarks a little differently, because I'm going to focus on what would we do if we actually set out to do research. And I want to point out that in this slide you see these bright streaks in the cloud deck of the West Coast. Those are chip traps. Those are evidence that if you add small particles to clouds you make them brighter. So the real question for us in thinking about marine cloud brightening is can we engineer this, can we control that mechanism.

At the onset of all this, I want to point out that this is a dual purpose experiment. And I think Joyce for identifying one of the real uncertainties in all of climate science, which is the aerosol cloud interaction, which leads us to the problem of climate sensitivity and is one of the leading causes of climate change is in climate sensitivity and changes in what we see in climate models. So our experiment addresses that specifically in an experimental way. OK. In one sense, our experiment is simple. That's fine. In one sense, our experiment simple. Put salt particles in a cloud and see what happens. It turns out, like with many things, it's a little more complicated.

So I put up here on this slide the five processes that need to be looked at. You need to generate particles. You need to look at how those particles disperse in the boundary layer, because what you're trying to do is inject particles in a source and have them entrained into a cloud. You then need to look at what happens to the microphysics of that cloud. By microphysics we mean the number and size of the drops. If you introduce more nuclei, you end up with more cloud drops, all other things being equal. That's how you get brighter tracks. Then the clouds are just dynamically, due to this change in the microphysics, the microphysics is not independent of what the subsequent evolution of the cloud is or vice versa. So we need to understand that. And finally we get down to the bottom, which is what we really wanted to know. How do all of these changes actually end up affecting the albedo or reflectivity of these clouds?

So our proposal is to go out and do this, and we have a project outline. The first part is to develop the sprayer. We actually have a nozzle that makes particles of the requisite size. They have to be less than 100 nanometers in diameter, which if you're not in the nanometers, means are really tiny. We have to be able to generate something like 10 to the 14th or 10 to the 15th of those particles per second per square meter. So that's a whole lot of particles. It turns out the nozzle makes about 100 less than we need. So we're talking about a couple of hundred nozzles to form the sprayer. So the first stage is to build that sprayer. The second stage is to test that out along the coast and then out at sea. You say why would you do it on the coast when we want to do it at sea? It's much cheaper to do things on the coast than it is out at sea. And finally, there's a limited field area experiment that we're talking about, which is roughly on the scale of 100 kilometers by 100 kilometers.

So if you look at this progression, I want to inject some reality here into some of the discussions. Just to get this far in the diagram given enough funding and continuity of funding is a decade long project at least. We're not talking about something that we can do in six months. Coupled in with all of this just the modeling side that Joyce has alluded to. Because part of this is can you actually predict what it is that you're doing in an experiment. So there's a real interplay here between the model and between the experiments. That's always the way our field has worked. The one thing I want to point out is we're suggesting something different about atmospheric sciences. We're suggesting that we might become more like physicists than astronomers. We might actually start doing controlled experiments, which is what physicists have always done. The difference is we're doing them in the real atmosphere as opposed to isolating in the laboratory because nobody knows how to make clouds.

So let me end up by re-emphasizing three things. One, this is a dual purpose experiment. It provides basic scientific understanding that is useful to the climate modeling community as a whole. It is limited in scope. We're not talking about planetary effects here. We're talking about small scale effects. And finally it's environmental and benign. We're talking about spraying salt, we're talking about spraying salt over the ocean where there's already salt. And so I'll end with that. Thank you.

DANIEL SCHRAG: Thanks Doug. So first of all, I just want to thank Lizzie and Gernot and David and Ted for organizing this. I know how much work it is to organize a meeting like this. And I've got to say it's also just amazing to see everybody here, because I've been involved in these types of workshops and discussions for many years, and this kind of interest is very gratifying. It's great.

I want to bring us back to the real climate world for just a quick second. This is a picture of arctic sea ice now. We're in the middle of an extraordinary event, extraordinary event. And if you're a climate scientist, this is jaw dropping. 2012, what you see in the green, was a pretty amazing year where you saw an incredible loss of arctic sea ice. This is wintertime ice now. And the floor just dropped out. I mean we're scratching our heads trying to understand what the hell is going on. Clearly some of this is in response to the really big El Nino that we had. But even that doesn't explain this kind of change. As you see, this is just off the scale from previous wintertime sea ice coverage in arctic. The next slide.

