

End of The Line

The Innovation, Politics, and Governance of Climate Engineering

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Candidate's Declaration

This thesis contains no material which has been accepted for the award of any other degree or diploma in any university. To the best of the author's knowledge, it contains no material previously published or written by another person, except where due reference is made in the text.

Aaron Tang

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Abstract

Reducing greenhouse gas emissions isn't enough. Staring down the barrel of climate catastrophe, we are exploring new technological solutions to our climate quagmire.

Greenhouse gas removal (GGR) is needed to limit global warming to under 1.5°C or 2°C, thus meeting the goals of the 2015 Paris Agreement. However, GGR at the scale necessary is difficult and will likely take some time. Solar geoengineering, reflecting sunlight to quickly cool the Earth, can buy time and in the meantime help avoid the catastrophic risks of global warming. This thesis focuses on the most widely impactful and discussed reflection method - stratospheric aerosol injection (SAI).

However, foundational questions of their plausibility and desirability are unanswered. If removal and reflection prove implausible or undesirable, it may be time to acknowledge that we have reached the end of the line – that we have exhausted our options to effectively limit global warming and thus face climate catastrophe. This thesis is an interdisciplinary investigation of the plausibility and desirability of GGR and SAI.

GGR requires the rapid innovation and scale up of many technologies. However, the empirical evidence of large scale systemic technological change indicates a century-long timeline. Additionally, GGR requires large changes in supporting systems that will likely add delay. GGR also requires the development of multiple approaches and 'dominant designs', limiting acceleration effects of economies of scale. Given GGR is likely to err slow, we may have to research SAI to buy time for renewable energy and removals.

SAI research is controversial due to concerns that research is a first step on a 'slippery slope' to deployment. However, there is no clear understanding of how a slippery slope could happen. This thesis puts forward a typology of slippery slopes, emphasising the importance of creating early lock-in to prevent unintended runaway feedback loops.

Slippery slope risks are important due to SAI's deployment risks. However, holistic risk assessments of SAI deployment are limited, and do not consider deeper aspects of risk like compound hazards or systemic risk. This thesis develops a framework for holistic risk assessment, applied to SAI. SAI's 'average' outcomes are likely safer than climate change. But under unlikely but highly impactful system cascades and interactions with other global catastrophes, SAI can harbour risks worse than climate change.

Given such extreme risks, we can try develop policy that accelerates GGR as best as currently possible. This thesis focuses on Australian GGR policy, advancing four leverage points of a successful Australian GGR policy strategy: creating a government home for systemic coordination, leveraging cooperative and competitive federalism, ensuring trust in sequestration and offset mechanisms, and sparking innovation. But even with full implementation, these may not result in sufficiently quick GGR.

Interestingly, across history are rapid, large, and complex technological changes that broke the speed limit, like the Apollo Program or COVID-19 vaccines. These were the result of deliberate political forcing. Breaking the GGR speed limit is possible, but like any centralised forcing, will come at a severe and deeply unequal social cost. Whether this is worse than the risks of climate change or SAI is uncertain. Such risk discussions are unfortunately missing from extant GGR literature.

We are thus not at the end of the line. Limiting global warming, without resorting to risky SAI, requires forcing rapid GGR innovation and deployment. There is a narrow corridor to 1.5°C. Whether we should take it is another matter.

Preface: Thesis by Compilation

This thesis is a thesis by compilation. It is composed of an introductory chapter, a series of five linked papers, and a concluding chapter. Please note that referencing and heading formats vary throughout the different chapters due to different journal requirements. For consistency, this thesis uses the *Nature* reference style where possible. Below is a summary of this thesis' five papers.

Chapter 2

Title: Tortoises and Hares: How Science and Innovation Disrupt

Author: Aaron Tang

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Chapter 6

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Author: Aaron Tang

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On the following pages are the contributor's statements for my two papers with coauthors.

Statement of Contribution

This thesis is submitted as a Thesis by Compilation in accordance with https://policies.anu.edu.au/ppl/document/ANUP_003405

I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

Title: A Fate Worse Than Warming? Stratospheric Aerosol Injection and Global Catastrophic Risk _____

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Publication outlet: Frontiers in Climate _____

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Contribution to paper: Led conceptualization, drafting, and review. _____

Senior author or collaborating authors endorsement: Luke Kemp _____

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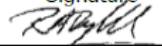
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I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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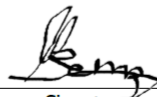
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Chapter 1: Shifting Tides

Climate policy has always changed.

What started out as mitigation saw the eventual, and initially controversial¹, need for adaptation. We couldn't reduce emissions fast enough, so we must now adjust to the impacts of global warming.

The 2015 Paris Agreement heralded another change, formally recognising Loss & Damage. As much as we can try improving adaptive capacity, some impacts are impossible to adapt to.

The individual pillars themselves have transformed too. Originally an issue of emissions graphs and economics², mitigation in reality has revealed itself to be a complex hydra of social acceptance, vested fossil fuel interests, and geopolitics^{3,4}. Adaptation scholarship has seen an increasing focus on preventing *maladaptation*, where climate responses actually increase climate vulnerability⁵. Responses to climate change have changed and will change again.

We are also in a period of technological change. The technological capabilities of humankind continue to grow, with alarmingly increasing speed⁶. Climate change itself is a testament to the world changing machine of human civilisation. Fire, sticks, and stones have given way to fossil fuels, urbanisation, and industrial agriculture.

Facing increasingly catastrophic risks from climate change, we are looking to further buttress our world changing capabilities. We are looking to leverage emerging technologies to deliberately and directly control the climate. Such emerging technological responses to climate change – greenhouse gas removal and solar geoengineering - are the focus of this thesis.

1.1. Greenhouse Gas Removal

Emissions reductions alone are insufficient to meet the Paris Agreement's target of limiting global warming to under 1.5 or 2°C. Lowering greenhouse gas emissions to zero is necessary to stabilise global temperatures. However, some sectors such as agriculture and aviation are highly difficult to decarbonise. Rather than 'real' zero, we must settle for 'net' zero. Net zero requires measures that can offset these unavoidable emissions. We require greenhouse gas removal (GGR).

GGR involves taking greenhouse gases out of the atmosphere, potentially at a scale where we reach a global state of 'negative emissions'. Removals are especially important to consider given the risks of temperature overshoot. We are on track to breach the 1.5°C threshold in the early 2030s⁷. In such a scenario, we would be wise to pull out as much greenhouse gases as we can, even if limited⁸. The risks of warming are simply too severe⁹.

Furthermore, the expanding collection of net zero targets all assume some level of GGR, albeit usually implicitly¹⁰. Making the needs and challenges of GGR explicit will best ensure net zero policies are rooted in substance, rather than just lofty promises¹¹ (the unfortunate current state of play¹²).

There is a wide suite of options available for removal, each with their unique pros and cons.

Planting trees, reforestation and afforestation, is an age-old method to remove carbon dioxide¹³. However, implementation is difficult because of land use concerns. Furthermore, imprudent selection of species and scale can compromise water security¹⁴.

Instead of trees, we can implement devices that directly suck greenhouse gases out of the atmosphere. This is known as Direct Air Capture (DAC). DAC's advantage is scalability¹⁵. It does not have the same land use limitations as planting trees. However,

DAC can have high energy and chemical demands¹⁵. Successful DAC deployment requires successful renewable energy deployment, and with plenty of energy to spare.

We can turn to industrial processes like bioenergy with carbon capture and storage. Through photosynthesis, bioenergy crops turn carbon dioxide into plant matter. Burning bioenergy crops would usually release the stored carbon, but specialised power plants can capture the released carbon for long term storage or use. However, expanding bioenergy crops can compromise food and water security^{16,17}.

Instead of industrial processes, we can consider soil-based or nature-based solutions. Biochar for instance involves heating biomass without oxygen to produce a form of pure charcoal, which can then be used as a soil amendment to improve agricultural productivity and carbon storage. However, soil carbon storage can be unreliable and will become increasingly unreliable due to climate change¹⁸. Enhanced weathering involves crushing rocks that naturally take in carbon dioxide over their lifetime, where the crushing of rock greatly increases their surface area and thus removal potential. However, poor implementation can risk leaving toxic trace elements behind, damaging soil and marine ecosystems¹⁹.

Beyond land, we can turn to the sea. At deep waters, we can consider artificial iron fertilisation. In the ocean are many tiny creatures called phytoplankton. Over the course of their lives and eventual death, phytoplankton take in carbon and sink to the deep ocean, where the carbon is (in principle) stored over the long-term. However, this can destabilise marine ecosystems²⁰, and global warming means that ‘escaping’ from warm upper layers of the ocean to the deep sea is less likely²¹.

These options are currently predominantly focused on carbon. Additional consideration for approaches that remove other greenhouse gases will be necessary, particularly for removal of greenhouse gases that warm on significantly smaller timescales.

Methane for instance stays in the atmosphere for roughly 12 years, but is a more potent greenhouse gas than carbon dioxide. Over 100-year timeframes (the usual framing), carbon dioxide is by far the most important greenhouse gas to address, simply due to the quantity that exists. But over shorter timeframes, like the timeframes we face for the impending 1.5°C threshold, removing other short-lived greenhouse gases like methane can be extremely impactful²².

The key factor for GGR is thus diversity. No single approach is slated to be able to remove enough greenhouse gases. Developing a ‘cocktail’ of approaches, informed by contextual social, economic, and political factors and comparative advantages, is the name of the removal game.

However, most removal approaches are at an early stage of innovation. The only proven mechanism for removal at scale is photosynthesis. Other options like DAC, bioenergy with carbon capture & storage, and biochar are at early demonstration phases²³, a far cry from large scale commercial deployment. Rounding off these issues is a need not only to accelerate existing options, but to create new ones^{24,25}.

With removal capabilities at such an early stage of innovation, a critical priority is accelerating innovation processes of GGR to allow for 1.5°C or 2°C aligned emissions trajectories²⁶. However, understanding and implementation of necessary GGR policy is currently limited, especially beyond the US and Europe.

This thesis addresses this gap, with its treatment of GGR focused on innovation acceleration, and the policy required to do so. This thesis discusses policy both within the context of current political windows, as well as at their most ambitious potential. It addresses an important geographic gap by assessing effective GGR policy in Australia, which has several comparative GGR advantages²⁴. These can best inform as wide a knowledge base as possible, which is critical to best manage the uncertainty inherent to technology and climate policy.

A first step to understanding acceleration is understanding the causes of delay. There is a rich innovation literature that can help answer this question²⁷, yet this literature has been relatively disconnected from the GGR literature. The most developed innovation literature for GGR frames innovation as a linear process^{23,26}, rather than a multi-sector, multi-actor, and non-linear process of experimentation and failure²⁷. Integrating such perspectives into the GGR discussion can provide useful insight into the key challenges and responses for timely greenhouse gas removal.

1.2. Stratospheric Aerosol Injection

All in all, there is no guarantee that necessary removal capabilities will be developed fast enough, especially given their early stage of development and complexity. In response, we may also consider solar geoengineering (also known as solar radiation management) - reflecting sunlight to quickly cool the Earth. This buys time for renewable energy transitions and greenhouse gas removal. There are different methods of reflection.

Clouds can be made brighter by seeding them with substances like salt, thus reflecting sunlight and cooling specific regions for a short period of time (substances injected at lower levels of the atmosphere fall back to the surface more quickly). However, this is best suited to a regional, not global response²⁸.

At the opposite side of the spectrum, we can place gigantic mirrors or clouds of moon dust in space to reflect sunlight before it even reaches Earth. However, these are prohibitively expensive and logistically difficult²⁹. Space industry and infrastructure is at a very early stage of development. Space mirrors are a long way off.

The focus of this thesis, and the focus of the extant solar geoengineering literature, is stratospheric aerosol injection (SAI). This uses a fleet of balloons or airplanes to inject substances like sulphur or calcium into the stratosphere, slightly and imperceptibly brightening the sky. Such particles would fall back to Earth, requiring the yearly (or more) reinjection of aerosols to maintain cooling.

SAI would be a truly global response to climate change – quickly cooling the Earth, with financial costs and logistical engineering demands within the reach of a minilateral alliance of sufficiently powerful countries^{30,31}.

However, solar geoengineering (and particularly SAI) is a deeply risky and controversial response to climate change. Interfering with the Earth's atmospheric processes would affect rainfall and drought dynamics, as well as change plant and animal distributions³². The possibility and impacts of cascading ecological side effects are very poorly understood, and heavily depend on the specific way in which an injection program is implemented^{33,34} (which itself hinges on deep political uncertainty^{35,36}).

Furthermore, there are concerns about democracy and controlling the 'global thermostat'. By nature, a limited number of actors would be making big and impactful decisions on the nature of Earth's climate, with subjective and contested winners and losers³⁷. Indeed, the global thermostat is not the only thermostat to manage. Changing injection quantity³⁸, location³⁹, and timing⁴⁰ can greatly determine the direct impacts of aerosol injections^{33,34}. Who decides what, where, and how, are foundational challenges of endgame deployment governance.

Due to the dangers of deployment, the research of SAI is controversial. There are many objections and controversies of research, ranging from underrepresentation of Global South actors⁴¹ to undermining current emissions reduction efforts^{42,43}. Furthermore, most scientific modelling implicitly makes highly optimistic assumptions, for instance worlds with a single central and rational central planner, or worlds with perfect international political cooperation over many decades^{44,45}. Most prominently⁴⁶, and the focus of this thesis, there are concerns that research is a first step to a 'slippery slope' towards deployment.

While there is a raft of concerns published in academic literature and public debate, there is little understanding of how these come together to inform a holistic risk profile of SAI. Such a holistic risk profile is important not only for better understanding SAI's own risks,

but also for allowing for comparative risk analyses with climate change. An ideal decision-making process would use SAI only if its risks were less severe than climate change⁴⁷. Such comparative risk analyses are notoriously challenging (distilling infinite possibilities into two clean easily comparable ‘alternatives’ is difficult), but a holistic risk profile of SAI is an important first step.

This thesis addresses this gap by conducting a two-prong analysis of research and deployment risks, and doing so in a holistic and systemic manner.

1.3. Research Questions

The discussion of the previous sections, as well as the aims of this PhD, can be summarised in two research questions and four sub-questions.

1. What are the prospects for implementing greenhouse gas removal at the scale and speed indicated in climate modelling and emissions reduction targets?
 - a. What are the reasons for delayed greenhouse gas removal innovation?
 - b. What are the policy and political settings that are necessary for sufficiently timely implementation?
2. What are the research and deployment risks of stratospheric aerosol injection research and deployment?
 - a. What are the prospects of a slippery slope from research to deployment?
 - b. What is the holistic risk profile of stratospheric aerosol injection, and how does this compare with climate change?

These questions altogether help investigate the plausibility and desirability of greenhouse gas removal and SAI.

GGR hinges on the plausibility of its speed. Can our gargantuan emissions task, already gargantuan even without the additional GGR challenge, be completed in the razor tight timeframes necessary to avoid catastrophe (Research Questions 1a. and 1b., explored in Chapters 5 and 6)? Plausibility is also important for SAI – can research that seems innocuous spiral to severe risks later (Research Question 2a., explored in Chapter 3) and are best-case ideal deployments that limit severe risks plausible (Research Question 2b., explored in Chapter 4), and what does this mean for SAI’s risks and benefits, as well as

the suite of responses we have for climate change? If SAI poses unacceptably extreme risks, are the risks of GGR more acceptable? (Note that SAI is itself clearly plausible because of its low engineering demands and cost, the issue here is the plausibility of doing SAI *well*^{30,31}).

Plausibility can be defined as being consistent with our background knowledge of society, politics, economy, and policy⁴⁸. Plausibility should not be conflated with likelihood. Something can be unlikely, yet still plausible. One example would be having a once-in-a-lifetime economic crisis followed by an even worse economic crisis caused by a pandemic 12 years later. This may well have been unlikely, but it ultimately was plausible.

Operationalising the study of plausibility involves hunting for empirical evidence and historical analogues ('has this or other comparable events happened before?') or identifying broad dynamics that are of relevance ('are there processes that support this happening?').

A second theme is desirability. This is most pertinent to SAI – with all the risks involved, is this a pathway we want to go on, or risk going on? For GGR, the issue is whether innovation and implementation at high speeds is desirable. A major issue in GGR literature is managing trade-offs and side effects⁴⁹. Rapid GGR deployment, like any hastily deployed infrastructural project⁵⁰, could exacerbate potential side effects instead of alleviate them (as explored in Chapter 6).

Desirability is far more subjective and difficult to define. By nature, any conclusion on the desirability of rapid GGR and SAI can be contested, assuming a clear answer on desirability is even possible. Regardless, desirability is still worth exploring. For the purposes of practicality, this thesis centres desirability around limiting global warming with minimal or tolerable side effects. This is intended simply for practicality as an anchoring concept⁵¹, not to assume away the subjectivity of what is desirable. Indeed, the subjectivity and intense controversy of both GGR and SAI make explorations of risk, side

effects, trade-offs, and concordant desirability more important. But even with all the subjectivity, some cases of desirability are easier to determine than others, and can guide investigation.

A key factor to consider is the potential presence of an alternative measure. For SAI, a key factor is the possibility of alternative pathway to avoiding catastrophic climate risks. If there is no alternative, then its risks may seem more acceptable. A plausible and less risky alternative, like timely GGR, makes SAI risks less acceptable. The likelihood of timely GGR would also affect SAI's desirability.

Risk comparisons can also aid discussions of desirability. SAI may very well be severely risky, but it may be less risky than climate change. As previously flagged, such comparative risk assessments are highly difficult. Using decision-making heuristics like maximin (choose the best worst case) can also help. See Section 1.6.2 for a detailed discussion of risk and how it is used throughout this thesis.

Lastly, it is important to point out that plausibility and desirability are interconnected. If the desirability of an idea hinges on implausible political assumptions and processes, then it is less desirable of an idea. Perfectly 'optimal' SAI that allays catastrophic climate risks with minimal side effects looks highly desirable, until it is revealed that the studies showing such scenarios make implausible assumptions like a single rational central planner.

This PhD thus interrogates the plausibility and desirability of emerging technological solutions to climate change – GGR and SAI.

1.4. Novelty: A True Stocktake

Altogether, this PhD is a check-up of the health of climate policy. We are exhausting options to effectively limit climate change. We need new options to respond to climate change primarily because we consistently fail to implement the old ones well. Climate change is a natural hazard, but climate catastrophe and our concordant desperation is

manmade. Such emerging emergency technological responses are necessary in principle, but are they plausible or desirable? Do these represent north stars for action, or are they false hopes that would lead us astray?

We may need these options in principle, but in practice they still may be desperate folly. This PhD investigates if they are.

Such an investigation is especially necessary because these options are key to limiting global warming to under 1.5°C or 2°C (and if we are to go by the original UN Framework Convention on Climate Change, the goal is to “limit dangerous anthropogenic interference with the climate system”). We therefore need at least one of the two emergency options provided.

And if they prove to be false hopes, then we will need to consider the possibility that we have reached the end of the line. The possibility that we have exhausted all options to comprehensively limit global warming, and face climate catastrophe⁹. In this case, we would need to reconsider the ‘goals’ of effective climate policy, the usefulness of frameworks like the Paris Agreement, and what we prioritise in our responses to climate change. If the emissions game is so handily lost, this would point to substantially more investment in adaptation and Loss & Damage (which have both been historically marginalised).

Reaching the end of the line could also affect the framing and communication of the climate challenge. Extant discourse focuses on temperature targets, particularly the 1.5°C warming threshold and net zero targets⁵². This reflects a strong emphasis in extant climate discourse for hopeful and positive communication, and to avoid being a ‘Doomer’⁵³. This also follows a trend of climate science to err on the side of least drama⁵⁴, as well as deliberate efforts by the Intergovernmental Panel on Climate Change (IPCC) to increasingly focus on low emissions scenarios⁵⁵ (even though they were initially not well supported by scientific literature^{56–58}). As gloomy as climate change appears to be represented, there has been a concerted effort to avoid focusing on its risks.

A holistic assessment that finds the end of the line could push back against the existing communication approach. It may be time to be explicit that the critical enablers of climate policy effectiveness, GGR and SAI, are implausible or undesirable. It may be time to investigate and communicate the systemic risks of climate change, which have thus far been dangerously neglected⁹. And it may be time to listen to the science of communication, which finds many factors that are far more important to effective communication than the strawman dichotomy of ‘hope’ or ‘doom’^{59–65} (like focusing on local tangible risks⁶⁶). To prepare for risks, we need to accurately communicate them. A first step could be to acknowledge that we are on a truly dangerously risky path, with only risky options ahead.

1.5. Methodology

These are big questions. Answering them requires a unique methodological approach that focuses on disciplinary diversity, systemic complexity, and identifying key leverage points. This allows for focused in-depth analyses without sacrificing a connection to realistic considerations of complexity.

The investigation of this thesis is further complicated by the fact that its core focus is on technologies, or technologies in certain forms, that do not yet exist. This makes the conventional application of the scientific method impossible. Common pillars of research, like data collection and concordant data analysis, suddenly become of secondary importance. These require the development of a unique methodological approach that ensures robust and substantive analysis when dealing with what are essentially imaginary technologies.

This section briefly outlines three methodological principles that guide the investigations of this thesis. They are foresight, a theoretical approach, interdisciplinarity, and the use of systems thinking and concordant leverage points. While these pillars are by necessity written separately, they were in practice heavily interconnected in framing the investigations of this thesis.

1.5.1. Foresight, not Forecasting

Why do we look to the future? There are two broad, non-conflicting, schools of thought⁶⁷.

We can look to the future to be correct. To make (probabilistic) judgements about what will happen. And if we're feeling supremely confident, we can even go into fully fledged prediction. Armed with knowledge of what *will* happen, we can adjust current behaviour. This is forecasting.