This is Antarctica by the way. So didn't get a lot of press, but Antarctic sea ice bottom also dropped off the chart in terms of summer in the Antarctic, and what's happening. Just beyond any kind of previous record. I think there's something important that I think about that is important I think for everybody to understand. It's very likely that we've only experienced somewhere between 50% and maybe 2/3 of the warming that we are already committed to because of the long time scales in the climate system. This partly comes from the resolution of the paleoclimate measure of climate sensitivity and a short time scale measure of climate sensitivity.

And it's something models do very poorly at. They're not trained on this. It's not something you can get out of the models necessarily. There's a wide range of this. But what that means is that today we are already committed very likely to something like 1 and 1/2 to 2 degrees of warming. So this 2 degree, 1 and 1/2 degree, frankly to me it's a little ridiculous. We're already there. We're already committed to be there. It may take a century or 3 to get there. But we are already committed to doing that. Even if we froze emissions, froze atmospheric levels at current levels right now.

Another little fact that people should keep in mind, the warming over the land where we live is roughly double the global average. So if we're talking about committed to 1 and 1/2 to two degrees, it means we're committed to three to four degrees already over the land in which we live. Just to remember that. I was shocked by Rose's comments earlier in the social science panel, where she said that she has never seen a geoengineering scenario that was plausible to her. I would say the exact opposite. I would say that I have never seen a scenario with geoengineering that is preferable to one without.

THOMAS ACKERMAN: The other way around.

DANIEL SCHRAG: OK. Sorry. The opposite. Yes you're right. Thank you. I apologize. Thank you very much.

DAVID KEITH: Rose is smiling.

DANIEL SCHRAG: OK. Rose is smiling. Let me be clear. I have never seen a scenario without geoengineering that's preferable to one with. In that I don't think people understand just what we're up against with climate. And that's what I want to inject in this conversation. This is not, oh my god, maybe climate will be worse than we thought, and therefore we have to develop research on geoengineering as an option. The most likely scenarios for climate over longer time scales are devastating to future generation, absolutely devastating. And by the way, sea level is an even more depressing issue. We're not going to have time to go there. It's bad.

So this is a slide that just shows nine different questions that are required—that you have to answer—to get to a low carbon economy. Right now, we really can't answer any of them. Technologically, I would say that the probability of getting on a scenario that keeps CO_2 in the atmosphere under 600 PPM. If I had to evaluate the likelihood, it's extremely low. I don't think we're going to be at 900 by the end of the century. I think we're going to be in the 600 to 700 range. And we could talk about that.

But it seems to me that we're hearing a lot from people and especially in the social science panel about what world we might want. And that's wonderful. I'm trying to describe the world as I see it. And I want to make this very clear, I'm not an advocate for geoengineering. However, as a scientist observing the world. This is not the world I want to see. It is the world that I believe exists. And the most likely scenario is not one where many countries come together and do some geoengineering experiment, geoengineering effort to combat global warming. I suspect it's going to be lots of individual nations doing things in their own self-interest with some coordination.

And the challenge for governance and for science is to work both on the coordination but also the really difficult problem of attribution. Because if any country does this, they will get blamed for any weather event that happens regardless. And we have to be prepared to understand all of these different attribution issues which are going to get very thorny and very complicated. I think it's a very difficult climate problem. And I think we're in infancy of our understanding of this. And yet it is still, in every case that I've seen, better than the alternative of just letting the climate warm. It is not a substitute for emissions reduction. We still need to do that.

But given the trajectory of the world and the difficulty of reducing emissions, this is something we really need to understand now.

DOUG MACMARTIN: I'd like to make sure that we have lots of time for audience questions. I wouldn't mind just a very, very brief ability for all other panelists to comment amongst themselves on what they heard from their panelists. Starting with Alan. Your slides referred to what we could learn from modeling what we can learn from volcanic analogs. What the potential for whether we need testing versus sort of waiting for the next large volcanic eruption. And I don't myself know what the status is of observing networks for the next volcano—if a Pinatubo eruption occurred next year, would we learn what we would need to learn?

ALAN ROBOCK: Let me comment briefly on that and then ask a couple of the panelists one question if you allow me. Yes. The Pinatubo eruption in 1991 was not very well observed. Our satellites couldn't see through the cloud. There were no LIDARs that looked up from below. There were no balloons that went up into the cloud in the tropics. And so a lot of what happened as the sulfur gas converted to particles, how fast it happened was not observed. And so we have an opportunity to be ready for the next eruption, and I've worked on a panel with NASA and another one in Europe, can we deploy balloons at all the different latitudes and test them every three months. And then have a bunch of them ready to go up into the cloud when we have the next eruption, which we can't predict. Do we have the right things in space to measure these particles?

I think we can learn a lot about geoengineering, but also learn a lot about volcanic eruptions if we're prepared. I just wanted to ask David and Dan a question each. David, I went up to Harvard a couple of years ago to talk about this balloon project and you said that you're only going to do it if it was funded by the federal government. And now I understand, because you haven't gotten funding, you're going to be doing with private money. And I'm not saying that that's the wrong thing to do, but I'm just wondering how do you address people that say this is a slippery slope in your own program? And Dan, I mentioned that there could be a global conflict if people can't agree on what temperature we want. Don't you think that's a worse scenario than the I know global warming also produces conflict, but can't you imagine that as a bad scenario for doing geoengineering too?

DAVID KEITH: Great questions. Yes. So we had said that we wanted to do the experiment, we'd only do it if it was dominantly publicly funded. And we're changing our mind. So just to be clear about that. So the funding for this experiment, assuming we go forward as we're heading, is from internal Harvard funds that were basically the start up funds to me and Frank Keutsch. That's not start up as a company, this is completely noncommercial. And we have a strong policy against commerciality and IP and for transparency. But it is internal funds at Harvard that were ultimately philanthropic funds. And as well, there's a larger effort that is philanthropically funded, mostly from environmental philanthropies.

And so we have changed our view. And in a funny way, in the current administration I'm actually really happy about that. I have real concerns about this administration's potential interest in this technology. I think if the Trump administration both slashes research for climate science and also cuts action on emissions, if in that same sense, which it's not a given that it will do but it looks like, it was also going to advance research on solar geoengineering, I think the best policy for researchers on geoengineering might actually be kind of active resistance. Because to actually stand up against the administration. Because my view is the long term thing we need here is a broad and sustainable effort on research, which needs to have broad buy in from many parties around the world and needs to be international. And I think, if in that scenario that I suggested the environmental advocacy groups who I'm a part, would actively attack central research program arguing that it was just a distraction. So I think that wouldn't be a sustainable or sensible thing.

So in that world, I'm actually much happier doing this experiment with funding from environmental philanthropies or a sort of core Harvard funding, than I would be doing it out of new funds from the administration. But that still doesn't address the hard governance questions that actually a pretty independent funding. And it would come back [INAUDIBLE] in a different question.

DANIEL SCHRAG: So I'll just say very briefly, Alan, that in terms of conflict, it's something I've been thinking about a lot recently. I'm a post-doc working on conflict with climate change. And frankly my judgment is that the likelihood of conflict from resource scarcity and migration driven by climate change impacts is far, far greater. Now there is an issue about coordination and conflict related to control of a solar geoengineering system. And I think it's absolutely certainly an important question to ask, and an important question to look into. But if you look at the likely impacts of climate, sea level, impacts on food production systems, I just see it's incomparable in terms of the likelihood of conflict. And I think a lot of studies coming out of retired military folks would support that view as well.

I want to just also briefly comment on the observing systems that you mentioned. To me there is a fundamental problem with the volcanic experiment that is completely separate from the observing system. And yes, our observing systems do not measure the full spectral radiation budget of the earth very well at all. And it's a huge important thing not just for solar geoengineering but for climate in general. And so the same systems we need for solar geoengineering are observing systems we need for the climate. And right now, we don't have them. And therefore if China started solar geoengineering tomorrow, we wouldn't have very good systems to understand what they were doing and what effects it was having. And that's a tragedy. We need to improve our observing systems.