Forecast methods range from structured group exercises, like the Delphi Model⁶⁸, the use of financial markets⁶⁹, and quantitative modelling. While models have produced historically accurate forecasts of geophysical processes, like climate⁷⁰, they do not capture the political and policy dynamics⁷¹ that are core to this thesis. The best in class (or at least, one of the best) for forecasting political and policy dynamics would be the *Superforecasters*, whose forecasts of world events are more accurate than industry and political insiders with top-secret information. The Superforecaster method hinges on continuously updating quantitative probabilistic judgements in organised groups to quickly inch to a correct conclusion⁷².

However, a forecast approach would not be appropriate for the purposes of this thesis. Beyond a year, Superforecast accuracy drops to the point where random guesses become more accurate⁷³. Checking the reliability of long-term forecasts (and thus improving on forecasts) is also practically difficult, since you must wait until an event does or does not happen. Forecasts also do not capture non-linear processes well⁷⁴. This thesis discusses non-linear technological, political, and policy dynamics that extend far beyond a year, thus far beyond the useful range of forecasts. Furthermore, forecast approaches perform best on binary questions⁷⁵ - yes or no questions like 'will Donald Trump win the next election?'. The questions asked in this thesis, and indeed any complex wicked problem, cannot be meaningfully expressed in a series of binary questions. Forecasting suits shorter time horizon areas with plenty of data and repetition, like election wins⁷⁶ or

successful strategies in sport⁷⁷. The plausibility and desirability of non-existent technology is not one such area. Thus, an alternate way of looking to the future is required.

Another school of thought dispenses with the necessity of being accurate. Rather, it looks to the future to gaze upon, and broaden, the realms of possibility. Based on what *could* happen, we adjust current behaviour. This is foresight.

Foresight is far broader than forecasting. Foresight can range from structured group scenario creation (like the Three Horizons method⁷⁸), structured expert elicitation (used here in Chapter 4), to the creation of art⁷⁹. This thesis centres more on foresight.

Foresight takes a leading role because of the wicked nature of this thesis' questions. They are complex and big picture questions about the plausibility and desirability of technologies that do not yet exist, and what this means for the climate policy toolbox. This calls for a long timeframe and a wide scope. This thesis also hinges on asking difficult 'variable' questions⁷⁵. Questions which do not have simple yes or no answers, and which do not have clear upper or lower bounds (indeed, they are not necessarily expressible in a simple linear numerical manner). An example of a variable question would be 'How would Donald Trump winning the next election affect the long-term development of the Republican Party and right wing politics?'.

This in sum means that this thesis is not trying to look to the future with the overriding goal to be accurate. It gazes forward to identify important and impactful versions of the future, and based on this makes judgements and recommendations to inform in the present. These versions of the future may not necessarily be probable. Their probability may even be unknown. Yet they can still be useful guides on how to best handle the uncertainty of emerging technologies and eternally changing politics and policy.

Chapter 3 puts forth a typology of the ways in which research could lead to an undesirable SAI deployment. Chapter 4 explores the worst-case risks of SAI. Chapter 5

outlines the contours of effective GGR policy with regard to the limits of Australian climate politics. Lastly, Chapter 6 explores the possibilities for extreme GGR acceleration beyond current political limits. These chapters are not meant to be predictive, but they highlight highly impactful versions of the future that are used to inform practice and discussion in the present.

Important to note is that foresight and forecasting are not strict and perfectly crisp concepts. They exist on a spectrum. Even this thesis, focused on foresight, will at least implicitly involve some measure of forecasting. For instance, at the foundations of the thesis is an implicit forecast that climate change will continue to be a catastrophic risk in the years to come, or that policy will continue to be heavily driven by political interests and compromises.

1.5.2. Interdisciplinarity and Diversity

The sheer diversity of issue areas in GGR and SRM, or indeed any wicked problem, are simply too broad to allow for the application of clean disciplinary boundaries⁸⁰. These technologies have impacts on ecology, have social and political complexities, and demand new policy frameworks. A proper analysis of these technologies calls for analytical licence to jump across different disciplines. This thesis' epistemological pluralism does not seek to create a single unified theory of everything, but simply recognises the diverse values and limitations of different disciplines⁸¹. This thesis is an interdisciplinary analysis of technology, politics, policy, and society. It draws on a mix of science and technology studies, innovation theory, International Relations, policy and political analysis, risk analysis, and history.

Such interdisciplinarity is especially important early in a subject area's development. Early disciplinary assumptions can create path dependencies that close off important windows of discussion. For example, overuse of economic theory and concordant assumptions of discounting (the idea that the future is less valuable than the present), can lead to an undervaluing of future risks, as has happened in climate policy^{82,83}.

Interdisciplinarity is thus necessary to avoid disciplinary 'capture'. Individual theories

always have limitations. The key is to be able to integrate findings across many different perspectives to develop as comprehensive of a perspective as possible⁸⁴. Broader diverse interdisciplinarity is necessary to guide more narrowly framed or contextually specific investigations later^{85–87}.

Such an integrative approach is especially important in creating new theoretical frameworks that guide analyses of new emerging fields⁸⁸. For example, power and power-based political theories like Realism can help explain the emergence of new technologies, but not completely. You would also need to consider bottom-up acceptance (drawing on insights from social psychology), technical aspects of technological price-performance improvement (drawing on insights from science and technology studies), and more. Chapter 3 does this very task, creating a typology of how research can spark different feedback loops to technological deployment.

Discipline selection across the different chapters of the thesis thus takes an approach informed by grounded and adaptive methodology⁵¹: the problem at hand defines the use and selection of disciplinary knowledge. Furthermore, use of a certain discipline does not necessarily mean that the work produced is strictly a product of that specific discipline. Chapter 3 for instance draws on social psychology (due to the need to understand the dynamics of social acceptance), but this does not mean that Chapter 3 is itself a paper on social psychology. It *uses* insights from social psychology, and other disciplines, to answer its own research question. This allows for the use of diverse sources of knowledge, reflecting the complex problems this thesis looks to investigate.

To signpost the disciplinary journey throughout the rest of the thesis:

- Chapter 2 is an extended analysis of innovation theory and science and technology studies to assess how and why some innovations develop more quickly and are more impactful than others;
- Chapter 3 uses a legal typology of the Slippery Slope argument as a foundation for development of a broader Slippery Slope typology, which draws on economics, science and technology studies, social psychology, power dynamics and International Relations, policy studies, and lock-in and resilience;

- Chapter 4 creates a complex risk assessment framework that draws on disaster risk reduction, systemic risk, compound hazard analysis, and latent risk analysis to review the diverse impacts of SAI (ranging from direct ecological impacts, to systemic impacts on health systems and subsequent pandemic risk, to interactions with extreme space weather);
- Chapter 5 is an analysis of technology and innovation policy in the Australian policy context, utilising insights from political economy, Australian constitutional law, federalism, carbon markets and offset policy, and general policy studies; and
- Chapter 6 takes a historical account of rapid innovations (the Manhattan Project, the Apollo Project, and COVID-19 vaccines), and applies this to extreme acceleration of GGR as well as discusses implications for contemporary innovation theory.

1.5.3. Systems Thinking and Leverage Points

Diversity is necessary to capture complexity, but it can also be a recipe for impracticality⁸⁹. Capturing interdisciplinary complexity and interconnection is important, but research must stay practical. It must be simple enough to write, and more importantly, be elegant to read. This calls for an approach that can capture complexity in a way that is sufficiently simple but does not betray its substance.

The use of systems thinking can aid such an endeavour. Systems thinking is at its core identifying key variables (nodes), their interconnections, and how they change the nature of a system over time^{90,91}. Things happen not just because of new or external shocks, but also because of how a system is structured⁹². Thinking in terms of key variables and their interconnections provides the opportunity to create visual representations of complex systems⁹³ (see Figure 1 for a very basic example), especially useful to abstract and simplify highly complex and uncertain issue areas⁹⁴. This is especially novel for GGR and SAI, as both areas with high complexity and uncertainty, and which thus far have not seen the explicit application of systems thinking approaches (a trend unfortunately shared across policy and politics literatures⁷⁴). Two key characteristics are of note.

a) Variable A $\xrightarrow{+}$ Variable B

b) Variable A $\xrightarrow{-}$ Variable B

Figure 1 The principles of causal diagrams. Figure 1a) shows when variables are positively proportional, indicated with '+'. As Variable A increases or decreases, so does Variable B. Figure 1b) shows when variables are negatively proportional. As Variable A goes up, Variable B goes down (and vice versa). This figure does not show a loop since Variable B does not connect back to Variable A.

First, different nodes can amplify or dampen others, and can loop back to create causal feedback loops (see Figure 2). When nodes dampen each other, the feedback loop is balancing. For example, when the body warms, it sweats to counteract the warmth, keeping body temperature in equilibrium. Conversely, when nodes amplify each other, the feedback loop is reinforcing. Compounding interest is a classic reinforcing feedback loop. The more money one has in their bank account, and more interest they will earn, this increases the money in their bank account, increasing the interest they earn.

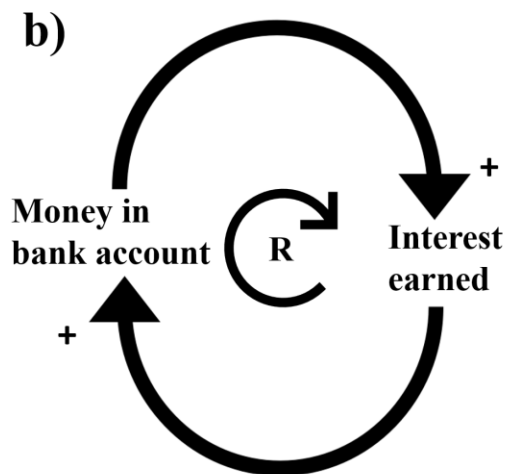
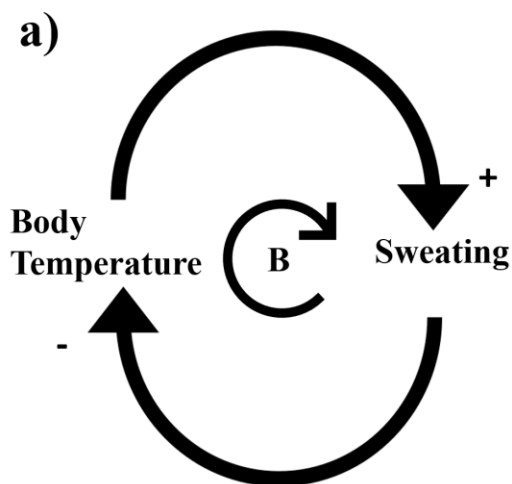


Figure 2 Examples of a balancing (a) and reinforcing (b) feedbacks in causal loop diagrams.

Feedback loops are critical because they are the engine of system stability and change. Where big picture politics, policy, and economy can seem like impossibly large machines to budge, a focus on feedback loops helps identify priority areas of action and analysis⁹². Indeed, history has a pattern of small changes that rapidly cascade into larger outcomes. Take the 2008 Global Financial Crisis as an example. Increasingly lax financial regulation led to an infection of predatory mortgage lending in the US, which cascaded into a housing bubble, which then cascaded into a global financial crisis that cost trillions to the global economy⁹⁵ (which was enabled with an overly interconnected international financial system⁹⁶). Identifying feedback loops can identify plausible pathways of change, and can explain why events in the past occurred why they did.

Second, some nodes will be substantially more interconnected than others. These can act as leverage points, where changing a singular node can affect the rest of a system. The Suez Canal was a critical leverage point of the global shipping, and blocking it just for a week created substantial costs to the global economic system⁹⁷ (again enabled with an overly interconnected commerce system with insufficient redundancies). Leverage points are important because they can help identify the most impactful and efficient areas of change. This is especially important to address highly interconnected problems like climate change, which affects and is affected by essentially everything. Focusing on leverage points can help identify the most important areas of investigation, and the most impactful levers to pull, rather than just addressing symptomatic issues⁷⁴. Meadows has famously put forward a typology of 12 leverage points (see Table 1)⁹².

Ranking	Leverage Point
1	The power to transcend paradigms
2	The mind-set or paradigm out of which the system—its goals, structure, rules, delays, parameters—arises.
3	The goals of the system.

4	The power to add, change, evolve, or self-organise system structure.
5	The rules of the system.
6	The structure of information flows.
7	The gain around driving positive feedback loops.
8	The strength of negative feedback loops, relative to the impacts they are trying to correct against.
9	The lengths of delays, relative to the rate of system change.
10	The structure of material stocks and flows.
11	The sizes of buffers and other stabilising stocks, relative to their flows.
12	Constants, parameters, and numbers.

Table 1 Meadows' ranking of leverage points, from most to least powerful.

Systems thinking and leverage points are reflected in the thesis as follows: Chapter 2 emphasises how incentivising innovation is not just a matter of focusing on science and research policy, but also creating a general environment for risk taking (leverage points 5 and 9); Chapter 3 explores how initial research can create positive feedback loops that lead to deployment (leverage point 7), Chapter 4 discusses the holistic risks of SAI and creates a causal loop diagram to illustrate SAI's broad impacts; Chapter 5 is based off the identification of several leverage points ranging from efficient federalism and coordination (leverage points 4, 6, and 10) to ensuring robustness and trust in GGR-related carbon offset systems (leverage point 5); Chapter 6 identifies political will as a leverage point to allow for extreme acceleration of GGR (leverage point 3).

Note that while causal feedback loops can involve quantification of variables, this thesis does not look to do so. The variables at play, for instance political power or social acceptance, are for now too difficult to quantify accurately. However, this does not preclude quantification in future.

1.5.4. Synthesis: Foresight, Interdisciplinarity, and Systems

Altogether, these serve as the theoretical anchors⁹⁸ of the thesis. They allow for the dual goals of practical analytical depth while also ensuring consideration of real-world

complexity and interconnections. Foresight is necessary for technologies like GGR and SAI, which extend far into an unpredictable future. Foresight, with its goal of expanding the horizon of potential futures, requires diversity of thought and discipline. GGR and SAI are also both widely impactful technologies, requiring contributions from many disciplines for robust assessment. These make diverse interdisciplinarity a critical tool in this thesis' investigations. However, the big picture scope of foresight and wide interdisciplinarity can result in impractical complexity. This calls for a systems approach which can help abstract and visualise complex systems, as well as identify key leverage points to focus on (imperative when navigating what is essentially infinite information).

1.6. Conceptual Framework

Complementing the methodological framework of the previous section, this section briefly discusses the key concepts that inform the analysis of this thesis. The methodology outlined how ideas will be used, and this conceptual framework outlines that ideas that will be used.

1.6.1. Innovation

One cannot discuss new and emerging technologies without discussing innovation. Innovation isn't just invention. Innovation represents multi sector and multi actor processes of developing new ways of doing and thinking. The technologies discussed in this thesis thus aren't necessarily even 'new' inventions. DAC for instance was originally designed for the International Space Station⁹⁹. But its innovation lies in a different conceptualisation of its use and concordant scale. Rather than being used to keep the International Space Station liveable, it is intended to be used at a much larger scale to address the climate crisis. This is still innovation – thinking of and doing new things.

We live in an age of innovation and progress. Unicorn companies – private startups with valuations of at least US\$1 billion – dominate and define modern industry. However, we should avoid getting sucked into the idea that innovation is a given. The systemic evidence of innovation shows us that most innovation is failure and a slow struggle^{100,101}.

The current revolution in artificial intelligence was preceded by multiple winters where research and industry essentially fell apart¹⁰². In addition to the bottom-up difficulty of getting new ideas to work, the Multi-Level Perspective emphasises how incumbent industries can derail rising innovations, as seen with the fossil industry and renewable energy transitions²⁷. Innovation thus requires deliberate support and acceleration²⁷. This is especially the case for innovations trying to address problems with a tight timeframe, like GGR to address climate change.

Supporting innovation calls for a broad range of policy approaches, instruments, actors. Long-term science funding is required to best develop high impact and disruptive innovations¹⁰³. Governments need to set directions to create the foundations of new markets¹⁰⁴, since free-market firms must prioritise survival in current markets¹⁰⁵. But innovation extends beyond science policy. Creating general environments for risk taking is necessary – a single research program or science park will have a limited impact if economic conditions create strong risk aversion^{106,107}. In this sense, and with a systemic frame of identifying leverage points, addressing surrounding policy issues (like Intellectual Property¹⁰⁸) in sometimes unexpected areas (like bloated real estate industries¹⁰⁹) can be just as if not more impactful in sparking innovation than purely focusing on direct innovation policy.

However, as much as innovation requires acceleration, innovation is not a universal positive. Innovation can harm^{110,111}. Innovators innovate not just to improve human wellbeing, but also to advance political and militaristic strategic goals. Algorithms to identify less toxic pharmaceutical drugs can also be used to identify more potent bioweapons¹¹². The use of different technologies can also change over time, sometimes without broader social or political debate¹¹³. What can start as an innovation to help can slip into innovation that harms. Responsible innovation¹¹⁴ is therefore just as important as timely innovation.

Innovation is central to any study of emerging technology. However, a proper conceptualisation of innovation is out of scope for this single introductory chapter.

Chapter 2 reviews innovation in greater depth, setting the stage for the technology analysis throughout the thesis. In particular, it helps provide a definition for how responsible innovation is defined for the purposes of this thesis.

In short, responsible innovation for the purposes of this thesis is ensuring GGR and SAI innovation is driven for the purposes of reducing the catastrophic risks of climate change, with a concordant discourse that fully represents the gamut of risks at play with GGR and SAI.

Chapter 3 explores the diverse ways in which research can lead to deployment. Chapter 4 discusses how changing politics dynamics likely preclude effective long-term technology governance. Chapter 5 discusses the broad range of policy approaches and instruments needed to support innovation (like ensuring trust in carbon offset policy). Chapter 6 emphasises the importance of *not* having an incumbent industry to battle, as well as the role of political goals in enabling strong innovation support. All chapters emphasise innovation risks, exploring the harms that new technologies or the nature of their specific deployments can create risk.

1.6.2. Complex Risk Analysis

To investigate the potential harms of technology, we need to interrogate the meaning of risk.

Risk is the potential for adverse consequences for human and ecological systems, and is a function of hazard, vulnerability, exposure, and response^{115,116}. A hazard is something that threatens to harm, for instance a heatwave, forest fire, or cyclone. These are becoming more frequent and intense due to climate change. Vulnerability is a representation of a system's propensity and susceptibility to be harmed by a hazard. Its informal antonym is resilience, the ability for system to cope with shocks and function in a same or similar way¹¹⁷. Disaster risk reduction generally focuses on increasing resilience¹¹⁸. Exposure is how much a system can be affected by a hazard (a town on the top of a mountain is not very exposed to sea level rise, but a coastal community would

very much be). Lastly, important to consider is that response is itself a source of risk. A response to a risk may fail to achieve its intended outcome or can create negative outcomes elsewhere. A lack of a response can also contribute to risk.

The idea of response risk is especially important for this thesis. GGR is a response that may fail to meet its promise of enabling low temperature emissions trajectories, if not implemented quickly enough. GGR, and especially SAI, can both create negative side effects in their implementation.

An important complement to response risks is the idea of risk comparisons and trade-offs. GGR may pose many negative side effects, but these may be preferable to the risks of climate change. SAI similarly may be preferable to climate change, but at worst could also create risks worse than worst case climate change. Risks do not happen in isolation, and should not be treated in isolation.

Hazards also should not be treated in isolation. Hazards can interact, affecting their probability or severity¹¹⁹. Though this thesis focuses on climate change, it is important to consider impacts and implications of other anthropogenic or natural catastrophes. Hazards do not create individual risks, they contribute to an overall *system* of global risk.

Hazard interactions may seem unlikely, but that doesn't make them unimportant. Indeed, a robust risk analysis will not solely look at likely outcomes. Low, or unknown, probability outcomes can be highly impactful. These 'tail' or 'deep' risks¹²⁰ should not be marginalised in risk assessments, especially for non-existent technologies where the realm of likelihood is still highly uncertain. Again, this thesis focuses on plausibility, not necessarily likelihood.

Importantly, large hazards and stressors are not the only hazards to consider. Smaller hazards are important too. They can amplify and cascade across different systems. Looking at the direct impacts of any hazard or stressor will show only a limited aspect of its risk profile. Assessing how risks cascade and amplify across systems, known as

systemic risk¹²¹, is important to a full risk profile of both climate change and its emerging technological responses.

Similarly to innovation, a full discussion of risk is out of scope of this short introductory chapter. This section puts forward the broad principles that inform how risk is understood and used in this thesis. Chapter 3 illustrates how initially small actions can create impacts that can cascade into later dangers. Chapter 4 includes a more detailed discussion of complex risk assessment, putting forward a framework for doing so. Chapter 6 flags how the risks of climate change itself are plausibly underestimated (due to its complexity) and applies climate risk trade-off and comparison dimensions to GGR.

1.6.3. Policy and Political Windows

Technology risks require the development of robust policy to minimise risks. Policymaking is not simply a task of centrally planned rational problem solving. Policymaking is solving problems in the context of shifting political interests.

However, the climate policy literature has a pattern of focusing on creating ‘effective’ policy instruments, rather than focusing on the more politically vexing questions of *how* or *whether* they will be used¹²². (This is despite broader areas of policy literature which are heavily connected with political considerations¹²³.) Analyses of ideal bottom up pledge and review treaty structures are sound in principle¹²⁴, but when exposed to real life climate geopolitics have resulted in a Paris Agreement that locks in dangerously slow incrementalism¹²⁵.

Policy instruments thus should not be discussed in isolation from politics. There is a critical interplay between solving problems and managing divergent interests¹²⁶. There can be disagreement over the appropriate approach or instrument, how it should be implemented, and the nature of a problem (including whether a problem exists in the first place). Policy implementation can also create strong political backlash¹²⁷. The name of the climate policy game is therefore about considering, and preferably actively building, alignment between policy approaches & instruments and political interests¹²⁸.