But the fundamental problem is that you can only do the experiment on the earth once. So with Pinatubo was also in the middle of a big El Nino, '92. And frankly, you will never be able to figure out how much of that was the El Nino of the cooling that was experienced, post El Nino, how much of it was Pinatubo. You will never figure that out, because the variability is so much, and the earth only has one—we've only done the experiment on the earth once. That would be true in a solar geoengineering world as well. You do the experiment and you try to attribute, good luck. Because you see an effect and you say, well do you believe the models or not? It's very tough.

ALAN ROBOCK: You're absolutely right. I was just talking about observing the microphysics of the formation of the aerosol cloud immediately after the eruption, not attributing the impacts.

DANIEL SCHRAG: But attribution is a killer. Yeah.

ALAN ROBOCK: Absolutely.

DOUG MACMARTIN: I have quick question for Joyce. There appears to be a slight difference in the expected scale of experiments between David's balloon experiment and your suggestion of a tenth of the Pinatubo. Relatedly—I was just wondering if you could comment on that.

ALAN ROBOCK: A few orders of magnitude.

JOYCE PENNER: Like I said, picking a tenth of Pinatubo was sort of out of the air. But my fundamental belief is that you can try to understand the microphysics in a smaller cloud. And you can kind of tune your models to get the same result. But in the actual implementation of a broad injection of SO_2, you would need a large enough injection that it was getting close to what you actually want to implement. And so that's kind of the slippery slope that requires some sort of governance in my view.

DAVID KEITH: There actually might not be much difference between us. I think the key thing was in actual implementation. So at least the way I see it, there's a bunch of microphysical parameters in models, and as you know better than me, you wrote some of the text. And we need to find ways to improve our knowledge of it. So for example, our experiment is all about that. And indeed, we're not just looking at sulfates, in fact we really aren't looking at suflates much. We've been thinking about other particles we might use that might reduce other risks. For example, calcium carbonate in the stratosphere appears to reverse ozone loss, because it reacts with some of the acids humanity put there, chlorine and nitric acid, and so pushes ozone back towards pre-industrial. So and also has other advantages that are not seeing the lower stratosphere. So some pretty significant advantages that we need to—

JOYCE PENNER: But might hurt cirrus clouds.

DAVID KEITH: Correct. And so there's a bunch of ways that we need to test that carefully, that are fundamentally microphysical experiments. So the way I see it is first of all is say the obvious. No one can test the large scale way the climate responds to forcing except by doing it. And even if you do it, you don't know the answer. So we have put CO_2 in the atmosphere, and we still don't know precisely the large scale response to CO_2, because of all the noise in the system. So once you start talking about implementation, there's no magic test that tells you climate response. But for micro physics, I think there's a way in which experiments want to be small. So I think to summarize, my view is that the natural scale for experiments is that you actually want to do a whole bunch of really small ones going at little micro physical aspects. And then, I think agreeing with you, if we ever get to a place where we actually, when again who's we, where there was some group that was moving towards deployment, then I think it's true that you'd start with some subscale thing and monitor it.

JOYCE PENNER: But my point is actually that small experiment is not going to teach you the microphysics or give you the confirmation in your model treatment that you want, if you want to eventually get to deployment. So this is the problem.

DOUG MACMARTIN: I think I'll just break in to say this is a very productive discussion to actually illuminate where we are in terms of the natural sciences on geoengineering. That to a certain extent, this is an emerging field. Like I used to say several years ago, slightly facetiously that we turn the sun down in climate models and noticed that it got colder. We're clearly beyond that, but there's still quite a bit to know. So Tom, I wanted to give you a chance to chime in as well.

THOMAS ACKERMAN: I think part of the way I focused my remarks was trying to differentiate between research and deployment. I think there's a tendency for us to start talking about research and leap to the endpoint of deployment. And if we don't separate that, we're going to get continuously confused about what we're actually talking about. And particularly in the kinds of things I'm interested in with clouds, we can do this at small scale. David can do microphysics at small scale. There is and there are these other questions about what's going to happen when we deploy, then how would we govern it? Well right now, we don't even know if it's possible.

So I would really like to separate those two things and say there's a difference in talking about research and talking about deployment. I have a paper, which we're in the second round of revision and hopefully will get published very soon with Steve Gardner who's an ethicist looking at what the ethical considerations are for testing. And how do you define test versus deployment? So I'm not suggesting these are easy questions, but I think it's one that we need to keep in mind.

I also want to follow up on Dan's comment about attribution, because this is something that, in my own thinking and modeling on this, I have run into. This is the wicked question without a doubt. And I would just add, for those of you who don't pay attention, our climate observing System is not steady state. Our climate observing system is rapidly running downhill. In five years, it will be far worse than it is right now.

DANIEL SCHRAG: Absolutely. And only accelerated if the White House budget were to come to pass.

THOMAS ACKERMAN: Yes.

DOUG MACMARTIN: So let's turn it over to audience questions. And as before we'll take three questions at a time.

AUDIENCE: Thanks very much. My name is Alex Hanafi. I'm with the Environmental Defense Fund here in Washington, DC. My question I think gets at one of the topics that Alan brought up early on about the challenges for discussions around solar geoengineering around where to set the thermostat. How you do that, and how you decide what that is. I'm curious as to whether you think and to what degree the Paris agreement has helped in that discussion. In Paris, the countries of the world agreed that they would limit warming to no more than two degrees and pursue efforts to keep it below 1.5. So to me, that suggests there is at least some spectrum of agreement, somewhere probably between 0 and 1.5 or maybe 2. And so curious about whether you think that has helped the discussion, and if so in what ways? Does it help limit the range of disagreement, and therefore make it a bit easier to resolve that question? Thanks.

AUDIENCE: I'm Rafe Pomerance. This question is with my arctic 21 hat on. I want to really support what Dan said. There seems to be a diversity of opinion. I really support Dan. I think we have to move this debate in a different direction. It would really help move research, I think which I support, strongly support. And that is to judge the need for this sort of research by looking at specific parts of the climate system that we have to have. With it we have lost already. Now I want to use three examples very quickly. First is the Arctic, which Dan touched upon with the sea ice slide. But it's reinforced by snow cover, loss, triggers to Greenland, permafrost thawing, and the loss of all the Arctic glaciers in Alaska and Canada, which are big contributors to sea level rise. Now the real world question is on May 11th, the eight foreign ministers of the Arctic nations gather in Fairbanks. OK. And one month before, the two weeks before that there's an IPCC assessment of the Arctic condition. So what would you like to tell the foreign minsters? What is the Arctic we have to have, and how do we get there? And maybe SRM or the cloud brightening, or cirrus thinning are part of that package. So when you look at the specifics, otherwise you lose the big pieces of global climate system i.e. permafrost, carbon storage.

The Great Barrier Reef is mostly dead now from coral bleaching. So climate call it ecological catastrophe is behind us. Third, Dan hinted that he's too depressed to talk about sea level rise, what do you do about the ice sheets? Can take the ice sheets under any scenario including geoengineering? Because the ice sheet issues upon us. Thank you.

DOUG MACMARTIN: I see Mike and Oliver and Janos. We'll take all three of those and then turn it to the panel, just in the interest of time.

AUDIENCE: Mike MacCracken from the climate Institute. I want to sort of follow on but generalize what Rafe said a little bit. Basically the notion that's been discussed in the science community is that I'm going to do research until I fully understand it and wait till I have full understanding. And I think we in the scientific community just know that's going to take a long time or forever. I mean, as David was saying we don't understand CO_2 fully enough right now. And so putting off what we do is just going to lead us to a worse and worse situation. So the other kind of approach is to basically start iteratively and learn. It's not one decision that one makes except that you're going to try and learn iteratively. And I guess I would think the ethical analog would be what happened with AIDS medicine, where researchers were basically well we're going to hold it off and test and test and test. And the AIDS people came in and basically said, well by then we'll be dead. And so let's move ahead and start doing some learning as we go. And I guess I'd like to hear the comment about do we wait till we know everything? Or do we try and work iteratively?