Indeed, many political scientists say that pure politics are the *primary* driver to consider. The main objective of political actors is frequently political survival^{129–132}. Problem solving is important, but this generally comes second. Political actors rarely commit to policy approaches or instruments which they think will result in their political demise. (Note that while political actors often commit to policies which result in their political demise, for instance Malcom Turnbull’s National Energy Guarantee in Australia or Theresa May’s entire economic policy in the UK, this is separate to intentional political suicide.) Since the prime objective of policy is political survival, it is in the interest of political and policy actors to pander to as small of a winning coalition as possible¹³³. Having a smaller winning coalition makes pandering more efficient, thus better guaranteeing survival¹³⁴. Political windows are thus strongly driven by (perceptions of) these winning coalitions^{128,135}. Policy can survive hurting people if it makes the right group of people happy.

There may be instances where the game of policy windows is bypassed. Crisis can create the conditions of “extraordinary policy-making”¹³⁶. The COVID-19 pandemic saw, in some instances, unprecedented investments in healthcare and social services¹³⁷. Crisis is also an opportunity, and climate change could be a crisis that allows for extraordinary policy.

However, crisis responses are not immune from unpleasant political flavour. There is a historical trend of governments that use crisis and emergency powers to expand their reach, known as the ‘Stomp Reflex’¹³⁸. China’s COVID-19 response was heavily driven by ambitions for expanding mass surveillance capabilities^{139,140}. The Patriot Act in the US was a response to foreign terrorism, but simply allowed for mass surveillance of US citizens¹⁴¹. In both these cases, the policy responses did not necessarily even reduce crisis severity or risk. Indeed, a recent review suggests that extreme COVID-19 responses potentially harmed more than they helped¹⁴². Crisis may open the door for extraordinary policy, but there is certainly no guarantee that such policy will ensure, or even target, risk reduction¹⁴³.

Understanding political windows is especially important to this thesis' discussions on plausibility. Discussion of policy instruments with no regard for political drivers limits its contributions to understanding plausibility. If political preconditions for a certain policy do not exist or seem implausible, that would make discussions on policy design moot.

However, there is a vexing analytical question of how to do such an integrated analysis when the political forces at play, and thus the potential political windows, do not yet exist. The analytical difficulties here do not preclude the importance of integrated policy-politics discussion. Rather, this thesis relies on two broad approaches to mitigate analytical difficulties.

First, rather than overly focusing on short term politics, which can quickly become out of date, this thesis where possible focuses on general political trends which can find relevance regardless of timeframe. For instance, the lack of political consistency at both domestic and international levels is a rule of politics that is not likely to be broken. Government departments can therefore be moved, abolished, or created (as discussed in Chapter 5) and international treaty participation is subject to changing political leadership (as discussed in Chapter 4). There are dynamics that are highly likely to last even through future uncertainty, and focusing on these where possible can provide useful guidance.

Second, this thesis where possible focuses on political windows and dynamics which are currently clearer. This for instance rules out a detailed discussion of international policy design – country positions on greenhouse gas removal and solar geoengineering are not yet clear (also confirmed via informal government engagement throughout the thesis), and their internal development is difficult to track from the vantage point of an independent academic. How these would affect the details of international policy design is, for now, largely a guessing game. However, the political drivers of Australian Labor climate policy are clearer, particularly from a Canberra-based academic with strong government connections. This is seen in Chapter 5, which focuses on Australian GGR policy in the context of Labor climate policy.

When discussing policy, this thesis looks to move beyond putting forward political vacuum ideas of a carbon price¹⁴⁴ or international geoengineering insurance scheme¹⁴⁵. Chapter 4 emphasises the importance of changing political windows in determining the nature of global policy. Chapter 5 for instance explicitly discusses its policy approaches in the context of an Australian political GGR window. Chapter 6 discusses ambitious policy beyond current political limitations, but does so within the context of extreme policy responses and the dangers that can follow.

1.6.4. Synthesis: Innovation, Risk, and Imperfect Policy

These together are the core concepts that are the foundations of this thesis' analysis. Innovation is needed to address global challenges, but complex systems like GGR require substantial acceleration and technologies like SAI can be strongly driven by political interests. Innovation can therefore also create new risks. Such risks should be understood in a systemic and holistic way, not just in terms of direct impacts (the usual focus). These risks thus require strong policy and governance, but these are severely limited by changing political will and political windows.

1.7. Methods

1.7.1. Literature Review

This thesis relies heavily on drawing from existing literature to synthesise insights and apply them to the emerging areas of GGR and SAI. This is done largely using targeted narrative reviews of literature. Narrative reviews are especially preferable for the purpose of this thesis, since they better allow for abstraction and application of ideas to different issue areas¹⁴⁶. This allows for strong interdisciplinary synthesis and argument. Systematic reviews, while a more comprehensive review of a particular issue area, are suited more to accurately summarising the findings of a field¹⁴⁶. Narrative reviews are also more efficient for the purposes of this thesis, which covers a wide range of different disciplines¹⁴⁷. A systematic review approach would not allow for such breadth. This means that this does not integrate every element of vast literatures like politics or

innovation, and it does not try to. It identifies key authors most relevant for the purposes of this thesis, and from there builds anchoring concepts to use.

For instance, Chapter 3 assesses the role of power in sparking or slowing change. A systematic review approach would involve reading and synthesising the vast corpus on power. This would be a task for an entire thesis, rather than a short section on a single paper. A narrative review of power allows for a quicker identification of key anchoring concepts, applying them to the subject area of research-sparked feedback loops, and doing so without making a claim of representing the entire corpus on power.

For another example, this thesis' treatment of innovation for instance is centred around the work, groups, and critiques of scholars such as Frank Geels, Jessika Trancik, and Vaclav Smil. These are not the only scholars of innovation, but they are most directly useful to the issue of technological pace. Other innovation and technology scholars, like Sheila Jasanoff, are important but are of less importance to discussions of technological pace.

The use of the narrative review is as follows: Chapter 2 in its entirety is a narrative review of innovation, Chapter 3 uses several narrative reviews (for instance on power, on extremeness aversion, on hype) to build a framework of how research sparks feedback loops to deployment, Chapter 4 is a review of SAI literature along its framework of complex risk assessment, Chapter 5 uses several narrative reviews (for instance on federalism, on carbon offset market design) to provide four priorities for Australian GGR policy, and Chapter 6 uses narrative reviews (of innovation delay and of the histories of the Manhattan Project, Apollo Program, and COVID-19 vaccines) to discuss extreme acceleration of GGR.

1.7.2. Expert Elicitation

Complementing the use of literature review is the use of expert elicitation. Expert elicitation is the synthesis of expert opinions in an area of high uncertainty. Best practice application of expert elicitation methods include dimensions such as a deliberate

selection of experts to hear from, detailed design in the specific framing of questions posed to selected experts, and integrate rounds of communal feedback and revision^{148,149}.

A full best practice application of expert elicitation methods was not possible for the purposes of this thesis for several interrelated reasons. First, the limited timing and resourcing of the thesis precluded an application of an onerous detailed method (in addition to the unavoidable literature reviews that were used throughout the thesis). The onset of the COVID-19 pandemic further limited the resources available, both from myself and of other potential experts. Lastly, given the novelty of both GGR and SAI, there is a highly limited pool of experts to choose from. A full expert elicitation process for Chapter 5 for instance would not be possible because the number of individuals with specialisation or interest in Australian GGR policy (nevermind expertise) would be in the low single digits. Application of an expert elicitation method would yield a general foundational discussion rather than findings borne from deep expertise. Answering much of the questions posed in this thesis would call for greenfield individual thinking, rather than the wisdom of the crowd.

Regardless, this thesis still can take principles of expert elicitation methods, namely the importance of structured thinking with clear targeted uncertainties to elicit perspectives on. The application of expert elicitation is seen in Chapters 3 and 4:

- Chapter 3 (using a series of narrative literature reviews) creates a typology of slippery slope mechanisms, which provided structure for the author's analysis and elicitation of the mechanisms of social and technological change. Chapter 4 (using a narrative literature review of risk) creates a framework for holistic risk assessment which provided structure for the authors' analysis and elicitation of the risks of SAI.

1.8. Positionality

Before proceeding to an outline and summary of the thesis, it is important to briefly discuss positionality. Unpacking one's world view and underlying personal and professional assumptions is necessary to make transparent any biases that may manifest

in the research¹⁵⁰. Note that I will not discuss every aspect of my identity, both for conciseness and because doing so would be infeasible.

I am partial to a big picture perspective. Being in Canberra, home of the federal government and to Australia's national university, further emphasises such a big picture frame. Experiences throughout the PhD like going to the UN, COVID-19 forcing a desktop research approach, and visiting the University of Cambridge (a highly privileged institution) also reinforce a perspective of the big picture and of the 'most' important issues. While this can be useful in identifying systemic interconnections, a core theme of the thesis, this can also marginalise on the ground perspectives that are crucial to just and equitable decision-making.

The potential impact of this is seen most starkly in Chapter 6, arguably the crux of the thesis. It discusses actions that cause bottom-up harms to avoid big picture climate risk. While risk trade-offs are an important perspective, they are not the only perspective to risk. Different benefits and costs do not necessarily 'add' or 'subtract' to create a 'net' value of aggregate benefit or cost (even though this is a core aspect of disciplines like utilitarian philosophy). In real life, they simply happen at the same time. Benefits for some also create and worsen injustices for others. Those injustices should not simply be marginalised in pursuit of the 'greater good'.

In recognising this, I have attempted to emphasise the pitfalls of such an approach, and explicitly do not recommend extreme technological accelerations that would hurt people on the ground. All I can say, being explicit with my many blind spots and assumptions, is that the nature of the risk discussion in GGR should widen. There needs to be a stronger connection between discussion of how to manage big picture risks, and addressing on the ground concerns and interests.

1.9. Thesis Outline and Summary

This thesis proceeds in Chapter 2 with a literature review on innovation, framing the analysis for greenhouse gas removal and SAI in later chapters. It reviews the general theory of how innovation 'works', as well as extant empirical experience with speed (or

lack thereof). This chapter also emphasises the interdisciplinarity of innovation. Innovation and new technologies are not solely technological studies, but at their best are interdisciplinary explorations

The foreword to Chapter 3 then briefly flags the likelihood of a slow GGR implementation (covered in greater detail in Chapter 6), thus necessitating an assessment of SAI.

A first step in assessing the risks of SAI is assessing the risks of research, and whether research is just a first step of a slippery slope towards deployment. Chapter 3 is an exploration of such slippery slope concerns, finding that despite being one of the most common academic and civil society objections to research, there is little consistency in what is meant by a ‘slippery slope’. This chapter puts forward a typology of slippery slopes; how initial research can spark different non-linear feedback loops that lead to deployment. Initial research can:

1. Spark price-performance improvements and sunk costs;
2. Normalise and legitimise initially extreme options;
3. Spark power dynamics and arms races;
4. Create hype, and;
5. Incrementally progress field tests until tests are essentially at scale deployments.

These have different likelihoods and applicability to SAI. Normalisation and legitimisation is for instance especially important for SAI, particularly since modelling studies consistently present an unrealistic best case vision with perfect political coordination^{36,44}. However, incremental field test progression is less likely, since even small physically innocuous field tests have not escaped intense political attention and controversy¹⁵¹.

Overall, the Slippery Slope is not a single monolithic concept. There are many ways in which research can lead to deployment, and this can affect potential endgame deployments and concordant desirability.

A strong complement to the risks of research would be to assess the risks of deployment. Research would be less controversial if the potential deployment was of less substantive importance. However, while many have raised the risks of SAI deployment, there has been little by way of a holistic risk profile.

Chapter 4 addresses this important gap by reviewing and synthesising extant literature, ranging from ecological impacts to political chaos to create a holistic risk profile of SAI. In sum, the worst-case outcomes for SAI opens the door to risks that are worse than worst case climate change. This happens in a world where injections are thick (ie. inducing more cooling), where there is a lack of robust backups, and where another catastrophe knocks out the infrastructure necessary to maintain cooling. In this instance, the rebound warming of a degree of cooling (or more) in a matter of decades is clearly worse than worst case climate change.

Herein lies the nuanced risk profile of SAI. In an ‘average’ world where unlikely political cascades and catastrophes don’t happen, SAI is more likely to be safer than climate change. However, the tail risks (ie. the less likely but more severe risks) are far more severe. In a single one-off instance, such tail risks may seem unlikely enough to neglect. Over multi-decadal, potentially over 100-year timeframes as defined by renewable energy and greenhouse gas removal timeframes, such tail risks then become far more important to consider.

With the presence of such extreme risks, we may want to simply accelerate GGR as best as possible within current policy and political systems. Country preferences of GGR are far too nascent to have an informed discussion of international policy, so a country specific focus is prudent.

Chapter 5 then is an attempt to explore the necessary policy to accelerate greenhouse gas removal, focused on Australia. Australia has key comparative advantages in GGR²⁴, thus making it a prime candidate for exploring ambitious GGR policy. This chapter also discusses such policy in the context of Australian climate politics, highlighting the

political windows to prepare for to best ensure timely development. This chapter puts forward four pillars of effective GGR policy:

1. Create a coordinating hub for greenhouse gas removal in federal government;
2. Leverage competitive and cooperative federalism among Australian States and Territories;
3. Ensure robustness and trust in existing sequestration policy to avoid unproductive polarisation of GGR; and,
4. Sparking Australian innovation by attracting international collaborations and building domestic innovation capabilities.

However, the fact remains that this will likely be too slow for the high demands of limiting global warming to under 1.5°C or 2°C.

Chapter 6 looks at extreme acceleration of GGR, going beyond the limits of current political windows. It begins with an exploration of delay, and argues that GGR implementation is likely to err slow. This is because:

1. Other transitions of similar scale and complexity usually take over 100 years.
2. GGR cannot be easily ‘plug and played’. It requires changes in broader technological (and social) ecosystems. Changing entire systems is more difficult than developing individual technologies.
3. The diversity of GGR approaches makes economies of scale effects less powerful. Reaching a ‘dominant design’ is important for fast development, but greenhouse gas removal by nature requires racing towards *multiple* dominant designs.

Chapter 6 then explores the most ambitious and extreme possibilities of acceleration (going beyond current political limitations). While most instances of complex innovations take time, there are examples throughout history that have seemingly broken the proverbial speed limit. Using the case studies of the Manhattan Project, Apollo Project, and COVID-19 vaccine development, this chapter highlights the enabling factors for the most extreme instances of acceleration. The enabling factors for extreme acceleration was not in the design innovation or technology policy, but rather overwhelming political will and the lack of a hostile incumbent industry.

However, such a rapid acceleration would worsen GGR's negative side effects. These risks must be integrated with the risk discussions of climate change and SAI. Rapid GGR's worsened side effects may be worth avoiding the catastrophic impacts of climate change and other extreme responses like SAI (though this conclusion is far from certain, not only because of the moral uncertainties but also because substantial improvements in decarbonisation efforts are needed). However, this would require a simultaneous supercharging of emissions reduction efforts, which does not seem to be on the horizon. Extreme acceleration of GGR without accelerating emissions reductions would hurt far more than it would help.

This thesis altogether explores the plausibility of desirability of GGR and SAI. Sufficiently rapid deployment of GGR within existing political systems is likely implausible. We may turn to SAI to buy time for renewables and GGR, but this presents extreme risks that hinder its desirability. So we may then consider extreme acceleration of GGR, but this would present many negative side effects. Whether these are worth averting the catastrophic consequences of climate change are deeply uncertain.

So we are ultimately not at the end of the line. We have not yet run out of all options to effectively address climate change. But we are close. Whether we want, or should, do what must be done to limit global warming is for now mired in a tragic mist of uncertainty.

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Foreword to Chapter 2: Tortoises and Hares

2.1. Place in the thesis

This chapter is a foundational review of innovation. It can be seen as an extension of the conceptual framework, but is itself also an important review that brings together the wide ranging innovation literature.

2.2. Key Conclusions

Innovation is not a single linear process of innovation through to deployment. Innovation cannot be framed as simply as ‘supply’ or ‘demand’¹. Innovation is multi-actor and multi-sector iterative process of doing new things². Indeed, innovation should not be conflated with invention. Using an existing technology in a different way can itself be innovation.

Since innovation is a complex phenomena of experimentation and failure, innovation often errs slow^{3,4}. Acceleration of innovation, of big systemic changes, is therefore critical, especially if innovation is needed to address pressing global problems⁵.

Encouraging and accelerating innovation requires pulling policy levers that may be beyond the remit of usual frameworks of innovation policy. Everything from a domestic real estate industry to grand geopolitical ambitions can be key determinants of effective or ineffective innovation policy.

Lastly, and perhaps most importantly, innovation is not always positive and is not always in the service of the public interest⁶. Many of the severe risks we face today, whether it be mass surveillance or climate change, are unfortunately a product of innovation⁷. Innovation is a double-edged sword.

2.3. Significance to the thesis

This review of innovation sets the stage for the technology analysis across the thesis.

Emphasising the wide-ranging policy levers of innovation, going beyond science and research policy, is a foundation to the interdisciplinarity seen throughout the thesis and particularly informs the design of policy in Chapter 5.

The framing of innovation as a complex process of experimentation, struggle, and delay is an important foundation for GGR, which is likely to err slow given its complexity and necessary scale (a key aspect of Chapter 6).

This chapter's treatment of responsible innovation is important both for GGR and for SAI. They may be necessary for climate change (either to enable low temperature emissions trajectories or to buy time to develop and implement new technologies), but that broad principle of necessity does not guarantee that the execution of GGR nor stratospheric aerosol injection will be in the public interest. Ensuring they act to help rather than hurt will require concerted effort.

Chapter 2 highlights the issue of Function Creep, that innovations can be developed and used in ways beyond their original intention (sometimes for the worse). This forms a core aspect of responsible innovation for the purposes of this thesis: innovation in the context of GGR and SAI should be developed and implemented for the purposes of reducing catastrophic climate risk for broad societal benefit, not primarily for ulterior political or economic motives.

In addition, Chapter 2 outlines broad principles for ensuring responsible innovation: being anticipatory, reflexive, inclusive, and responsive. Of particular importance to this thesis, given GGR and SAI's early stages of innovation, is being anticipatory. Early assessment of the risks of GGR and SAI are necessary, and such risk assessments should be conducting in a holistic way which captures interconnectedness and complexity. For the purposes of GGR, the risks of accelerating technology should be considered in conjunction with the risks of climate change and other extreme responses to climate change (as argued in Chapter 6). For SAI, research and discourse should aim to

encompass the full gamut of risk at play, not just the easier to measure direct impacts or idealised best case scenarios (as argued in Chapters 3 and 4).

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Chapter 2: Tortoises and Hares

How Science and Innovation Disrupt

For the Australian Academy of Science

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1. Moonshots and More

1959. The first integrated circuit is invented. It is a large square tablet, about the size of your torso (Figure 1).

1969. The Apollo Guidance Computer has just guided humanity's first moon landing. The Apollo Guidance Computer is built from integrated circuits, now much smaller and more efficient.

1998. Texas Instruments releases the TI-73 calculator. Its processor is 140 times faster than the Apollo Guidance Computer¹. The TI-73 is designed for high school math.



Figure 1 The first integrated circuit, with inventor Robert Noyce in 1959. Noyce co-founded Intel in 1968.

Today, the smartphone in your pocket is magnitudes more powerful than the TI-73. Its uses include everything from running a business to finding love.

The meteoric rise of integrated circuits, what we know as computer chips, is a gold standard for disruptive innovation. They are a product of both basic and applied science. For better or worse, these silicon squares have extended human capabilities far beyond previous horizons. They are the bedrock of modern society, of personal computers that allow decentralised accessible entrepreneurship and a strong creator economy. But also of mass surveillance programs and lethal autonomous weapons.

Integrated circuits' rapid improvements are tracked with Moore's Law: processing power doubles every two years. Interestingly, Moore's Law tracks the performance improvements of many other technologies too. Nagy et al.'s statistical analysis of 62

diverse technologies show a surprisingly consistent adherence to exponential improvement². New technologies and innovations hold tremendous potential.

But most innovation fails. Early survival is difficult. Getting innovations off the ground is a critical priority. Analysing how and why some become rapid and large disruptions (Hares) and why some lag (Tortoises) can allow us to better understand how to support the new horizons of society. Indeed, facing pressing catastrophic and existential risks like climate change, knowing how to catalyse rapid shifts is necessary to address society's most pressing needs.

Such rapid shifts are not limited to the invention of new technologies. Innovation isn't just invention or basic science (a common conflation). Innovation isn't always technological. What may be boring common practice in one context can still be highly innovative in another.

This review canvasses over 30 years of innovation study, synthesising different perspectives on how and why science and innovation prove disruptive (or not).

Innovation is more than invention, and encouraging innovation is about hitting leverage points in politics, society, and economy, rather than just targeted changes in science and research policy. Indeed, innovation is always political. Innovation does not always provide broad social benefits, and often harms. Science-policy has critical roles to play in taking a wide scope in its policy advocacy, as well as enabling substantive and productive societal debate of innovation directions.

We begin with a systems discussion of innovation. Innovation involves multi-actor, multi-sector processes of experimentation, scaling, implementation, and adoption. A robust canvas of innovation requires a holistic view of entire social, economic, and political systems ([Section 2.1](#)).

But across the complexity and panorama of innovation, the simple name of the game is how to responsibly support risk taking and new ventures ([Section 2.2](#)). This can be especially difficult since countries, businesses, and people can justifiably be deeply preoccupied with short term survival ([Section 3.1](#)).

Innovators therefore innovate for different reasons – some try to break open new markets, others try to optimise existing ones ([Section 3.2](#)). Innovation itself is similarly diverse. Innovations can explore the fundamental questions of the universe as well as practical real world improvements. These two goals are often framed as differential, but they are tightly linked ([Section 3.3](#)). Rounding off this diversity is a discussion of who innovates. Governments and the public sector play a crucial role in being the first movers of innovation ([Section 3.4](#)).

This diversity means that what we see as innovation policy must be similarly diverse. The conventional focus is on the research and education system ([Section 4.1](#)). But doing the ‘boring’ work in building a welcoming business environment ([Section 4.2](#)) and ensuring effective regulation in areas like intellectual property is key to unlocking an entire innovation *system* ([Section 4.3](#)).

However, innovation isn’t always positive. Innovation directions are often driven by political and strategic interests, rather than bringing broad societal benefits. Innovations can cause harm. These risks should be a core focus of any innovation policy ([Section 5](#)).

Ultimately, innovations most likely disrupt when overarching social, economic, and political systems prepare for them – when they create risk taking environments and hit at leverage points beyond just science and research systems. The nature of disruption is thus not subject to pure randomness, but is often subject to contested social and political goals. Herein lies the blessing and curse of innovation. What and how we disrupt is partially under our democratic control, and partially not.

2. Innovating, Fast and Slow

2.1. The Slow Struggle

Innovation is a bloodbath. Change doesn't come easy nor quick. The transition from candles to kerosene lamp was on the order of 50 years³. This is a relatively quick transition – there was a lack of candle 'industry' to lobby policy, and candles were simply outmatched by kerosene lamps. They were brighter and lasted longer.

Yet a 50-year time frame would still be too slow for the rapid changes that we now require. Furthermore, we aren't necessarily concerned with the rise of an innovation in isolation. We are concerned with shifting entire systems.

Figure 2 looks at global transitions of entire industries, framing the transition 'threshold' at providing 25% of global supply⁴. Peak dominance is another milestone still.

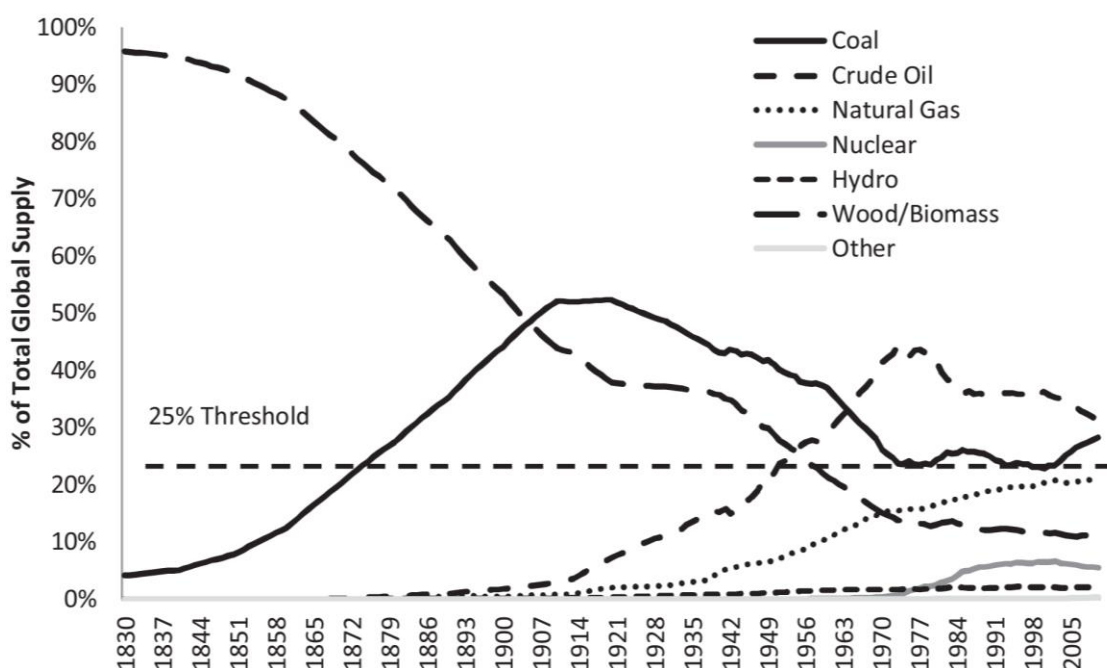


Figure 2 Global Energy Supply by Fuel Source as a Percentage of the Total, 1830–2010⁴.

Coal passed the 25% threshold in 1870. Another 50 years was required to reach its peak.

This was over 500 years since the first *commercial* coal mine in the UK. This does not include the decades of science and experimentation that came beforehand.

Crude oil passed the 25% threshold in 1950. This is about nine decades after Edwin Drake drilled the first commercial well in Titusville, Pennsylvania in 1859.

Smil's statistical analysis of system level innovations puts the expected timescale at 90 years⁵. There are deeper nuances regarding the 'start' and 'end' date of innovation. Earlier stages of innovation, like ideation, are impossible to capture in empirical studies. Regardless, the end message is clear – large scale system change takes place on a timescale of roughly centuries. This is driven by a number of factors:

1. Failure is inevitable. Experimentation simply often results in failure. It takes time for the rarer successful innovations to arise.
2. Existing industries may have vested interests to oppose new disruptive innovations.
3. Societies may not trust new innovations and resist adoption (or exhibit slow speeds of adoption).
4. Innovation success isn't just about any specific innovation or technology. Entire systems must be built around and for them. Adopting an innovation also involves pulling along industrial, technological, policy, and societal systems.

Indeed, the broader systems are one of the main determinants of how fast a change occurs. If changes in the broader ecosystem are needed, innovations will face a more delayed and turbulent road to adoption^{6,7}. If an innovation can be easily plugged and played in existing technological systems, changes can happen relatively quick.

Energy efficient lightbulbs for instance are easy to adopt and implement. All that is required is to unscrew the old bulb and screw in the new bulb. No changes in broader urban and energy systems required.

Quick implementation still applies even at larger scales. Implementation of solar geoengineering, specifically stratospheric aerosol injection, is not projected to be onerous for similar reasons. The main practical development necessary is simply modified

aviation tankers⁸, not dissimilar to automated drones already in use by modern militaries. This could fit relatively easily in contemporary aviation systems.

Conversely, the pathway of the HD-TV was fraught with delay. HD-TV was first developed in the 1960s by the Japan Broadcasting Corporation⁹. But scale up and widespread adoption only occurred in the early and mid-2000s. This is because the HD-TV required broader improvements in video capture and transmission¹⁰. There was no point in buying an HD-TV if the video you were watching was captured on standard definition cameras, or if video compression standards changed HD video to standard definition. HD-TV only took off when broader ecosystems shifted to fit the HD-TV's needs.

The most critical contemporary transitions, namely transitions to renewable energies and the rapid research, development, and deployment of negative emissions technologies, require substantial shifts in their broader ecosystems. Renewable transitions for instance require substantial changes in charging and energy grid systems¹¹. Negative emissions technologies require developments in storage, monitoring, and carbon transport¹². The most important transitions face many hurdles. If allowed the conventional route of slow innovation struggles, they will fail to deliver the timely outcomes necessary to mitigate catastrophic climate risks. Rapid acceleration is necessary.

This paints a difficult picture of innovation, but there are still further difficulties that these and other innovations face. Namely, an existing ecosystem is not limited to the technological or the scientific. The ecosystem at social, economic, policy, and political levels also require shifts. A holistic and multi-sector approach to innovation governance is necessary to best unlock the value of science and innovation.

2.2. Accelerating Complexity

How can innovations at a system level be accelerated? The Multi-Level Perspective (MLP) is a widely used framework in academic literature^{13–16}. It splits analysis along three layers:

1. Niches - small, protected spaces for innovation,
2. Regimes - existing sociotechnical systems,
3. Landscapes - broad contextual factors.

At the core of the Multi-Level Perspective (Figure 3) is a narrative for how new innovations compete with, and sometimes replace, existing regimes.

Foster innovations to take advantage of windows of opportunity

Internal and external forces pressure the existing system, which can realign around maturing innovations

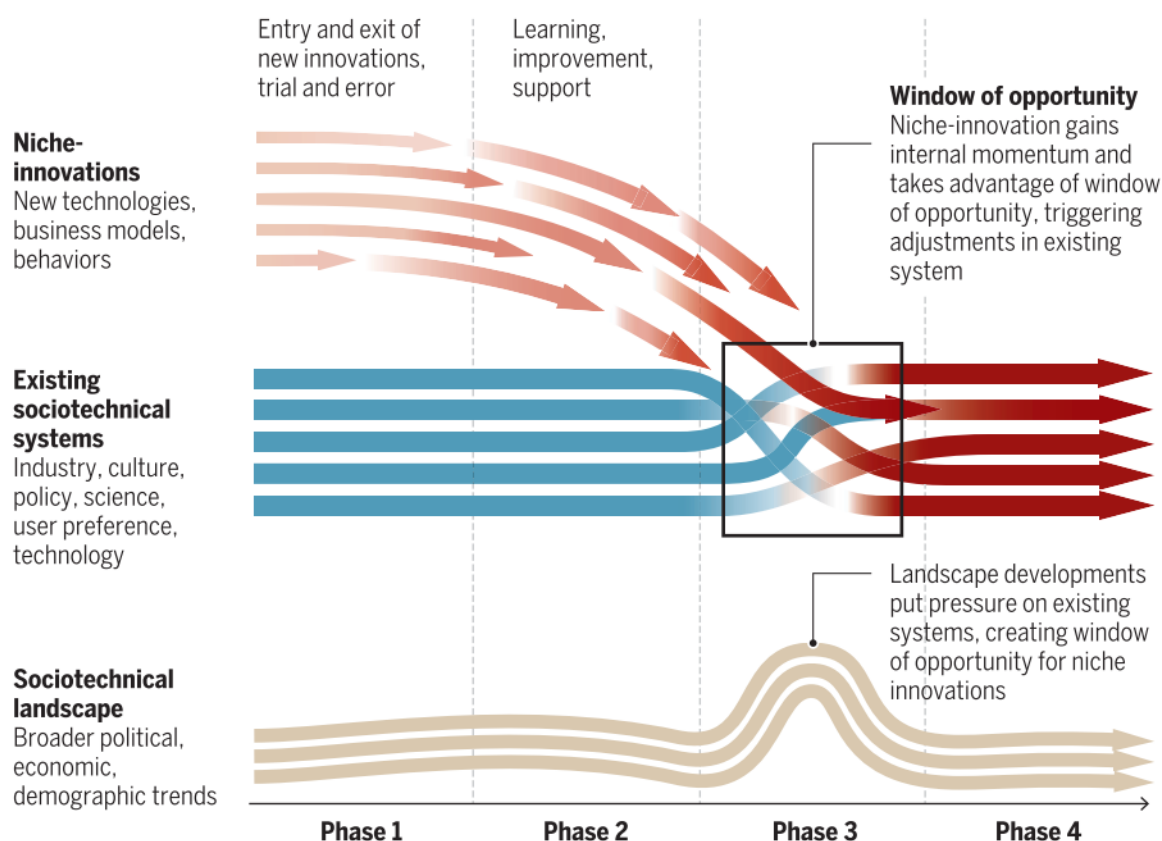


Figure 3 The Multi-Level Perspective¹⁴.

In niches, different innovations rise and fall. They may be propped up by research grants or a small protected market^{17,18}. There are many innovators, directions are disparate, and failure is the norm.

That is until landscape shocks disrupt existing regimes. They show, or create, a situation where existing systems and practices fail to meet a critical need. A gap in regimes is a window of opportunity for niche innovations to disrupt¹⁹.

With a new window of opportunity, niche innovations begin to align in direction. Innovators compete to fill the gap in existing regimes. Eventually, they reach a dominant design²⁰ – a singular and stabilised idea of what the innovation ‘is’.

The new innovation expands and scales, beginning to challenge the incumbent regime. The new innovation also begins to attract changes in other supporting systems and technologies. The incumbent regime, looking to preserve its power and dominance, either undermines the innovation or co-opts it into its own activities¹⁶. So begins a battle between incumbent and innovation.

If an innovation triumphs over an incumbent, surrounding systems (whether infrastructural, social, or political) fully shift and cater to the emerging innovation. Eventually, what was the innovation then becomes the dominant incumbent.

The current transition from fossil fuels to renewables represent many of the Multi-Level Perspective’s key dynamics.

1. The incumbent regime in this case is the current fossil fuel regime – a collection of not just energy infrastructure, but social ways of doing and political structures.
2. Climate change creates pressure on the fossil fuel regime. Science, society, and (slowly) politics are realising that human survival is at risk due to the greenhouse gas emissions of fossil energy.
3. This pressure opens a window of opportunity. Renewable energies, which do not emit greenhouse gases, are an alternative that can take the place of the fossil fuel

- industry. These include relatively mature technologies like wind and hydropower, but critically include rapidly emerging technologies like solar panels.
4. While there has been some experimentation over what solar panels ‘are’ (Solyndra for instance attempted to make solar panels using copper instead of silicon¹⁷), the dominant design of solar panels has stabilised.
 5. Renewable energies now find themselves in the early stages of competition with the fossil fuel regime. Special fossil fuel interests have notoriously undermined renewable energy transitions²¹, hoping to remain dominant. But emerging renewable industries are sparking transformations in their broader technological ecosystems. Chargers for electric vehicles for instance are seeing emerging implementation. Renewable energy systems are being increasingly adopted due to lower prices and more reliable energy supply.

It may seem like renewable transitions are a forgone conclusion – they are cheaper, more reliable, and more resilient. But this does not guarantee changes will take place sufficiently quickly. A full transition to renewables in 2080 still spells climate disaster.

What does the Multi-Level Perspective say about accelerating transitions? Acceleration is all about aligning niches, regimes, and landscapes. Broadly, this means supporting new innovations to experiment and grow, removing incumbent resistance by removing their influence or providing them benefits, and increasing landscape pressure.

In the context of renewable transitions, this could include increasing subsidies for renewable energies, employing counter-lobbying against fossil industry lobby groups, and increasing pressure from the climate movement to incentivise policymakers to legislate in favour of renewables (and against fossil fuels) more aggressively.

The science-policy system may have roles in garnering policy and commercial support for new renewable energy technologies like green hydrogen, engaging with fossil actors to determine novel business models that draw on negative emissions technologies, or leveraging the legitimacy of science to increase public and political support for the energy transitions. Science-policy actors also have a role to play in leading discussions on novel pillars of climate action, like greenhouse gas removal^{22,23} or solar geoengineering²⁴.

The main point here is that accelerating change, changing a Tortoise to a Hare, is not a matter of any single policy instrument or approach. A systemic and holistic policy response, comprising a policy *package*^{25,26}, is necessary to accelerate changes in large complex systems. The specific science-policy toolkit will change across problems and across contexts.

For instance, incentivising renewables in a market economy like Australia would call for more market-based mechanisms like a feed-in-tariff. Alternatively, incentivising renewables in a more centralised economies like China or India would call for more government procurement.

What the Multi-Level Perspective therefore ultimately provides is a way to identify appropriate policy packages more quickly by highlighting the critical issues to resolve:

1. What support do niche innovations need?
2. How can incumbent regimes be managed?
3. How can pressure from the broader contextual landscape be increased?

While useful, there are some caveats to the Multi-Level Perspective's narrative.

The stages of change and competition are not perfectly crisp. There are overlaps. Pinpointing specific stages of change is an exercise in futility, especially considering the non-linear nature of technological change. While the Multi-Level Perspective is effective at highlighting the *broad contours* of change, it (like any theory) should not be used as a prescriptive predictor of change.

The Multi-Level Perspective frames the motivator for innovation as only trying to disrupt an existing system. This is not always the case. There are a diverse set of drivers that can motivate innovation (see [Section 3.2](#)).

There are also deeper nuances to consider in who the innovator at play is, and what the reason for innovation is. Depending on who innovates and why, the nature of the

innovation at hand can shift drastically. Public and private sector innovations vary greatly in the nature of how ‘ground-breaking’ they tend to be, due to the political economy of producing public goods (see [Section 3.4](#)).

Most importantly, the Multi-Level Perspective frames the most important aspect to be the nature of the incumbent industry. This is broadly true - the decades of empirical evidence and application of the Multi-level Perspective^{16,27–30} give credence to the idea of a self-interested incumbent industries. But incumbent industries can still make major strategic blunders. They are not always the critical hurdle barrier the Multi-Level Perspective makes them out to be. Some areas, like greenhouse gas removal, may even lack a technological incumbent.

For Science-Policy: The Leverage Points of Acceleration

Science-policy has diverse roles to play across each stage of innovation. This could range from promoting and identifying commercialisation opportunities for niche innovations, identifying transition and integration opportunities with existing industries, and ensuring the voice of ‘science’ is a major part of a relevant discourse. Theories like the Multi-Level Perspective can point to key leverage points to focus on. This can be useful in efficiently allocating resources to different issue areas. Some issues require the creation of safety nets, others are about negotiating with existing industry, and others can be about political advocacy.

Box 1 The Leverage Points of Acceleration

3. Why, What, Who

3.1. The Innovator's Dilemma

There is a long (and statistically relevant³¹) history of firms that do not innovate enough or do not capitalise on their innovations.

Kodak is one such example. It was a leader in film photography, but internal teams were breaking new ground on digital photography³². Kodak invented the first digital camera in 1975 and the first mega-pixel digital camera in 1986. Internal analyses³³ concluded that digital cameras would disrupt Kodak's core film photography business. However, they would have roughly 10 years to prepare for such a transition. Kodak had every head start, every innovation, in the palm of their hands.

So Kodak tried to pivot to digital. They invested over \$2 billion in research & development for digital imaging, and installed 10,000 digital kiosks across their networks³⁴. (They also tried diversifying into pharmaceutical chemicals³⁵, buying Sterling Drug for US\$5.1 billion in 1988. Their expertise in film chemicals proved of little use, and Sterling was sold at a loss 6 years later³⁶)

Kodak's digital pivot had an Achilles heel - they did so to preserve their centralised store-based photo printing business. Photography and printing competitors like Hewlett Packard moved into home storage and printing^{34,37}. But the real mistake was assuming their market was still photo printing.

Rather, their market was photo *sharing*. By 2003, camera phones outsold digital cameras³⁸. The creation of Facebook soon after was the nail in the coffin. In this new world, store-based film printing was a thing of the past. Surprisingly, in 2001 Kodak had even bought Ofoto, a photo-sharing website. Tragically, they used Ofoto to drive more print sales³².

By the 2010s, Kodak's main business strategy was to sue other companies for patent infringement. Kodak filed for bankruptcy in 2012. As part of their bankruptcy plan, Kodak sold Ofoto for less than \$25 million. In the same month, Facebook bought Instagram for \$1 billion.

Kodak's infamous *Kodak Moment* wasn't borne from a lack of knowledge about what upcoming disruptive innovations would be, nor a lack of innovative capacity. Kodak had strong innovation and market forecast capabilities. Why sleepwalk into disaster?

They were overly focused on appeasing existing markets. Ensuring survival by relying on what already works. Conversely, innovations are by nature built for future markets. For future demands that may not currently exist. Fujifilm for instance transitioned their film capabilities into LCD screen production – an industry that did not at the time exist, but which ultimately proved to be their lifeboat as the film photography industry sank³⁹.

Innovating requires acceptance that most innovations will fail in the hope that one or two may prove successful later on. It's hard to prepare for the weather next week when you're in the middle of a hurricane. Herein lies the Innovator's Dilemma⁴⁰ – how do firms balance between short-term and long-term survival?

There is no clear answer, since there is no clear picture of what a 'correct' short-long term balance is. The balancing act depends on several interconnected issues:

- Who innovates? Private sector innovations tend to be less innovative, focusing more on optimisation rather than disruption. Basic foundational research is more often a product of public sector initiatives.
- In what sector? Pharmaceutical and automotive industries for instance require substantial innovation investment.
- The policy and business landscape. Is risk taking supported? Are there safety nets? Do these aggressively pick winners, do they focus on broader directions, or is the direction of innovation simply left to the free hand of the market? Is there sufficient support for scientific freedom and innovation?
- The specific and subjective risk aversion of any given firm or actor.

Regardless of the specific balance, short-termism takes precedence^{41,42}. This is seen beyond the business landscape. Countries face the same strategic difficulties. Australia for instance has strong comparative advantages in renewable energy, with an abundance of space and high solar and wind energy potential. Yet for decades Australian industrial

strategy has focused on mineral exports (alongside agriculture and real estate speculation)⁴³. Australia is ranked 91st in the Harvard Complexity Index for this very reason⁴⁴. Australia is a Lucky Country⁴⁵ for simply having world leading mineral export potential. Where competitors like the US, China, South Korea, and Japan are racing ahead with the industries of the future, Australia has been content to continue selling rocks.

In further answering why and how some countries, firms, and innovations race ahead, the next section explores the critical questions that determine one's balance of short-term and long-term ambitions.

3.2. Why Innovators Innovate: Need Seekers, Market Readers, Technology Drivers

Innovators are not equal. Innovators innovate for different reasons. There are three broad categorisations of innovator motivations⁴⁶.

Some innovators try to leverage innovation to create new markets. They identify future needs, and attempt to provide breakthrough innovations that punch open a new market, which they have first mover advantages in. Apple for instance identified a need for mobile internet devices that centralised communications in ways that existing mobile phones could not. Apple created the iPhone, breaking open the smartphone market, and placing itself as the frontrunner. These innovators are known as *Need Seekers*.

Other innovations capitalise on existing markets, expanding their reach to create sub-markets they can cater to. This can involve creating cheaper and more accessible innovations that can allow for broader adoption. They may not be first movers, but they are fast followers. The advent of cheaper Android-based smartphones helped expand the smartphone market. These innovators are *Market Readers*.

Lastly, some try to optimise existing markets with technological advancements. Where the innovation of the original Need Seeker was a breakthrough technology to create a market, it did not necessarily represent the pinnacle of technological capabilities.