AUDIENCE: Hi. Oliver Morton. I was interested, a couple of you talked about this issue of attribution. And it's struck a chord with me with what Rose was saying earlier about always treating problems as gaps. Should you not perhaps consider, I don't know who on the panel would like to take this, the idea that actually don't worry about attribution? Attribution is not a problem. It's a wicked problem but you're not going to solve it. What you have to do is if you can't have a governance system that's robust against contested attribution, then you've got a big problem.

AUDIENCE: Janos Pasztor from the Carnegie Council. I'd like to come back to that issue of experimentation versus deployment from a governance perspective. Largely aimed at David, but maybe other members of the panel can also address it. So I get the concept of small scale experiments that you're trying to do now, micro physics, chemistry, dynamics etc. Yes. That's clear. And I could also envisage very nicely ad hoc governance mechanisms to make sure that those experiments are done correctly, and they buy in, and legitimacy and all that. The other end, I also get it that in order to really test the impacts on the climate system, you actually have to implement it. That's the only way we're going to find out with or without attribution, let's not get into that issue.

But the governance of that will have to look very different than the governance of the small scale experimentation that you are doing. So can we break this down a little bit in stages so that it doesn't become as simple slippery slope, but it becomes clearly identifiable stages so that we can also develop governance for those respective stages? Thank you.

DOUG MACMARTIN: Se have Alex's question, did Paris help in terms where to set the thermostat? Rafe, what would you tell the foreign ministers at the Arctic Council? Mike on starting iteratively and learning. Oliver's question on attribution, and Janos's question about stages doing research in stages to avoid a slippery slope.

DANIEL SCHRAG: I want to start on Mike's question very quickly.

DOUG MACMARTIN: OK.

DANIEL SCHRAG: Mike I think is exactly right. Mike McCracken said basically this distinction, I think the way I think about it, Mike, is that the mandate for research right now, the urgent need for research is not to perfectly understand it, because we never will. But when I look at the future, I see a substantial likelihood that some country will try to do something in this domain. And therefore, I want to try to understand this as much as possible before they actually do it. I don't see it as a slippery slope to aiding them doing it, I think if anything, and this is something I think David and I both agree on, that a substantial fraction of the money that we're raising for the Harvard solar geoengineering program, I don't know whether it's 20% or 30% or 50%, but a substantial fraction is going to go specifically to focus on all the ways that we could screw up. All of the ways that geoengineering might create problems. Because to me, you want to fund as much skepticism and as much research into the issues that you want to stay away from. I think we know, for example right now, that if you were to do it today, you would not do it with sulfur. That would be a very bad mistake. Even though a lot of the community still thinks of this sulfur. But I don't think that's true based on what we know of the chemistry.

But again, what we need to do is focus the research with the idea not that we are—we won't decide whether this gets used or deployed. We will try to understand it so that whoever makes the decision, whatever country or group of countries makes a decision, that it can be done with a better degree of knowledge. There are still fundamental things we don't understand and we never will. But the issue with land plants and hydrology and evapotranspiration is really complicated. And my suspicion is we'll never actually know exactly those parameters and models, the soil moisture and everything else. It's an amazingly complicated. I'll pass it on to Tom.

THOMAS ACKERMAN: I was going to follow up on Mike's question. And a little bit on this idea that if we don't understand everything, we can't do anything. We are doing an experiment right now that we don't understand. So let's not fool ourselves that we understand exactly what the climate system is going to do and next 50 years. So we're saying, oh, we don't understand that. We want to do research. And I would go to the AIDS analogy, it's not a bad one in this case, because I would say that the doctors already had an idea of what would work and how to make it work. And we have not got that same level of confidence in the ideas that we have, I think, as they had when they actually went towards. So I see this as an iterative process for sure. And I would say that we start with small experiments, and David's got a bunch of them and I've got a bunch of them, that lead on to bigger experiments. And we need to figure out how to do that. But I think we should disabuse ourselves of the idea that anything we do is going to make the world worse. We've already got a worsening world.