Technology Drivers optimise existing products by innovating to maximise technological capabilities, for instance with the advancements of the Google Pixel or Samsung Galaxy (which introduced facial recognition security in 2011 with Android 4.0 Ice Cream Sandwich, whereas Apple only introduced Face ID in 2017).

Need Seeker innovators are conventionally framed as the most competitive. They produce the most radical and breakthrough innovations. The general consensus in innovation studies and strategy is that Need Seeker mindsets and innovators should be encouraged⁴⁶. This is broadly true, especially given the aforementioned difficulties in future-oriented thinking. However, an overemphasis on Need Seekers belies the diversity that is necessary in a robust innovation strategy.

There is no guarantee that one firm or country will produce the next breakthrough innovation. Banking on only creating the next big thing leaves one exposed to disruptive breakthroughs that come from other actors. Ensuring capabilities to be a fast follower or to extend the capabilities of existing innovations may not be ‘prime real estate’ of disruptive industries, but nonetheless is critical to ensuring competitiveness amidst highly dynamic and uncertain environments. China has been labelled as a copycat, with some downplaying China’s innovation potential⁴⁷. However, China has been able to leverage capital gains and innovation experience to position itself to lead and Need Seek in other innovations.

Furthermore, Market Reading and Technology Driving can still take advantages of one’s comparative advantages, even if the initial Need Seeking has been done by others.

For instance, the initial breakthrough of Direct Air Capture technologies was completed in the US and Europe. Direct Air Capture was used in the International Space Station before it was proposed as a solution for climate change⁴⁸, and those leading the charge to use Direct Air Capture as a climate solution have been largely American and European.

However, Australia, with its advantages in renewable energy potential and abundance of space, can still be a fast follower and technology optimiser by attracting overseas investment and international innovators to use Australia as a demonstration testbed. Australia could lead expansion of Direct Air Capture infrastructure and supply chains in the Asia Pacific (Market Reading) and could spark technological price-performance improvements (Technology Driver). In this case, Australia would not have been the original breakthrough innovator, but could still position itself as an innovation leader.

The motivators for innovation are varied, being a first mover is not the only ‘sell’ available when advocating for increasing innovation investment.

3.3. Hunting Pasteur’s Quadrant

An implicit assumption in the framework of Need Seekers, Market Readers, and Technology drivers is that Need Seekers work on innovations that are more basic, foundational, and blue sky. Market Readers and Technology Drivers conversely focus more on innovations that are more applied and incremental. Many who represent science advocate for the increased prioritisation of foundational basic research. But is the dichotomy of basic research & applied research valid?

The dichotomy of Basic & Applied Research has its origins in the 1945 report *Science, The Endless Frontier*⁴⁹. This was written by Vannevar Bush, the US Director of the Office of Scientific Research and Development. Bush’s report is heralded as the beginning of modern science-policy, and led to the creation of the National Science Foundation in 1950.

Bush framed basic research as research ‘without thought of practical ends’, focusing on understanding fundamental laws of nature. Applied research, on the other hand, focuses on utilising the bank of knowledge that basic research creates, aiming to provide solutions to practical problems. Bush argued that without substantial investments in basic research, industrial progress would stagnate⁴⁹. Materials science for modern mobile phones for instance could not exist without basic research in solid state physics⁵⁰.

Bush's narrative was successful in sparking interest (and more importantly, funding) in American science and research⁵¹. It served its political goal. Indeed, Bush's linear model is still influential in modern science-policy debates⁵². However, Bush's linear model of basic and applied research is a false dichotomy.

The lines between basic and applied research are blurred and dynamic. Applied research can in fact lead to advancements in basic research⁵³:

- The laws of thermodynamics were largely derived from industrial advancements in creating the steam engine⁵⁴.
- Fundamental radio astronomy was borne from trying to reduce radio antennae noise⁵⁵.
- Thomas Edison's work on developing practical power networks was the foundation of abstract network complexity studies in maths and computer science⁵⁰.
- Niels Bohr's foundational atomic research was key to developing the nuclear bomb in the Manhattan Project. Manhattan (an applied research project) then led to more avenues in basic research, like atomic physics, which later again led to practical developments in nuclear medicine⁵⁰. There was constant mixing between applied and basic research outcomes.

What we can clearly see is that Bush's linear focus belies the dynamic complexity of science, innovation, and the pursuit of new ventures.

Most importantly, Bush's framing has become outdated from a policy salience perspective. Where the framing of basic research being the fuel for industrial advancement was compelling in 1950, the same argument does not stick today. Basic research is now often seen as a tool for long term outputs rather than a core engine of progress⁵¹. Current policy discourse focuses heavily on the commercialisation potential associated with applied research, with longer term basic research seen as a secondary priority only worth investing in during times of stability (where there is budget and risk appetite to look at decadal timeframes).

This has resulted in a withdrawal of funding into what is classified as ‘basic’ research, with deferral to private sector actors to fund ‘applied’ research. But keeping in mind the blurred lines between basic and applied research, what has really happened has been a regression of innovative capacity. The proportion of disruptive science has steadily declined in recent years⁵⁶.

There may be avenues for further reframing, like reframing basic research as ‘long-term applied research’, but whether this would be sufficient to redistribute innovation funding is uncertain.

An alternative to the linear separation of basic and applied research is Pasteur’s Quadrant⁵⁷. Rather than framing fundamental understanding and use inspired research as separate categories, Stokes suggests thinking of these as spectra on different axes (Figure 4).

There is research that fits Bush’s idea of basic research, represented by the atomic and quantum physics research of Niels Bohr. Applied Research is represented by Thomas Edison’s inventions of the phonograph, motion picture camera, and light bulb. But there is also research that both advances fundamental knowledge and has clear practical applications, represented with Louis Pasteur’s research on pasteurisation. Pasteurisation both advanced fundamental knowledge of bacteriology and increased the shelf life of milk and wine. Here lies the vaunted Pasteur’s Quadrant.

Research is inspired by:

		Considerations of Use”?	
		No	Yes
Quest for fundamental understanding?”	Yes	Pure basic research (Bohr)	Use-inspired basic research (Pasteur)
	No		Pure applied research (Edison)

Figure 4 The model of Pasteur’s Quadrant⁵⁷.

Pasteur’s Quadrant does away with the assumption that basic research and applied research are separate. Creating large industrial and societal disruptions can happen concurrently with the advancement of fundamental knowledge. Where science advocacy often pushes the value of basic research based on the principles of curiosity and discovery, Pasteur’s Quadrant highlights a different policy narrative: Basic Research in and of itself can still be practical and disruptive.

Pasteur’s Quadrant is influential, but sometimes misinterpreted. Some have since argued that researchers should aim for Pasteur’s Quadrant research in order to best place science and innovation to serve society^{58,59}. That all scientists should aim to simultaneously juggle scientific fundamentals with practical outcomes in all the research that they do.

This is not necessarily the case and was not the intention of Pasteur’s Quadrant. Figure 5 is an alternative, but far less influential, imagining of different types of research. Most

importantly, Figure 5 highlights the dynamic *interactions* and outputs of different types of research. The point of Pasteur's Quadrant is not to solely prioritise use-inspired basic research (ie. Pasteur's Quadrant research).

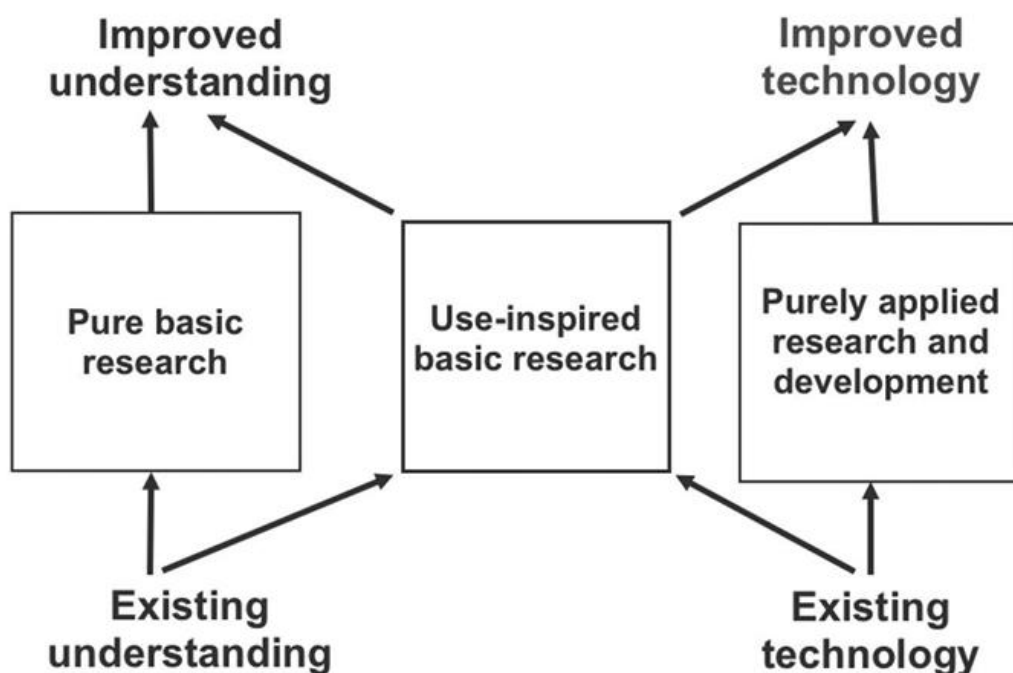


Figure 5 A dynamic model of basic, applied, and Pasteur's Quadrant research⁵⁷.

Rather, Stoke's work emphasised the importance of ongoing and wide-ranging research support. Rather than simply prioritising Pasteur's Quadrant research, what is necessary is an integrative innovation system that recognises the diverse values and interactions between different types of research. No one can know if the next big disruptor will be a product of basic or applied research.

Wherever its source, allowing transitions, cross pollination, and spillovers⁶⁰ across basic and applied research areas will best allow for impactful innovation (as in the case with image recognition innovations for different donuts being applied for recognition of different cancers⁶¹). Maximising applied research outputs also requires maximising basic research, and vice versa⁵³. Basic research is not an isolated luxury.

A robust *mix* of different research types is necessary, requiring strong innovation networks and diverse innovation capabilities. Such an approach should be further informed by context and actor specific comparative advantages and ambitions, but a necessary precursor step is to move away from the trade-off dichotomy of basic and applied research.

3.4. The Entrepreneurial State and Public Sector Visions/Missions

Having discussed the *whys* and *whats* of innovation, we turn to the important consideration of *who* innovates.

There is an influential narrative of innovation being driven by private sector actors. Whether it be Tesla or Apple, IBM or Intel, the private sector is what fuels the engine of progress. Government should let innovators innovate, get out of the way, and only step in as an investor of last resort to fix market failures that may arise. In his Person of the Year interview with Time Magazine, Elon Musk asserted that governments are poor “*stewards of capital*”⁶². Allowing government bureaucrats to ‘pick winners’ is dangerous. After all, one of the Obama Administration’s earliest investments was Solyndra – a company looking to create solar cells out of copper. Solyndra soon failed, costing the US government over \$500 million¹⁷. Letting market forces (i.e., the real expertise) decide the directions of innovation is how to drive innovative forces most effectively.

The private sector leadership narrative has been compelling. It pervades not just innovation, but many aspects of modern policymaking. For decades, the core philosophy driving many governments has been to privatise and recede⁶³. Government has increasingly deferred to consultancies⁶⁴. The role of government in guiding and sparking innovation has shrunk, especially in Australia⁶⁵.

But the private sector leadership narrative lacks empirical foundations. Private sector actors generally have low risk appetites. Where investments are risky and demand high capital (as is the case for many innovations, especially if they have long lead times for

outputs), private investment shrinks⁶⁶. Indeed, where private sectors have apparently innovated, they tend to come off of the backs of substantial government support and innovations already produced by government programs⁶⁷.

For instance, Tesla and SpaceX have both received roughly \$5 billion in subsidies⁶⁸, a far cry from Elon Musk's lone innovator narrative. Silicon Valley as a whole was propped up with early government spending. US Department of Defence and Apollo-era public investments provided early financial stability and a risk friendly environment^{69–72}, allowing for a hotbed of entrepreneurship. Many of the innovations underpinning the iPhone came from government: lithium-ion batteries were invented by the Department of Energy and microprocessors were invented by DARPA (see Figure 6).

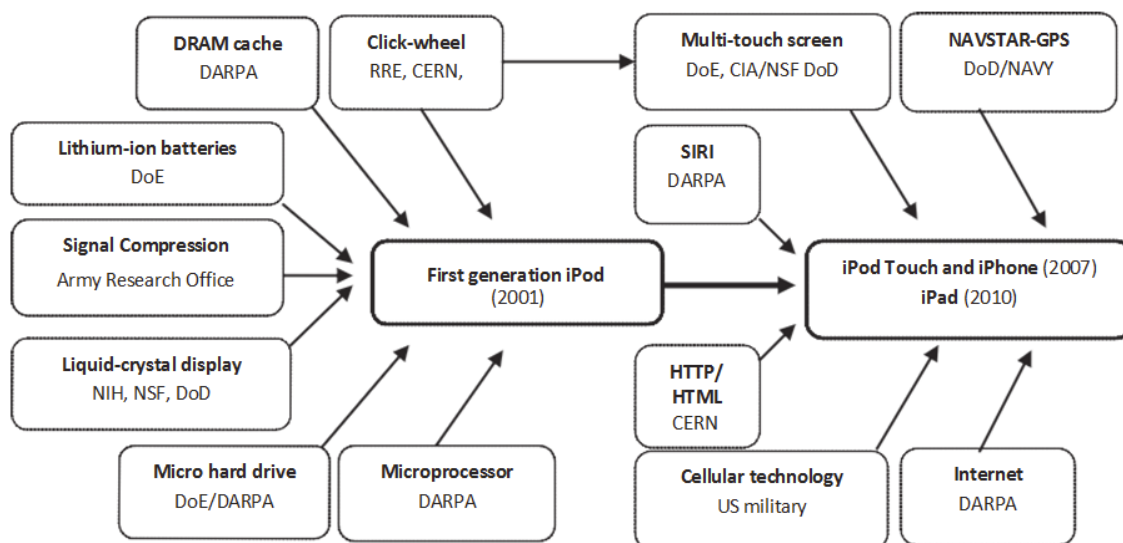


Figure 6 The iPhone and its government research roots⁷³.

Despite the public sector socialising the risks of initial investment, benefits overwhelmingly accrue to the private sector. There is no strong mechanism to ensure large private sector innovators act in the public interest. Where climate policy has already struggled due to the fossil lobby^{21,74,75}, the likes of Alphabet or Amazon dwarf the likes of Shell or Exxon Mobil. Facebook has essentially run a global experiment on society

and human interaction with no oversight^{76,77}, resulting in tangible effects on elections and political systems⁷⁸ (enabled with an intentional lack of internal governance and oversight⁷⁹⁻⁸¹). Society is destabilised while Mark Zuckerberg becomes a billionaire.

If innovation is to be leveraged to address the most pressing issues facing society, relying on the free hand of the market would be imprudent. Government involvement is necessary to provide a central coherent innovation vision and direction that is aligned with long-term public interests, rather than for short-term profit seeking⁸². Indeed, countries with more parliamentary forms of government and stronger civil society representation are correlated with higher proportions of public R&D funding⁸³. Critically, government involvement is necessary to attract investments in more risky and radically innovative research areas^{84,85}.

Rather than worrying about the risks of picking winners, which every government does explicitly or implicitly regardless of policy approach⁸⁶, governments should pick *directions* and support the willing⁶⁷. Such directions are now known as Innovation Missions⁸⁷. Similarly to how the US government stimulated private sector competition for the Apollo Project's priority research areas, which they deliberately picked, current governments should explicitly intervene and create risk taking environments⁸⁸ for contemporary priority areas, whether it be renewable energy, negative emissions technologies, or artificial intelligence. For example, Challenge Prizes (where a pot of money is available for innovations that look to meet certain societal challenges) can allow for picking a clear direction while also harnessing market forces of competition⁸⁹. Rather than being an end of pipe fixer of market failures, governments need to be investors of first resort.

However, it would be a mistake to say that the private sector is universally non-constructive. Private sector actors have a critical role to play in any robust innovation system. While the US government was instrumental to socialising risk in the early days of Silicon Valley, the 1980 Bayh-Dole Act allowed private use of research outputs from government funded research projects⁹⁰. Only then did Silicon Valley see its vaunted

growth. Early public investment is necessary to spur the initial growth of small markets, ‘crowding-in’ private sector investments^{85,91,92}. But longitudinal statistical analyses show that it is only private sector investments that can act as the engine of rapid non-linear market growth⁹³. There is not necessarily a trade-off dichotomy between government intervention and market competition – both are key ingredients to disruptive innovation⁹⁴.

For Science-Policy: Why, What, Who

The motivators for innovation are diverse. Some innovate to create new markets, others try to be fast followers, and others try to optimise existing markets with advanced technology. This means that the language of innovation advocacy in science policy can be similarly diverse. Being a first mover is not the only ‘sell’ for science and innovation. The prospects for ambition are wide.

Science-policy often uses the framing of basic and applied research. But this is empirically incorrect. Spillovers mean that both can feed into one another. The lines between basic and applied research are blurred. This also opens new opportunities for science-policy advocacy and engagement. Basic research need not be framed as solely a future investment – it can be useful now, if broad innovation policy frameworks allow for effective spillover.

Governments need to be first movers to create environments friendly for risk-taking and innovation. A diverse suite of private sector actors is necessary. Both are important targets for science policy.

Box 2 Why, What, Who

4. What is Innovation Policy?

The common thread across the previous sections on Why, What, and Who is that there is no single silver bullet for innovation. A robust innovation system will include a balance between:

1. Innovators who are aiming to optimise existing markets and innovators aiming to create breakthroughs that lead to new markets.
2. Innovations which are a product of basic research and applied research (and interplay between the two).
3. Public sector actors to set directions and private sector actors to execute visions and spark non-linear feedback loops.

Ensuring the necessary diversity requires broadening the scope of what we see as innovation policy. This section outlines the broad policy approaches for a diverse innovation system.

4.1. The Conventional Innovation System: Research and Education

Innovation policy is (justifiably) focused on research and education policy. A strong science and research system is necessary to ensure investment into high risk and highly disruptive projects. The bedrock of strong policy here is sufficient funding for universities and other research institutions. These can be protected spaces where highly innovative ideas can experiment and grow without exposure to the harsh reality of market forces. These spaces provide an environment for non-market focused niche innovations to grow.

Beyond funding size, there is a need for efficient organisational structures and limited bureaucracy. A 2019 survey found that 44.3% of time in US federally funded research projects is spent on administration and grant proposals, an increase from previous surveys⁹⁵. The Australian Research Council is similarly notorious for bloated administrative burdens⁹⁶. With these logistical issues, even just resorting to random allocation can be more effective^{97,98} (especially in new and emerging research fields with high uncertainty⁹⁹). Innovations already face a difficult path to survival. Death of an

innovation should be driven by its merits and flaws, not the bureaucratic costs of grant applications.

A strong tertiary education system is also critical for the skill-building necessary for new industries. The renewable transition in Australia is heavily bottlenecked by a shortage of a construction and tradesperson workforce with sufficient renewable expertise¹⁰⁰. This requires a major increase in investment and expenditure across the entire tertiary education landscape, especially vocational education^{101,102}.

Deliberate government funding is necessary, especially early in a transition, to ensure adequate human capital. Educators by themselves will not likely change curricula and offer new programs *before* an industry is created (why produce a set of graduates for an industry that doesn't exist?). However, a new industry requires highly responsive curricula and innovative education practices^{103–105}. Public sector direction is necessary to pre-empt a market failure of skill undersupply.

While important, sectors beyond research and education are crucial to effective innovation policy.

4.2. A Strong Business Environment

Entrepreneurial spirit is critical to effective innovation. However, private firms tend to be risk averse and focused on short term survival (see [Section 3.1](#)).

A popular policy approach to incentivising entrepreneurial engagement in innovation is to recreate Silicon Valley – to try create new innovation and regional technology hubs^{106,107}. Tech Central in Sydney for instance is being touted as Australia's Silicon Valley¹⁰⁸.

The rationale is that these regional hubs allow for specialisation, economies of scale, and innovation networks that allow for knowledge sharing. Successful innovation hubs like Silicon Valley and Route 128 were a product of substantial government intervention.

These are highly visible and localised investments that do not require changes in overarching business or industrial policy.

However, multiple analyses of regional innovation hubs show a mixed track record^{109–113} (too mixed even for the expected failures in innovation). The successes may be big, but these have come with a flurry of forgettable failures. Public sector engagement is important, but this does not change the fact that picking winners is difficult.

Regional hubs have a double layer of picking winners: the industry of intended specialisation *and* the geographic area where success will happen. The fast pace of contemporary economic change creates yet another layer of difficulty: slow and long-term infrastructural lock-in¹¹⁴ created with regional investment is at odds with the fast-paced dynamism of contemporary markets.

Lastly, many of the funds earmarked for regional investment often are subject to special interest lobbying. Rather than create genuinely risk tolerant and innovative environments, innovation hubs have historically focused on appeasing the likes of Foxconn and Amazon¹¹². With these compounding difficulties, the mixed track record of Silicon Clones is not surprising.

Instead, what is necessary are the ‘boring’ changes to create good business environments. Innovation relies on people willing to take risks and push preconceived knowledges and ways of doing. Innovation does not generally happen along predetermined industrial categorisations or artificial borders. An innovation cluster is a drop in the ocean if the overarching social, economic, and political environment overly skews towards safety and risk aversion.

For example, investment in Sydney’s Tech Central does not change the overarching fact that Australia is an entrepreneurial laggard. Australian venture capital cannot compete with larger funding pools overseas¹¹⁵. In the 2022 IMD World Competitiveness Yearbook, Australia ranked 61 out of 63 countries for entrepreneurship¹¹⁶.

A broad and systemic perspective is key to understanding the barriers and drivers of a strong business environment – sometimes the biggest drivers of a strong business environment lie beyond the ‘business’ sector. For instance, an unhealthily large real estate industry stifles risk taking and strong business environment. High housing prices weaken bottom-up labour markets that are critical to innovation, a pattern seen in Australia^{117,118}, the US¹¹⁹, and China^{120,121}. If one has money saved up, a house deposit can be more prudent than investing in a start-up or innovation. This may maximise individual gains, but compromises aggregate system-level competitiveness.

Providing a blueprint for a strong business environment is beyond the scope of this review. The policy approaches and instruments for business stimulation will be different across contexts. Business stimulation will differ across more centralised economies, like China or India, and more market-based economies like Australia or the US. Instruments can include tax incentives or labour laws, but also include leveraging social levers like open and skilled immigration^{122–124} or a fostering a culture of risk taking^{125,126}.

4.3. Regulation

The ‘glue’ for innovation and business is an effective regulatory environment¹²⁷. Knowledge spillovers across different approaches and areas of innovation are critical. Both research and business domains require removal of unnecessary red tape to function most effectively. But a ‘wild west’ of innovation would simply be an environment of disorganisation, rather than a coordinated systemic environment of risk taking. An environment of regulation that provides sufficient structure without overreaching and overcontrolling is necessary.

This section will briefly summarise a critical component of regulation - Intellectual Property (IP).

Innovators work best when they collaborate. But they need to feel safe in doing so. If innovators feel collaboration is simply a recipe for stolen ideas, they will isolate. The all-

important crossovers and spillovers will become limited.

In addition to protecting ideas, IP can serve as a vital revenue source. Initial capital can be critical in initially creating companies or stepping into manufacturing¹²⁸. IP also enables the use of licensing arrangements that help track supply chains and ensure quality control, combating harmful counterfeits like fake COVID vaccines^{129,130}.

However, striking the right balance between access and exclusive rights is essential. While IP can be beneficial for innovation, it can also be exploited by cartels to boost prices, exclude new innovators, and concentrate profits to corporate actors^{131,132}. Lowering IP barriers may increase short-term innovation access, but could deter long-term innovation¹²⁸. The role of IP in incentivising or limiting open innovation endeavours (distributed innovation processes with intentional knowledge flows across organisations¹³³) is greatly under-researched¹³⁴.

Additionally, IP and innovation are deeply disconnected subject areas – a problem of parallel universes¹³⁵. IP is critical to effective innovation policy, but there is a dearth of two-way engagement between the two to make effective systemic policy. For instance, the Czech Republic notoriously based research funding on the number of patent applications, leading to an increased filing of useless patents¹³⁶. Australia had an innovation patent scheme that tried to provide ‘second tier’ patents (patents with a lower barrier to entry) to support small and medium-sized enterprises. However, this was discontinued because larger companies flooded innovation patent applications, crowding out smaller innovators¹³⁷.

Not only must we get IP right in peacetime, we must ensure IP functions effectively during crisis (where innovation is critical). For example, during the COVID-19 pandemic, vaccine patent waivers were necessary to broaden vaccine manufacturing beyond high income countries^{138–141}. With technologies like solar geoengineering, banning patents (or at least patent royalties) is likely necessary to prevent profit driven and erroneous deployments^{142–144}.

IP is but one aspect of the broader regulatory system to consider. Effective innovation policy calls for robust consideration of the entire panorama of regulation. Indeed, this reinforces the core message of this section: innovation policy is broader than science and research. The key levers to target are wide and varied.

For Science-Policy: When Innovation Policy is Much More

Science policy is broader than just research or innovation policy. Increased research funding in a priority area cannot overcome an overarching environment that is unfriendly to risk-taking and new enterprise.

Areas which may seem beyond the strict remit of science-policy may be central to effective innovation strategy and advocacy. A strong innovative and risk-taking environment is difficult during a housing crisis, for instance. It is all about identifying key leverage points and interconnections between science, innovation, and broader society. Science-policy actors should be active in and target such leverage points.

Box 3 When Innovation Policy is Much More

5. Responsible Innovation

A critical flaw of the review thus far, a reflection of extant innovation research, is an assumption that innovation is always good. This is not the case.

Ensuring innovation speed and disruption is one limited aspect of effective innovation strategy. Questions of *what* is being disrupted, for *who*, and at *whose cost* are just as, if not more, important^{145,146}. However, these questions are often left behind.

Innovations, especially novel technologies, have the capacity to be misused and cause harm, whether purposefully or not. AI that can find less-toxic alternatives to common pharmaceuticals can also identify *more* toxic compounds that can be used for chemical and biological weapons (a capability only ‘discovered’ after the original researchers were invited to a security conference)^{147,148}. Vannevar Bush espoused the benefits of innovation, but also was an early advocate for the Manhattan Project and creation of nuclear weapons¹⁴⁹. While pivotal to ending World War 2, science also laid the foundations for the globally catastrophic risks from contemporary nuclear warfare¹⁵⁰.

Innovation is not always aligned with broad societal goals, nor a purely altruistic endeavour that linearly advances humanity’s progress. Indeed, ‘desirable’ futures to innovate towards are highly political and subjective^{145,151}. Politics, strategic interests, and conflict are major determinants of innovation directions^{152–154}. This is an inescapable realpolitik. Behind many pushes for innovation are major political imperatives:

- The innovation gold standard of DARPA came in the footsteps of the Sputnik launch by the Soviet Union, a space race, and Cold War with Russia^{155–157}. DARPA, an innovation leader in mass surveillance and lethal autonomous weapons (enabled by the post-9/11 Patriot Act¹⁵⁸), is now a major contemporary source of catastrophic risk¹⁵⁹.
- The US CHIPS and Science Act of 2022 increased funding to the National Science Foundation explicitly to counter Chinese innovation^{160,161}. In particular, it looks to increase US semiconductor manufacturing in case China blockades or restricts Taiwan’s semiconductor supply¹⁶².

- China's most recent five-year plan mentions innovation over 100 times, especially emphasising 'indigenous innovation'¹⁶³. This is driven by the COVID-19 pandemic, which exposed China's external reliance in critical industries like medicine¹⁶⁴.
- Taiwan's development of its vaunted semiconductor industry was driven by post-war partnerships with US enterprise, particularly RCA Corporation and Texas Instruments¹⁶⁵. Its semiconductor industry, heavily interconnected with US aligned or allied firms, in principle now provides a 'Silicon Shield' against a potential Chinese invasion¹⁶⁶.
- Similar patterns can be seen without great power politics. South Korea's 'Miracle on the Han River' was driven by post-war ambitions for rapid development. South Korea's strategy was to protect certain companies, like the grocery store Samsung, and give them an (unfair and politically driven) safety net for innovation¹⁶⁷⁻¹⁶⁹.

Innovation can be weaponised, even in areas beyond conventional kinetic or digital weaponry. In September 2022, the US prohibited Nvidia and AMD, two leading manufacturers of computer chips, from exporting high end computer chips to China¹⁷⁰. Why cut off a major trading partner, and further why would a capitalistic market driven country directly wade into the commercial actions of two specific private sector entities? The answer is simple: the US wants to limit China's development of AI capabilities. The US wants to win the AI arms race.

Even without intentional weaponization, innovations can reflect, reinforce, and deepen inequalities, rather than uniformly advancing progress. Innovations can even set us back decades, and put us on paths of destruction even when there are clear pathways for collective benefit. Existing social and political structures can skew the benefits and participation of innovation, even when they initially look to more broadly distribute benefits. Software programming for instance began as a woman dominated profession. In 1980, 70% of Silicon Valley programmers were women¹⁷¹. However, the norms of corporate America soon created an environment hostile to women, leading to contemporary gender imbalances in technology¹⁷¹ and innovation¹⁷².

Even innovations that can lead to broad societal benefits can be drowned out by other more dangerous innovations. For instance, the German railway system in 1903 created the world's fastest railcar, using an *electric* engine¹⁷³. The speed record set would last for over 50 years. This was a product of the 'Studiengesellschaft für elektrische Schnellbahnen' (Research Association for High-speed Electric Railways), who commissioned competing companies on the project (AEG and Siemens & Halske), leveraging innovation networks and competition. This could have been the start of an electric railway revolution, but was soon drowned out by the demands of war. Instead of railroads, German innovation created the Messerschmitt 262 - the world's first fighter jet.

All technologies have the capacity to be misused and cause harm, purposefully or not. This can be in its direct impacts, but also in the other innovation directions it drowns out.

This should be the default way of thinking about new technologies. Alarming, new technologies are becoming more powerful faster than governance systems can respond¹⁷⁴. Offence is becoming overwhelmingly stronger than defence.

Misuses of technology and innovation can be difficult to foresee and monitor. Technologies are simply tools. How they are used can change. AI image recognition for different donuts for instance has been used for image recognition of different cancers⁶¹. Use changes can happen slowly over time, without broader societal discourse or governance oversight – known as 'Function Creep'¹⁷⁵.

We must therefore go beyond closed system risk assessments of innovation and technology. Broader social and political systems are critical sources of risk, not just the technology or innovation itself. A nuclear arsenal is a catastrophic threat because of the high latent risk¹⁷⁶ it carries, but it takes political forces willing to deploy them to release their catastrophic potential.

In fact, given the political quagmires of worsening innovation risk, the best response can be to not innovate. This is an especially salient argument for the deepest technological

risks we face. Consider Artificial General Intelligence (AGI): an AI system that is comparable to a human. We can try to contend with issues like value alignment¹⁷⁷ ('how do we make sure a powerful autonomous system does what we want it to do?'), or we can try to not develop AGI. Some capabilities may not be worth developing.

Not innovating may seem like a futile attempt at going against the grain of progress. Technologies like AI have strong economic and political drivers that incentivise development, as well as core features (like decentralised ownership and short term profit opportunities) that can make restraint difficult¹⁷⁸.

However, restraint has a surprisingly strong empirical track record¹⁷⁸. Not only do innovations often fail, there is a historical track record of highly strategically valuable innovations that simply were not developed. These include a Soviet Internet in 1960¹⁷⁹ or electric cars in the late 1800s¹⁸⁰. Facing many extreme technological risks, there is a role of science-policy discussing what areas of science & innovation *are* and *are not* worth pursuing.

To guide our paths into the future, scholars have put forward principles core to responsible innovation^{181–185}: ensuring innovation aligns with broader societal interests, summarised below.

1. Anticipatory: Looking to the future not necessarily to make predictions (forecasting¹⁸⁶), but to ask 'what could happen' (foresight¹⁸⁷)? This is important to best identify risks before they happen, and to negotiate and discuss the desirability and undesirability of different futures.
2. Reflexive: Constantly being aware of current practices, commitments, assumptions, and limits of knowledge. This is important to make underlying ethical and moral quandaries explicit, which are often hidden under the guise of 'objective' science.
3. Inclusive: Ensuring inclusive, deliberative, and genuinely participatory dialogue, not just end of pipe stakeholder engagement. This is important to empower historically marginalised voices, as well as broaden views of desirable and undesirable directions.

4. Responsive: Responding to the issues raised previously with pace and substance, and ensuring (or building) the capacity to change or shape innovation directions.

These are not a responsible innovation panacea. How to best implement these principles will vary across issues and contexts, and is still an area of uncertainty (especially given ideas of justice and progress can be heavily contested)¹⁸². Regardless, these principles are a useful anchor moving forward.

For Science-Policy: Responsible Innovation

The Academy of Science has justifiably emphasised the importance of freedom and autonomy in research. This is especially important given the recent history of top-down political vetoes of Australian research funding, as well as the general politicisation of the Australian research landscape.

But alongside research freedom is the need for *responsible* freedom, to ensure autonomy and diverse innovation also aligns with broad societal benefits. The Academy, with its influential voice, has an especially important role moving forward to highlight areas and actions of responsibility and irresponsibility.

The recent ARC Review focused on administration and efficiency of ARC processes, not necessarily responsible research and innovation. A potential area of advocacy moving forward could be a responsible innovation statement as part of grant applications (similar to previous national interest statements).

Box 4 Responsible Innovation

6. Looking Forward

Effective innovation policy must be a wide-ranging, integrative, and democratic package of many instruments, with high capacity to adapt, geared specifically to each problem and each context. Crafting a central innovation playbook is out of scope for any review (especially for this review, which has emphasised that innovation policy is much broader than commonly framed).

Resources like UNESCO GO-SPIN¹⁸⁸ (an online database of innovation policies), the Global Innovation Policy Index¹⁸⁹, and other academic publications^{182,190} have already made initial attempts to summarise innovation policy instruments. Figure 7 is one such graphical example.

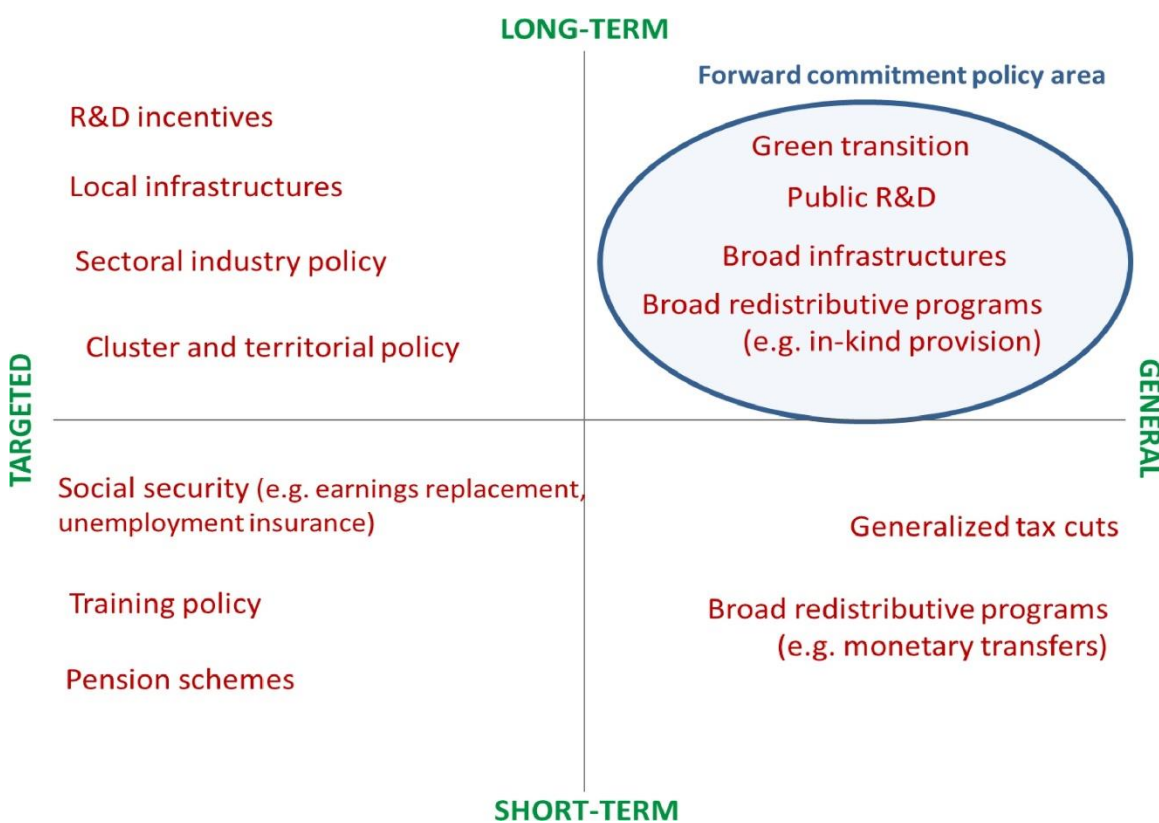


Figure 7 Collection of innovation policy approaches⁸³

Regardless, any summary of policy instruments will be woefully incomplete. What this review can more authoritatively provide are three key principles and approaches that can guide effective innovation policy, advocacy, and engagement moving forward.

First, governments are important first movers in innovation. They can provide big picture visions and create environments that encourage private sector risk taking. Early innovations need safety nets and protected spaces to develop. Countries which are highly innovative are innovative by design. Such directions are inevitably political – this represents both a curse and an opportunity.

A good early step is then to identify the unique cost barriers and performance goals of any specific innovation. Identifying these key leverage points is especially important since they may not be in the strict remit of science or innovation policy. Indeed, innovation debate should not focus on specific technologies. Rather, discussions should revolve around the leverage points of entire technological, social, economic, and political systems, even if they lie beyond the typical arena of science and research policy.

Lastly, as much as incentivising and accelerating innovation is important, innovation also carries many major risks. Humans are complex creatures living in a complex world – mistakes always happen. The difference now is that the frontiers of technology mean the magnitude of mistakes are far more severe. Innovation can be a tool of progress, but it can also be a tool of destruction. This razor's edge must always be central in any discussion.

We have a responsibility to make the world a safer, more prosperous, and fairer place. We are also facing threats that we do not have compelling responses for. For both of these, we need to innovate. The horizon of new ideas can unlock new levels of health and prosperity, but only if we make it so.

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Foreword to Chapter 3: Slippery Slopes

The previous chapter illustrated how complex technological changes, like GGR would be, historically err slow^{1,2}. Going beyond the broad historical comparison, Chapter 6 further investigates GGR specific characteristics that would make it err slow. These are complexity (demanding changes in broader infrastructural, social, and policy systems) and diversity (limiting the economies of scale advantages of a single technology). This provides a placeholder answer to Research Question 1a, to be further discussed later in Chapter 6.

Since GGR has strong reason to not meet the tight timeframes of limiting global warming to 1.5°C and 2°C, we may need to consider global sunlight reflection (also known as solar geoengineering). The most prominent method of this is stratospheric aerosol injection – deliberately injecting substances into the atmosphere to slightly whiten the sky and quickly cool the Earth, thus buying time for renewable transitions and GGR.

3.1. Place in the thesis

This Chapter is an assessment of the risks of research into solar geoengineering, with a focus on stratospheric aerosol injection throughout. The most common objection to researching solar geoengineering is the idea that research is the first step on a ‘slippery slope’ towards deployment³. However, the slippery slope has received very limited attention in the solar geoengineering literature. Assessing the slippery slope, and answering Research Question 2a., is a critical aspect of assessing the risks of research, and thus the desirability of stratospheric aerosol injection.

3.2. Key Conclusions

Despite being a frequently mentioned objection to research, the slippery slope is not well understood in extant literature. Different authors mean different things when they mention the slippery slope, and the term is used inconsistently.

This chapter puts forward a typology of different potential slippery slopes, identifying many plausible ways in which research could spark non-linear feedback loops that result in deployment.

Critically, this chapter argues that we should consider deliberate creation of lock-in, bucking the trend of previous literature that emphasises the dangers of lock-in⁴. Lock-in is not necessarily good or bad. It is a system state⁵. Lock-in of principles of democratic and inclusive decision-making would be a welcome form of lock-in because they would lock-out undesirable feedback loops from forming. Unchecked non-linear feedback loops, especially those related to highly impactful technologies like stratospheric aerosol injection, are simply too dangerous to be left unchecked.

3.3. Significance to the thesis

This chapter helps inform the desirability of stratospheric aerosol injection. Research does plausibly lead to deployment, but this should be treated probabilistically rather than deterministically.

The way in which research is done is vital to minimising slippery slope risks (or risk perceptions) and thus the desirability of stratospheric aerosol injection. For instance, research that is heavily reliant on best case modelling would increase the risks of premature normalisation of sunlight reflection as a legitimate policy response, and could leave us unprepared for the vexing realpolitik of policy. This would make stratospheric aerosol injection a less desirable response. Conversely, research that more accurately integrates political complexities into its analysis, is more democratic and deliberative, and centres principles of responsible innovation would make for better inputs into effective governance of a more desirable form of stratospheric aerosol injection.

This chapter has been published in *Global Environmental Change*. Please note that the version here varies slightly from the published version, which contains minor

typographical errors (what is Section 4.2.1 for instance has been published as Section 4.3). The journal has been contacted to amend these errors.

Appendix 1 is a companion article for public audiences, published in the *Bulletin of the Atomic Scientists*.

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Chapter 3: The Slippery Slopes of Climate Engineering

Research

Abstract

Climate engineering research attracts Slippery Slope concerns – the idea that initial research will inevitably lead to inappropriate deployment. Some have dismissed it as an unrealistic, unproductive critique. However, extant climate engineering discussions of the Slippery Slope discuss an unorganised set of different causal mechanisms with little detail. These range from technological cost reduction, to the creation of special interest lobby groups, to normalisation across society and policymakers. Dismissing the Slippery Slope may be premature if its causal nature is unclear, especially given the potentially high impacts and controversy of global climate engineering deployment. Disaggregating and clarifying the Slippery Slope can reduce unnecessary ambiguity, promote productive debate, and highlight risks that require further attention. Drawing on previous Slippery Slope literature and mechanisms of change from range of disciplines, this paper creates a typology of Slippery Slopes for application to stratospheric aerosol injection and other emerging technologies. Initial research can lead to deployment by 1) sparking price-performance improvements and sunk cost biases, 2) contributing to normalisation and legitimisation, 3) altering power structures, 4) sparking hype, and 5) incrementally progressing development. These feedback loops may currently seem unlikely, but unforeseen dynamics could still trigger rapid development and implementation of stratospheric aerosol injection. Conversely, there is no guarantee one of these Slippery Slopes will occur. The point is that they could – the future is too uncertain to fully dismiss non-linear change, particularly for high impact and accessible technologies like stratospheric aerosol injection. This can provide direction and clarity for effective technology governance and Slippery Slope discussion. Furthermore, this typology differentiates the Slippery Slope from lock-in and highlights their interaction points. Slippery Slope dynamics are processes that can (but are not guaranteed to) lead to different types of lock-in. Lock-in is when a technology is entrenched in existing

sociotechnical systems. Given the risks of unchecked undesired lock-in, lock-in is a state to be *encouraged* instead of avoided.