DOUG MACMARTIN: David, did you want to take one of the other questions?

DAVID KEITH: A few, but quickly. Oliver, as often happens, said just what I wanted to say, the idea that we magically have to solve the attribution problem or the technical problem I think is just nuts. So there are thousands of people, US citizens dying today from Chinese air pollution. We don't know precisely how to attribute it, and we don't have a legal system to deal with it. And life goes on. The fact is all sorts of things in international politics have spillover effects. We don't have perfect attribution. That's the world we live in. And the world just bumps along. So I think the idea that we need to magically know attribution is just a canard.

I also want to pick up, maybe it's obvious, but Rafe said that the exact what we need, is if there is a we and there's a fixed objective answer. I want to be clear that I think in terms of where to set that thermostat or what we need, these are inherently political questions. There's no objective answer about the right climate to aim for. I can tell you what I as a voter would vote for, partly because I like the high arctic. I'd aim to bring us back towards pre-industrial. But that's not the right answer. It's just the answer I happen to vote for. And the idea that there's some kind of magic threshold at two degrees C or something else that we politically agreed to, I don't buy. So from my point of view, these are deeply political questions and they should be, because they involve values in a deep way that we need a better, more inclusive conversation about those values and answer.s

And with regard to Janos's question about how to actually move towards governance, i think that we don't have an answer. And the right answer for the next few years is to simply have more open conversations that bring in a range of different options for what governance could look like. And I think the biggest mistake we would make would be to quickly making a kind of implicit assumption that there's one way to do this.

DOUG MACMARTIN: Joyce?

JOYCE PENNER: I wanted to address the question about experiment versus deployment. In my mind that is where the most important questions regarding governance lie. In other words, I am perfectly happy with Keith and Tom starting experiments at the level and size that they're thinking about, because I don't think that they're going to be disruptive in a way that is important to the whole conversation, for example, about whether or not and how much mitigation to start working on. But I think that there is a line somewhere between the size of Pinatubo and the current discussion of experiments that needs to be thought about and needs to have some framework in mind about when we cross that line. And that line is somewhere between deployment and experimentation. I don't know where it is, but I think that is the question for governance that needs to be addressed.

ALAN ROBOCK: I'd like to address two of the questions, first of all about the two degrees. That's not a global agreement that we will be happy with a two degree world. That's a pretty arbitrary level to shoot for so that people have something to shoot for in terms of mitigation. But as Dan showed, we're only at about one degree now, and the Arctic and Antarctic are already melting. Islands are going under water. And so they would not be happy with two degrees. The 10th arctic shipping conference is being held this week. So there are winners of global warming, and some people wouldn't mind a few degrees warmer.

So it's not a global agreement that we're ever going to get. People are thinking now about more regional geoengineering. Are there ways that we could control the climate in different places by different amounts? And there are computer simulations going on right now. Doug is doing some and I'm doing some. So it's not that we have to agree on one number. Maybe there is a scheme where we can look at more regional things by pulsing the emissions or brightening the ocean in certain places. So these are just early days. But there may be some ways that we could not just have to agree on one global number. But that's certainly a pretty arbitrary number that that's not an agreement.

DANIEL SCHRAG: Yeah. The big challenge for me is it something like how do you protect Greenland, ice sheet, but still allow Russia to have access to it's northern ports that they're very happy with. That's a really hard problem.

ALAN ROBOCK: And cover it with a reflecting sheet. The other assertion which a few people made is the only way we'll know how it works is by doing it. I don't agree with that. As we already discussed, we don't know how to do attribution, so even if we do it, how will we know whether the climate changed because of that or because of natural variability? And there are some things that are too dangerous to ever do. I mean the government I think is now planning to spend a trillion dollars over the next decade on modernizing our nuclear weapons arsenal without doing any testing even underground. It's all going to be done theoretically in computers. And so if we could have a millionth of that money to do geoengineering research, we'd be really happy. So some things you can't just do outdoors because we only have one planet. And so we may just have to be happy with our climate model simulations. After all, why are we even worried about global warming? We have no observations of the future. It's based on the same climate models.