1. Introduction

Should we research climate engineering?

In the face of inadequate emissions reductions, climate engineering has emerged as a solution to climate change. This involves drawing down greenhouse gases from the atmosphere or reflecting sunlight to cool the Earth.

The most critiqued of sunlight reflecting methods is stratospheric aerosol injection (SAI): injecting particles into the atmosphere to slightly whiten the sky. SAI could address global warming risks if implemented carefully (Kravitz *et al.*, 2016; Irvine *et al.*, 2019). However, SAI deployment could also introduce a raft of new risks, potentially worse than the climate impacts SAI seeks to offset (Tang and Kemp, 2021).

SAI's risks drive both academic (Lin, 2020; Solar Geoengineering Non-Use Agreement, 2023) and civil society (Low, Baum and Sovacool, 2022) concerns that SAI research is the first precarious step along a 'Slippery Slope' to a dangerous SAI deployment. A Slippery Slope may be a concern for other climate engineering technologies, but a SAI Slippery Slope is especially important to consider because of SAI's relative accessibility – its cost would only be in the billions (Smith, 2020), and a minilateral deployment is feasible (Biermann and Möller, 2019). No other climate engineering technology has such a combination of magnitude and accessibility. This paper thus analyses the Slippery Slope argument against SAI.

The Slippery Slope argument suggests that an initial acceptable action (Action A) would snowball into undesirable actions or system states (Action B). Thus, Action A should be avoided. Applied to SAI, SAI research and development (Action A) may lead to widespread deployment (Action B) and associated risks. The Slippery Slope argument would therefore conclude that SAI research should be avoided.

However, most Slippery Slope debate focuses on the nature of Actions A and B, and less on the mechanisms in between. This is an issue shared with the broader philosophical

discussions of the Slippery Slope (van der Burg, 1991; Walton, 2015) as well as previous usage of the term. The Slippery Slope can be understood as Action A triggering a non-linear feedback loop that later results in Action B (Walton, 2015) (Figure 1).

Understanding potential feedback loops is critical to robust discussion and governance of initially innocuous but potentially high-risk actions.

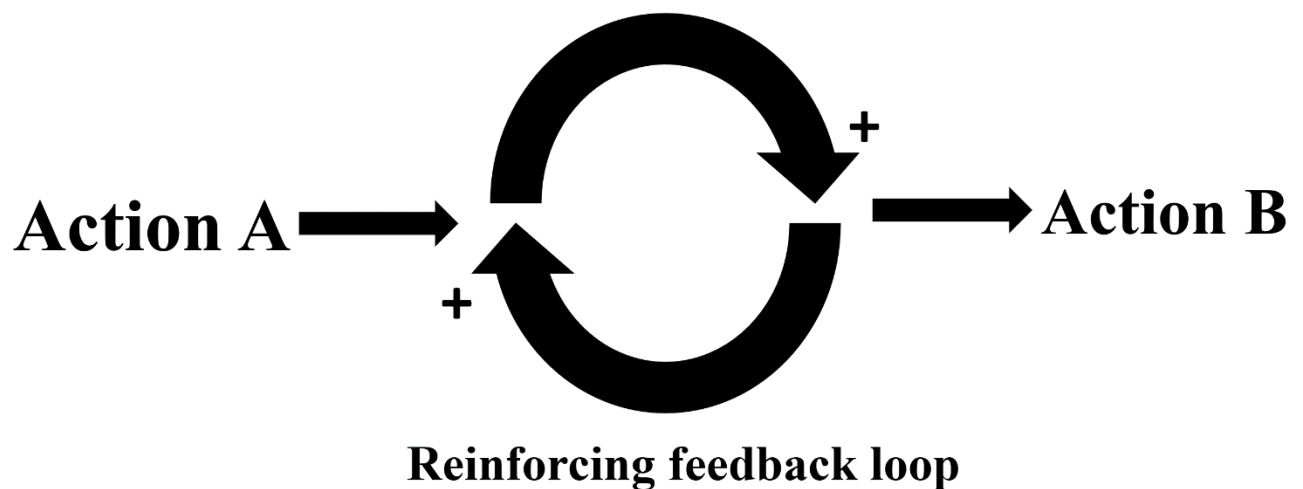


Figure 1 The Slippery Slope's Feedback Loop

The Slippery Slope is often cited in SAI discussions, and is one of the main objections raised by environmental NGOs (Low, Baum and Sovacool, 2022). However, it is inconsistently applied. Characterisations of the Slippery Slope range from the economics of technological lock-in, to the role of special interests, to SAI becoming more normalised over time (see Section 3). Despite the concept's prevalence, dedicated analyses of the Slippery Slope are lacking. The closest literature critiques the underlying philosophical premises of the Slippery Slope (Callies, 2018; Smith and Henly, 2021), but not necessarily the different ways in which a Slippery Slope could arise. The Slippery Slope is mostly tangentially mentioned, rather than being a subject of analytical discussion. Similar vagueness and inconsistency can be seen in other core SAI concepts, like moral hazard (Tsipiras and Grant, 2022).

Governing a Slippery Slope or responding to Slippery Slope concerns will be difficult if there is an imprecise understanding of its function. Productive debate and prevention of undesired outcomes can be most attainable if specific causal dynamics, rather than the broad Slippery Slope argument, are the explicit object of discussion. This will provide useful structure and clarity to such discussions.

Resistance to immediate/short-term actions may be based on the nature of the action itself, or on Slippery Slope concerns. Being able to disentangle this can help researchers and decision-makers understand, engage with, and respond to stakeholder preferences and values. This would better allow engagement and discussion based on key leverage points from empirical analysis, rather than broad and vague emotive arguments.

There is also further use in clarifying what a Slippery Slope would lead to. Lock-in, where a technology is entrenched in sociotechnical systems, can be a useful concept in filling this gap. Slippery Slopes and lock-in are similar, but there has yet to be an extended discussion of the relationship between a Slippery Slope and lock-in within the SAI literature. This is important because the broader lock-in literature similarly highlights the diversity in the lock-in concept, as well as the idea that lock-in isn't always a state to be avoided (Seto *et al.*, 2016). Integrating this into the SAI literature could improve rigor of governance discussion moving forward.

Aiming to address these gaps in the SAI and Slippery Slope literature, this paper introduces a typology of Slippery Slopes (summarised in Figure 2), as well as theoretical and empirical evidence that support and refute the different Slippery Slopes (see Section 4). These Slippery Slopes are differentiated from lock-in (see Section 5), where a Slippery Slope process is a feedback loop that can later lead to a type of lock-in. This framework provides structure for SAI Slippery Slope analysis, and can aid understanding of the cross-sectoral mechanisms of technological change in areas beyond climate engineering (like synthetic biology or artificial intelligence).

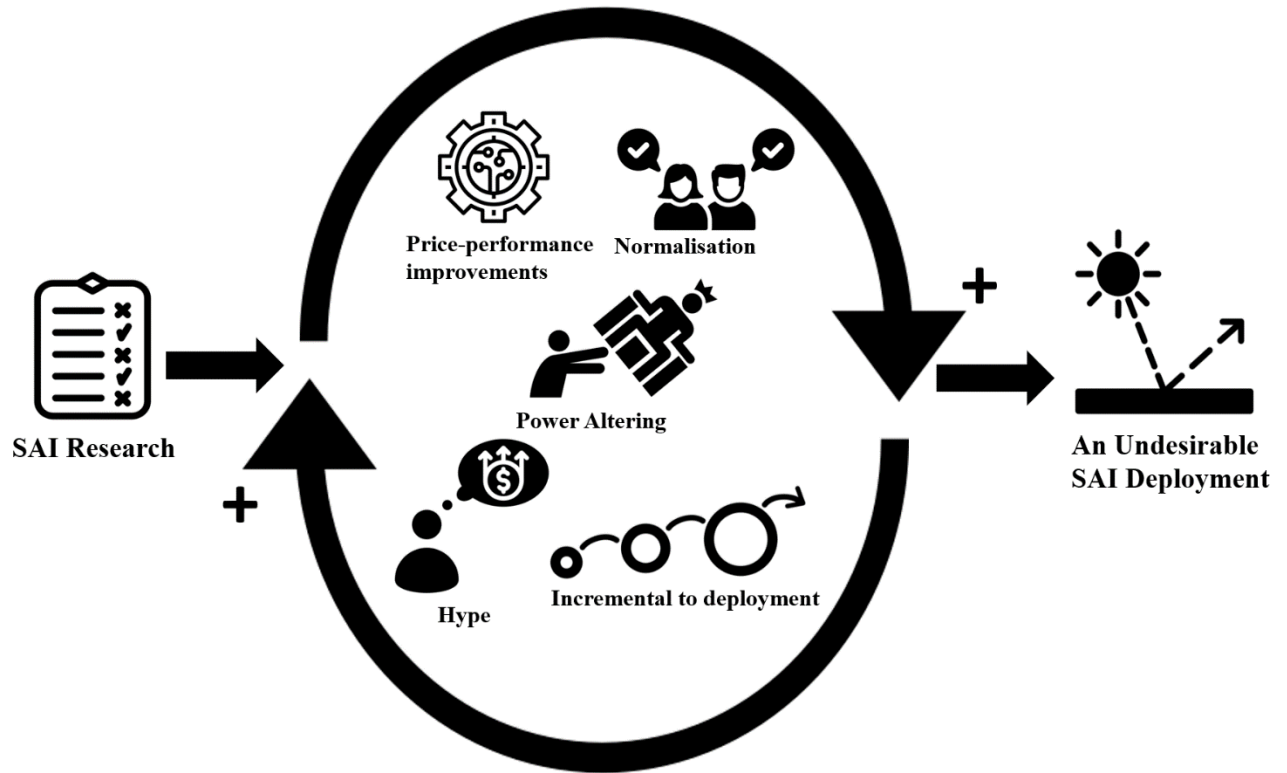


Figure 2 The Slippery Slopes of SAI

A clear typology of technological change (see Table 2) can greatly enrich analysis, even if the Slippery Slope is not the core object of discussion.

Short-term governance of SAI should aim for lock-ins that lock-out possibilities of unfair distribution of costs and benefits, institutions that entrench unfair power structures, and norms that facilitate uninformed debate.

2. The broader Slippery Slope literature

There is a well-developed literature focusing on the philosophy and principles of generic Slippery Slope arguments (Walton, 2015). However, these provide little guidance for understanding the applied causal nature of the Slippery Slope (Whitman, 1994). Some have raised the importance of understanding the causal mechanisms between a potential Action A and Action B (the 'Empirical Slippery Slope' (van der Burg, 1991)), but there

has been little since then to clarify potential mechanisms at play. Without precision of the Slippery Slope's possible function, discussion can overly focus on fear and emotion rather than evidence and reason (Saliger, 2007).

The Slippery Slope has also seen application in other areas like synthetic biology and law. Issues resolve around Slippery Slope concerns of a legal right to die (Lewis, 2007) or genetic engineering (Resnik, 2022; Walton, 2017). This area of literature has seen more of a focus on causal drivers. However, these are generally-one-off hypothetical analyses of a single causal chain, rather than an identification of critical leverage points and potential non-linear feedback loops that could be applied across contexts.

The closest systemic mapping comes from Volokh (2003), who identifies several causal mechanisms that explain the Slippery Slope, geared for legislative decisions (Table 1).

Mechanism	Summary
Cost Reducing	An initial legislative decision that lowers the cost of a later legislative decision. This can occur by representing a partial investment in a later action, or an initial action that provides information/experience that helps the implementation of a later legislative decision.
Attitude Altering	An initial legislative decision that shifts opinion. Enacting law provides legitimacy to its underlying justifications. Alternatively, an initial legislative decision may make a later decision seem less extreme.
Political Power	An initial legislative decision that empowers/creates an interest group, or divides an established interest group.
Momentum	An initial legislative decision that makes a movement seem powerful and successful to time-poor or information-deficient actors. These actors therefore shift support to align with what they assume to be powerful and successful.
Small change tolerance	Small legal changes that go unnoticed and bypass societal scrutiny. Small legal changes are noticed, but make an issue seem inconsequential. Actors then develop rational apathy.

Table 1 Volokh's Slippery Slope Typology

Potentially due to its legal focus, Volokh's typology has lacked application to SAI (the only exception being a 2013 workshop presentation (Schäfer, 2013)). Volokh's typology can be amended to be more applicable to non-legal fora, particularly SAI and other emerging technologies. Instead of focusing on the feedbacks sparked by legislative decisions, this analysis focuses on the feedbacks sparked by SAI research. Volokh's typology can also be complemented with a further analysis of possible counter mechanisms.

3. The Slippery Slope in climate engineering literature

There is a lack of dedicated analysis of the Slippery Slope and its function. In extant climate engineering literature, the Slippery Slope is mostly discussed only tangentially (Horton and Reynolds (2016), lacking analysis of its precise causal nature. The only dedicated analyses of the Slippery Slope in the climate engineering literature are Callies (2018) and Andow (2023).

However, Callies' (valid) focus is not the underlying causal nature of the Slippery Slope. Callies instead interrogates the philosophical premises of the slippery slope argument: that research will lead to undesirable deployment (uncertain), and that deployment is inevitable (empirically untrue). Andow's focus is on treating Slippery Slope arguments probabilistically, rather than deterministically (ie. it *could* happen, not *will* happen). Both articles make useful contributions, but still leave the core causal nature of the Slippery Slope in question.

Untangling politically charged terms like the Slippery Slope is important for productive debate and effective governance of potential risks of SAI research. Existing discussion can be organised along broad causal arguments¹ (see Supplementary Material for a brief qualitative coding of relevant literature).

¹ Horton and Reynolds (2016) organise the Slippery Slope along similar lines: the growth of vested interests and the progression of technological development.

The first emphasises the role of economies of scale, performance improvements, and sunk costs in fast tracking deployment from initial research (The Royal Society, 2009; Gardiner, 2010; Hamilton, 2013b; Preston, 2013; Stilgoe, Watson and Kuo, 2013; Horton and Reynolds, 2016; McKinnon, 2018; Reynolds, 2019). For example, Preston (2013) writes “*The more time and money that are invested in research, the harder it becomes to stop that technology from moving toward implementation*” (p28). McKinnon (2018) emphasises the role of economies of scale and learning opportunities in locking out alternative research pathways. This can be complemented with literature on the mechanisms of technological cost reduction and innovation success (see Section 4.1).

The second emphasises the normative influence of research, particularly where research and engagement (perhaps inadvertently) changes the status quo and normalises otherwise extreme or taboo technologies (Jamieson, 1996; Solar Radiation Management Governance Initiative, 2011; Banerjee, 2013; Hamilton, 2013; McKinnon, 2018; Ott, 2012; Pidgeon, 2013; Preston, 2013; Raynor, 2011; Smith and Wagner, 2018; Talberg et al., 2018; Cairns, 2014; Hamilton, 2013b; Reynolds, 2019; Biermann, 2022; Biermann, 2021; Climate Action Network, 2019). Research can also affect expectations. High hopes from initial research can also spark a self-reinforcing feedback loop of interest that encourages further research (Ott, 2012; Hamilton, 2013b) (see Section 4.4). Existing analysis on normative influence can be complemented with more specific analysis of how norms, and normalisation, can snowball change over time (see Section 4.2).

The third emphasises lobbying and vested interests, especially of researchers and innovators who have self-interests in deployment, motivated particularly by intellectual property/patent incentives (Jamieson, 1996; Gardiner, 2010; Solar Radiation Management Governance Initiative, 2011; Hamilton, 2011, 2013b; Ott, 2012; Preston, 2013; Stilgoe, Watson and Kuo, 2013; Cairns, 2014; Parker, 2014; Horton and Reynolds, 2016; Lin, 2016; McKinnon, 2018; Reynolds, 2019; Solar Geoengineering Non-Use Agreement, 2023). Non-government organisations like the Climate Action Network (2019) and ETC Group (2010) assert that researchers will advocate for the scaling up of

demonstration projects when smaller demonstrations or desktop studies fail to provide desired or sufficient results. Fossil fuel interests may also have an interest in accelerating SAI research to deployment (Smith and Henly, 2021) to delay renewable transitions. But Smith and Henly (2021) contend that SAI has limited commercial potential, especially in comparison to Direct Air Capture technologies that directly sequester carbon. This calls for further analysis on the mechanisms of power and influence (see Section 4.3), as well as analysis on the nature of research progression (see Section 4.5).

Despite this disorganisation, the Slippery Slope concern is beginning to be dismissed in academic SAI literature. Callies (2018) emphasises that research & development (Action A) does not inevitably lead to deployment (Action B), and that SAI's deployment (Action B's) undesirability is still uncertain. Smith and Henly (2021) assert that current socio-political hostility to SAI contradicts the Slippery Slope's core logic.

These are not convincing rebuttals. In these rebuttals, understanding *how* change could happen is critical. There are many different drivers and barriers of technological change, with different likelihoods, (un) desirabilities, and governance implications. Additionally, when dealing with non-linear feedback loops, the current material experience should not be treated as permanent. Changes in technology, policy, environment, norms, and culture can quickly and unexpectedly spark non-linear change. Alternative framings of SAI can affect desirability. SAI is widely framed under the context of global cooling. Alternate objectives, like using aerosol deployments to prevent cyclone formation² (Carrio and Cotton, 2011), may garner less resistance and an easier route to research and deployment. This may lead to using SAI for global cooling later in the future. The Slippery Slope is very much a live concern. Beginning to understand its precise nature will help guide governance discussions.

² For the purposes of staying within the scope of an initial organisation of the Slippery Slope, this paper frames SAI in the context of global cooling. Where other SAI objectives are relevant, they will be explicitly stated.

This paper does not attempt to clarify whether SAI deployment (Action B) would be desirable. However, unpacking the mechanisms that lead to SAI deployment are critical considerations to SAI's desirability. SAI driven by power dynamics and special interests for instance has different desirability to measured and considered SAI that embodies democratic knowledge and decision-making.

Another critique is that Slippery Slopes in any other technological research are just as likely (Smith and Henly, 2021). This is true, but neglects the unique magnitude of SAI consequences. SAI could directly affect the entire climate. Few technologies would be able to match the negative consequences of an insufficiently considered and harmful SAI deployment. SAI's high magnitude is compounded by its relative accessibility. Building the necessary aviation payload system is not predicted to be difficult (Smith and Wagner, 2018), and the projected yearly cost of roughly US\$18 billion per Celsius of cooling per year is well within reach of large states and/or minilateral alliances (Smith, 2020).

The Slippery Slope in relation to SAI is more important than with other technologies. The Slippery Slope has so far lacked dedicated analysis of its causal nature and organised consistent discussion, yet by some it has been dismissed. Dismissing the Slippery Slope may be premature if there is a lack of precision of how one would work. Connecting existing Slippery Slope structures to SAI, by amending Volokh's typology of Slippery Slopes, can clarify precise mechanisms of non-linear technological change, and help structure discussion. The next section details an amended Slippery Slope typology.

4. An amended Slippery Slope typology

This section details theoretical logics and empirical evidence for different Slippery Slopes, but also illustrates that the relevant causal mechanisms are not unstoppable and unidirectional (see Table 2). The objective of this section is not to develop a prescriptive theory of change or create exhaustive syntheses of different disciplines. Nor does the use of empirical examples always imply that such examples were themselves Slippery Slopes. Rather, this section aims to develop useful analytical concepts that can help clarify different mechanisms of non-linear technological change, as well as flag potential

empirical indicators of a slope where possible, thus providing clarity to the Slippery Slope debate.

To develop such concepts, a targeted narrative review (Green et al., 2006) of key areas of literature was conducted. Areas to review were derived by identifying a technological ‘mirror’ to Volokh’s original concept (for instance, the mirror to Volokh’s original concept of judicial cost reductions and precedence would be literature on technological price-performance improvements and sunk costs). In the different areas of literature, core orienting concepts (Layder, 2013) were identified, and were the basis of the causal mechanisms presented here. A narrative review approach is especially useful to synthesise and adapt key concepts to emerging technologies, rather than just summarising an area of literature (Greenhalgh et al., 2018).

Slippery Slope	Mechanism	Counter Mechanism
Material Cost Lowering: Price-performance improvements	Efficiency improvements and economies of scale established in early research lower the costs of large scale applied research and deployment.	Economies of Cost: Technological improvements can take too long or be insufficient.
Material Cost Lowering: Sunk Costs	Initial investments in research represent a sunk cost, one which researchers and investors are interested in seeing returns from.	Sinking Ship: The lack of technological improvements can overcome sunk costs.
Sociocultural Cost Reducing: Extremeness Aversion	Initial research (particularly public acts like demonstration) and engagement desensitise publics. This reduces aversion to ‘extreme’ solutions.	Extremeness Increase: Actions too ‘extreme’ can galvanise social resistance, precluding further development.

Sociocultural Cost Reducing: Legitimation	Research carried out by legitimate actors serves as a legitimising force for new technology.	Resistance: Initial actors or actions may not carry legitimacy, instead sparking resistance.
Power Altering: Entrenchment	Initial research that demonstrates benefit for a specific interest group, who then incentivise further research and lobby for additional economic and political support.	Power Grab: New coalitions can disrupt existing power structures.
Power Altering: Coalition formation	Initial research that attracts the support of otherwise unaligned actors, forming a coalition that supports research.	Power Resistance: Power balancing can offset the power gain of new coalitions.
Policy Momentum	Initial research that signals potential and garners hype, attracting support and investment in development.	Disappointment: Technological progress may not meet expectations, resulting in damaged trust that can preclude future support (even with later technological improvements).
Uncertainty- driven Increments	Smaller initial research that avoids socio-political backlash and engagement, but slowly adds up to technological lock-in. Initial research that highlights uncertainty. To resolve uncertainty, more research is undertaken. Research by nature always highlights more uncertainty and knowledge gaps. In response, more and more research is undertaken.	Big Bad Step: Not all research is incremental. Politicised events, like considering geoengineering at UN Environment Assemblies, attract substantial political attention.