DANIEL SCHRAG: I want more than a million, but that's OK.

ALAN ROBOCK: Well you can have a millionth and I'll take a millionth.

DOUG MACMARTIN: OK. We'll take two quick questions.

AUDIENCE: Joseph Majkut in Niskanen Center. One of the most interesting things that you all sort of variously touched on was that the questions you ask or the type of science you're asking to do when you're doing geoengineering research is a little bit different from what's been done in the past. Can you or anybody up there or paint a picture of 10, 20 years into such a program, how our knowledge might be different from today in useful ways? For instance, understanding decade to decade trends in global temperature, or the effect of global temperature on general circulation, as two examples.

ALAN ROBOCK: Let me just say geoengineering research is intimately connected to climate research and the enterprise that's been going on for a long time with weather and climate. And so you can't just separate it. And things we learn in specific targeted research will be helpful for all of these problems. And so, and as we advance our understanding of climate variability, we'll know what things might change that on purpose. So you really can't separate them.

THOMAS ACKERMAN: I disagree with that actually. We have been doing, and I'll go back to what I said earlier. What we're talking about doing is changing in some sense the way in which we interact with the system. We have been passive observers of the system. We are talking about injecting things into the system and asking how the system responds, which is much more basic physics and chemistry approach to the world than astronomy or geophysical sciences. I think there are things that we can learn at the small scale for sure. This whole question that Joyce put up, all the questions that she had about marine cloud brightening are basically with one exception on the small scale. That would allow us to do that experiment to compare the models to develop an understanding we don't have now by doing it in a controlled space.

So I think there are fundamental things that can shift our knowledge and this whole interaction of clouds. Alan and I've had this discussion before about what you can learn from a model and what you can't learn from a model. I've cut my teeth on an experimental side of the field, and I'm pretty sure there are things I can never learn from a model.

ALAN ROBOCK: But that's not what I said. All I said is that what you want to do comes out of basic meteorology and it's not coming out of left field. And as we, and I'm not saying you shouldn't do your experiment. I think you should. I'm just saying that it's all, that as things you, I think I'm agreeing with you. What you learn will help us do better modeling.

DAVID KEITH: I think, maybe in between is two points—as Alan said, research on this topic is intimately tied to research on climate and atmospheric Geosciences generally. And one of the things that means is because there has been so little direct attention to solar geoengineering, but it is building on this huge, more than a century of research in an atmosphere of Geosciences, I think that there's actually a chance for surprisingly rapid progress. That is a serious research program in a decade, I think could really tell us a lot. Because it's not starting from scratch. It's basically mostly connecting up a whole bunch of things that we half knew but haven't used for that application, plus doing some new focused experiments like the kind we're talking about. But even those experiments are building on the experimental techniques and knowledge that have been developed for decades.

DANIEL SCHRAG: By the way, you can look back at the last—since 2007, when David and I hosted that workshop at the American Academy of Arts and Sciences, I would say there's a number of important things that we know now, including for example that we don't want to use sulfur, I would put that at the top of the list, that we didn't know then that we know now. And that's a big change in just eight years. So projecting 10 years from now, I think we'll actually know a lot. Again, there will always be the potential for surprises. This is an earth system that is unbelievably complicated. But lest you think that's a reason to scare you away from going down this path, the potential for surprises in the real world without geoengineering I would argue is greater.

I would say the magnitude of the experiment we're doing with carbon dioxide in the atmosphere is so massive that the potential for really terrifying surprises is even greater. But all the more reason for us to use as many smart people in the climate community as we can to think of all of the possible ways this could go wrong, all of the—to me I personally am drawn to the asymmetries. Ways that geoengineering doesn't work, doesn't compensate, because that's—you want to probe those cracks. And that's what we need to do over the next decade is probe as many of those cracks as possible, and figure out just how devastating they are.

DOUG MACMARTIN: Thank you. On that note, thank you all. Thank all of the panelists here.