		Awareness Advocacy: Researchers and organisations like the Carnegie Climate Governance Initiative advocate for more political and social SAI engagement not less.

Table 2 The amended Slippery Slope Typology

4.1. Material-Cost Lowing Slope

The Material-Cost Lowering Slippery Slope is when an initial action cheapens progressive next steps. Initial research can lead to widespread viability and implementation. Moore's Law, the observation that technological performance improvements are exponential, is central to this dynamic. Originally applied to the development of computer chips, Moore's Law also tracks the improvements of many other technologies (Nagy et al., 2013; Farmer and Lafond, 2016). If an early innovation gets off the ground, exponential improvements can sometimes be expected. Technology becomes cheaper and more accessible over time (Bostrom, 2019). Cost reductions and accessibility improvements are at the heart of whether a new technology achieves widespread adoption. Core to material cost reduction are two causal mechanisms: price-performance improvements and sunk costs.

Price-performance improvements sparked by initial research can lead to viability later. Research provides operational experience that can cheapen (or project to cheapen) deployment costs (Thomassen et al., 2020). This can incentivise further development. Continuous reduction of technology cost lowers the barriers to entry for investment and early adoption. This can gradually incentivise a critical mass (Rogers, 2003; Hall, 2004) that leads to mainstream support. Production expansion creates economies of scale (Nagy et al., 2013; Lafond et al., 2018) that further reduce costs as research and development

progresses. This self-reinforcing feedback of price-performance improvements and research investment can snowball the development of technology.

Research investments (time, money, talent, other resources) also represent sunk costs. Investments in research often cannot be recouped. Time spent is time spent. But full benefits for research investment may only arise over longer timeframes. There are long lead times in nurturing early innovations, building infrastructure, establishing supply chains, and developing appropriate policy support. This can bias decisions to continue research in the hope of finding returns on investment (Roth et al., 2015; Mañez and Love, 2020). If there has already been investment in limited deployment, upscaled deployment can seem more cost justified. Sunk costs help entrench research investment, keeping the door open for later exponential performance improvements.

But cost reductions can be insufficient. Some technologies do not sufficiently increase performance to justify continuous research investment. Early innovations are volatile and failure is standard (Geels et al., 2017). The success rate for pharmaceutical innovations is roughly 11 % (Kola and Landis, 2004; Callies, 2018). Intel's *New Business Initiatives* portfolio had an innovation survival rate of roughly 10 % (Raynor, 2011). These low success numbers come despite the substantial innovation funding in pharmaceutical and computing industries (Jaruzelski et al., 2014). Such figures also do not capture earlier and difficult to measure stages of innovation, like ideation. The actual success rate is therefore likely lower than measured figures. Even if there are sunk costs skewing decisions towards additional research, sometimes the best response to technological volatility and unacceptably long lead times is to cease investment.

Additionally, even if technological hardware costs go down, overall network costs can still be high. Technological development can be stymied due to poor fit the established technological ecosystem. Widespread implementation of the high-definition television was delayed by changes in improved video capture and transmission technology (Adner and Kapoor, 2016). The full cost of adopting networked personal computer systems in offices was roughly 10 times the individual hardware cost (Brynjolfsson and Hitt, 2000)

due to personnel training and infrastructural adjustments, like installing servers. A major difficulty with hydrogen energy is that it requires substantial developments across its multisector supply chain (Eljack and Kazi, 2021). The extent to which a new technology can be ‘plug and played’ within existing systems can override individual cost dynamics.

Price-performance arguments are less relevant to SAI. SAI should fit reasonably well within existing aviation infrastructure, and the technological modifications needed are projected to be relatively modest (Smith and Wagner, 2018). SAI’s costs, in the billions, are likely to be lower than the trillions required for widespread decarbonisation, regardless of potential technological innovations. Another possibility lies in decentralised SAI (Reynolds and Wagner, 2020). If do it yourself or small-scale kits of SAI (likely using balloons rather than airplanes) because highly cheap, a wave of individuals or start-ups (like the controversial *Make Sunsets* (Temple, 2022)) may be on the horizon.

A more relevant Slippery Slope relates to research related sunk costs. Initial SAI field tests may be more likely than envisioned. There has been clear resistance to SAI field tests, as seen with the experience of the Harvard SCoPEX project (Saami Council, 2021). But this operates under the broader umbrella of SAI for global cooling. Other objectives may experience an easier path to field testing. SAI technologies for instance can be used to prevent cyclone formation (Carrio and Cotton, 2011). Aerosol injection to directly reduce disaster risks from climate change may garner a less hostile social and political reaction. SAI infrastructure (ie. aircraft and atmospheric measurement) may be eventually used for global cooling, especially if they see limited use in non-cyclone seasons.

Overall, Slippery Slope concerns regarding reduced technology costs seem of less concern to SAI. Slippery Slope concerns in relation to research creating sunk costs seems of higher concern. Tracking developments in aerial substance deployment methods (eg. aerial tankers) may be a useful empirical indicator of a potential slope.

4.2. Sociocultural Cost Reducing Slope

New technologies can lack fit with existing social and cultural systems. The extent of misfit with existing sociocultural systems can be framed as incurring a ‘sociocultural cost’ (for example, unfamiliarity (Shrum et al., 2020), stigma (Vasi and King, 2019), or inequality (Schot and Kanger, 2018)). This can also be understood as the ‘socio’ aspect of sociotechnical systems in existing discussion (Bellamy, 2016; Sovacool and Hess, 2017). The sociocultural cost concept can provide the analytical flexibility to encompass the different social hurdles that SAI would face.

In the context of the Slippery Slope, a Sociocultural Cost Reducing Slope is when an initial action reduces sociocultural costs such that further action is encouraged or faces less resistance. In other words, an initial action that sparks changes in sociocultural systems to better fit a new technology. (Volkh originally frames this as an ‘Attitude Altering’ Slope. Emphasising the sociocultural instead of individual attitudes better considers collective dynamics, like culture.) This section covers two key aspects of sociocultural costs: reducing extremeness aversion and leveraging legitimacy.

4.2.1. Reducing extremeness aversion

Extremeness aversion is the tendency of actors to avoid options perceived as ‘extreme’ in favour of more ‘moderate’ options (Neumann et al., 2016). Reducing extremeness aversion entails making a certain option seem less extreme, and therefore more socially acceptable.

Actors can become desensitised to initially ‘extreme’ options via continued exposure. Continued exposure to community violence or violent media for instance can desensitise individuals to violence (Fanti et al., 2009; Kennedy and Ceballo, 2016). Initial research, particularly public activities like field testing and demonstration, can desensitise and normalise larger scale deployment of initially ‘extreme’ SAI, as has been previously argued (Jamieson, 1996; Preston, 2013; Cairns, 2014; McKinnon, 2018). Alternate uses

for SAI technology, like cyclone prevention, may be an initial use that precludes extremeness aversion of global cooling.

However, desensitisation is not guaranteed. Small field tests may still be perceived as too extreme to tolerate. The proposed 2009 Stratospheric Particle Injection for Climate Engineering (SPICE) garnered backlash from a group of international NGOs, and later was cancelled due to patent related conflict of interest concerns (Pidgeon et al., 2013). Where trust and social licence from local stakeholders is lacking, continuous community backlash can delay or cancel development (Moffat et al., 2016; Temper et al., 2020).

Whether research will desensitise SAI to publics and key decision-makers is uncertain. The SAI experience so far has been consistent and even escalating opposition (Biermann, 2022). The social and political issues of SAI cyclone prevention on the other hand are heavily under researched. Cyclone prevention aerosol injection may still be sufficiently ‘extreme’ to stymie development. Cyclone prevention field tests (albeit via cloud seeding rather than SAI) in the 1960 s-70 s attracted attention for potentially worsening Hurricane Betsy in 1965 (Whipple, 1982) and for allegations from Fidel Castro that the US was attempting to weaponize the climate (Wood, 2007). But sociocultural dynamics are hard to predict and foresee. SAI desensitisation may still be possible over the coming decades, especially if SAI remains an active object of discussion. Tracking (social) media discourses around SAI may be an empirical indicator of extremeness aversion dynamics.

4.2.2. Legitimising

The technical expertise and credibility of research communities can be leveraged, sometimes by non-scientific actors, to influence policy decisions (Finnemore and Barnett, 2004; Lacey et al., 2015; B’eland and Cox, 2016; Maas, 2019). For example, the United States sugar lobby incentivised research to emphasise the health risks of fat instead of sugar (Kearns et al., 2017). Dietary guidelines later downplayed sugar’s cardiovascular and cancer risks (Nestle, 2016). Similarly, the research community of nuclear weapons leveraged their expertise to argue for the security benefits of nuclear arms control treaties, a key driver in creating the 1972 anti-Ballistic Missile Treaty (Maas, 2019).

However, the legitimising process hinges on the legitimacy of the actor performing the initial action. With increased politicisation of science (Haerlin and Parr, 1999; Mahony and Endfield, 2018), scientific actors do not universally carry legitimacy, especially when researching controversial topics like climate change or SAI. Illegitimate actions taken by illegitimate actors can instead spark resistance. For instance, the rogue gene editing of two babies in 2018 sparked calls for a moratorium on the research of human genetic modification (Lander et al., 2019). Initial perceptions of SAI as illegitimate and unrepresentative (Biermann and Möller, 2019) may spark resistance that precludes further development.

Will research legitimise SAI? This too is unclear and contested, but plausible. Researchers framing SAI as a positive intervention (Harding et al., 2020), worth giving a chance (Nature Editorial, 2021), and in an optimised ‘designed’ manner (Low and Honegger, 2020) may contribute to legitimising SAI. Still, this is highly contested among researchers (Biermann, 2021). In January 2022, a group of roughly 50 researchers called for a moratorium on the use of solar geoengineering, explicitly flagging the dangers of legitimising solar geoengineering as a policy option (Biermann, 2022). Those presenting SAI as a legitimate option may currently be a minority, but this does not preclude a broader legitimisation of SAI over time. Again, aerosol injection for alternate uses like cyclone prevention may garner a different legitimacy response, but this requires further research (not least for how to determine or track different extents and perceptions of legitimacy).

4.3. Power Altering Slope

The Power Altering Slippery Slope occurs when an initial action affects the power balance between actors, stimulating further action.

A new technology can provide disproportionate benefits to an existing powerful actor. The actor in question may therefore look to accelerate the technology’s development

(Geels et al., 2016). These actions look to leverage innovation to entrench existing power structures.

The ascent and strengthening of one group can trigger retaliation by another. In reaction to a threatening and powerful actor or coalition, actors may respond by trying to create a more equal ‘balance of power’ (Walt, 1985; Person, 2017). This could involve building coalitions with other actors, investing in one’s own power, undermining the legitimacy of the rising foe, or reducing the alignment of the opposing actors to ‘wedge’ their coalition (Schweller, 2016).

Could SAI research trigger a power altering slope to deployment? There are several plausible mechanisms already identified in existing literature. For instance, SAI potentially aligns with fossil interests. Fossil based interests may encourage SAI development and deployment to delay renewable energy transitions, helping to preserve existing fossil- based power structures. SAI may also be subject to broader geopolitical power dynamics. SAI could represent a new frontier of transboundary climate control, potentially sparking an arms race for SAI capabilities among powerful nation states or coalitions. Whether these will occur is difficult to predict, but they are possible. Tracking SAI investments from fossil fuel actors and international developments of SAI discussions could be useful indicators of a power altering slope.

An often raised but sparsely critiqued power dynamic relates to researchers and Intellectual Property interests. The logic being that, motivated by Intellectual Property benefits, researchers will lobby for deployment. This is where a relatively clear intervention point lies. Banning patents for solar geoengineering technology, as has been advocated (Biermann, 2022), may shut down this causal line before it begins. This could be easily tracked with SAI patent applications or approvals. But researchers may still be motivated to lobby for deployment for other reasons, for instance a genuine belief in the usefulness of SAI. Again, this is certainly possible and uncertain. Especially where input from researchers is more informal, policy engagement from researchers may be difficult to systematically track.

Will researcher lobbying affect decisions? As discussed previously, the legitimacy of researchers can affect policy decisions. But whether they will, especially in the context of SAI, is unknown. An important consideration is the nature of the science-policy connection within any decision circle. An administration which marginalises the voice of researchers will also marginalise the lobbying capacity of such researchers. Alternatively, a more integrated science-policy connection provides more opportunity for science based (and potentially IP driven) lobbying. Another consideration is the other power drivers at play. If SAI is treated as an issue of broader geopolitics and security, the input of researchers may be secondary to broader political concerns. For instance, if the driver of development and implementation is to do so before another great power does, a researcher lobby group would only reinforce an already active and more overpowering driver for action.

4.4. Policy Momentum and Hype

The Policy Momentum Slippery Slope³ occurs when an initial action creates socioeconomic and political signals that affect perceptions of power and support (Cornwall et al., 2007; Baeriswyl and Cornand, 2010; Mielke, 2019) that lead to further action. Especially important to technological innovation is ‘hype’ - where technological promises and expectations greatly exceed technological fundamentals. Technological expectations and promises signalled by research (inadvertent or otherwise) can spark support that later leads to material technological development (Borup et al., 2006; Intemann, 2020). Healthcare innovations like stem cell and cancer treatments for instance have a rich history of attracting investment based on expectations rather than

³ Unlike sociocultural cost, which applies more to broad dynamics like culture, policy momentum is most relevant to key political elites and interest groups. Sociocultural dynamics may not always match the momentum of decisionmakers. Reform of Wall Street and financial institutions in the US had relatively broad sociocultural support, manifesting in the 2011 Occupy Wall Street movement (Kavada, 2015). However, this lacked political momentum from key policymakers and interest groups (Crutchfield, 2018), stymieing change.

technological fundamentals (Glassman and Sun, 2004). Like a self-fulfilling prophecy, perceived potential and momentum can lead to material action.

Signalling does not unidirectionally result in change. Signals require coherence (Mielke, 2019) and repetition (Downie, 2017) to maximise effectiveness, despite their inherent ambiguity and inconsistency. It is unclear what precisely constitutes a ‘policy signal’, where they may originate from, or what their precise magnitude of their impact is. Mielke’s (2019) analysis of signalling and investment decisions uses a typology of market, policy, and civil society signals to organise signal mechanisms. Despite this typology, example signals still range from ‘public debate’ to price fluctuations in the European Union’s Emissions Trading Scheme. It is uncertain which signals are (intentionally or implicitly) recognised as signals, and recognised by actors who have decision-making capacity. In addition, if material developments do not sufficiently match hype, a loss of trust can result in reduced support even if material progression later improves (Master and Resnik, 2013; Lacey et al., 2018). Signals are not consistently or universally positive nor salient.

Will SAI be subject to a boost in hype that later results in material support? We do not know. But other technologies with little novelty or substance have received millions in funding. Juicero, a company which sold \$400 juicers, attracted \$120 million in investment (Levin, 2017). Theranos touted a blood testing machine that only required a drop of blood. Without clear or peer reviewed evidence that the machine actually worked, Theranos attracted over millions in investment and had a \$9 billion valuation in 2013 (Waltz, 2017). SAI conversely does have underlying technological feasibility and has relatively clear deployment objectives, namely preventing global warming, that may attract hype and investment. Tracking SAI investment may be a useful indicator, but determining whether (and to what extent) investments are due to hype or underlying technological fundamentals may prove difficult.

Alternatively, the controversial nature of SAI may preclude a hype cycle from forming. The ‘signals’ that initial research may produce may themselves be too unclear to spark

consistent action. Continued SAI research may signal policy support and investment potential. But contemporary resistance to SAI research may signal an unpopular technology with limited practical potential. The future of an SAI hype train is therefore unclear, but ultimately plausible.

4.5. Uncertainty-Driven Increments

The Incremental Slippery Slope highlights natural biases to concentrate on large changes and marginalise small change. Incremental changes can collectively produce large change over time (Lindblom, 1959; Rothmayr Allison and Saint-Martin, 2011), especially if they pass thresholds that result in non-linear progression (Howlett and Migone, 2011). Smaller deviations to the status quo in theory (Pal, 2011) are more reversible, incur lower cost, and incur lower risk than larger sweeping changes, inviting less political attention (Rothmayr Allison and Saint-Martin, 2011). Smaller increments can also appear inconsequential. Actors may lose interest or defer the issue to others (Volokh, 2003).

In research, incremental progress is driven by a desire to reduce uncertainty. Technological development by nature carries uncertainty – particularly of risks, benefits, and potential policy and governance responses. Reducing these uncertainties can better inform decision-makers. However, more research instead often highlights more knowledge gaps (Mearns, 2010; Enserink, Kwakkel and Veenman, 2013), especially of complex socio-political dynamics (McKinnon, 2018; Haikola, Hansson and Fridahl, 2019). Driven by a constant desire to reduce uncertainty, research continues incrementally. This could be easily tracked with by monitoring outputs and plans of SAI field tests (if publicly available).

NGOs such as the Climate Action Network (2019) and ETC Group (2010) have expressed concern that this uncertainty-driven incremental logic will incentivise upscaling of SAI field tests and demonstration. Starting at desktop experiments, progressing to small scale field tests, then potentially resulting in large scale tests that are similar to full deployment (Hamilton, 2013; McKinnon, 2018). Smaller field tests may not appropriately represent potential issues of larger deployment, thus incentivising larger

scale field tests and consequently research by deployment. Despite being oriented around uncertainty reduction, uncertainty will always be present.

The first critique of this logic is that the incremental progression to full deployment is more difficult than originally framed. Field tests would not necessarily be similar in scale to a full deployment (Smith and Henly, 2021). Further progressions and scale ups beyond initial field tests would be necessary to be comparable to a large deployment.

Furthermore, SAI has not followed this attention avoidance logic. Incremental changes related to already intensely political issues do not necessarily avoid attention. Incremental research advances are also sometimes punctuated by material, social, or political breakthroughs (Kuhn, 1962; Kaiser, 2012). Major political events, namely discussions at the United Nations Environment Assembly or petitions for SAI non- use, are key political breakthroughs that attract attention. Influential scientific assessments, like a 2021 report advocating for ‘cautiously pursuing’ SRM research by the US National Academies of Sciences, Engineering, and Medicine (Lowry, 2021), also present influential breakthroughs that attract attention (Gupta and Möller, 2019). Indeed, the trajectory of SAI development and implementation may not be a linear incremental progression. A world experiencing more warming may use SAI as an emergency desperate last resort - a pathway of rapidly evoking emergency action rather than a slow incremental push.

Nor has the avoidance of social and political attention been the intent of SAI researchers. Organisations like the Carnegie Climate Governance Initiative have engaged governments on SAI and its associated governance challenges. There are many social and political issues of SAI development, like representative decision-making, that researchers assert require broad and deep social engagement (Winickoff et al., 2015; Biermann and Möller, 2019; Low and Buck, 2020). Researchers have encouraged more public engagement on SAI, not less.

5. Separating Slippery Slopes and Lock-In

Slippery Slope mechanisms should be differentiated from ‘lock-in’. While there is an implicit understanding in current research that Slippery Slopes and lock-in are separate (Cairns, 2014; Lin, 2020), there has yet to be an explicit separation and contrasting of the two concepts.

Lock-in is where a certain technology is entrenched in existing sociotechnical systems (Unruh, 2000; Erickson et al., 2015; Seto et al., 2016; Dornelles et al., 2020). Slippery Slope mechanisms by contrast are processes where technology can (but are not guaranteed to) become entrenched in existing sociotechnical systems. This is not to say that Slippery Slopes are dynamic mechanisms leading to a static lock-in outcome. Both are positive feedback loops. Lock-ins are the reinforcing feedback loops once a system state has become dominant, whereas Slippery Slopes are the feedbacks which potentially lead to dominance of a system state.

There is no single form of lock-in. Seto et al. (2016) differentiate between three types of lock-in: infrastructural, institutional, and behavioural (amended here to cognitive (Cairns, 2014; Trencher et al., 2020), which focuses more on subjective perceptions rather than individual habits).

1. Infrastructural lock-in: when incumbent infrastructures have overwhelming price-performance advantages and sunk investment/network costs that new technological entrants cannot compete against.
2. Institutional lock-in: when powerful special interests deliberately manipulate institutional dynamics (legal, political, organisational structures) to best suit their interests.
3. Cognitive lock-in: when norms and cultural values heavily favour specific ideas and behaviours.

Conversely, existing lock-in discussion in climate engineering literature has usually focused on a specific form of lock-in. For example, McKinnon (2018) frames lock-in as a matter of cost (infrastructural lock-in), while Parson and Buck (2020) frame lock-in as a matter of power and special interests (institutional lock-in). Both are valid conceptualisations of lock-in. Highlighting lock-in diversity, lock-in interactions, and

preceding Slippery Slope mechanisms (Table 3) would allow for a more robust and nuanced discussion of risks and governance. But their context and interactions within a more diverse suite of lock-ins is necessary to highlight in future.

	Infrastructural	Institutional	Cognitive
Material Cost Reduction	Cost reductions creating economic viability for technology, leading to widespread implementation.	Cost reductions of technology that aids the interest of powerful actors.	Initial cost reductions that lead to locked-in perceptions of a technology's practicality and affordability.
Sociocultural Cost Reduction	Reduction of barriers like extremeness aversion, allowing for widespread implementation.	Sociocultural dynamics that align with the interests of certain coalitions, potentially by design (Oreskes and Conway, 2010).	Normalisation and legitimisation that leads to locked-in perceptions of a technology's (un)desirability.
Power Altering	Innovations that align with the interests of already powerful actors, who provide support and investment, leading to earlier widespread implementation.	Initial power dynamics creating winning coalitions for special interests, that then shape institutions to best shape their needs.	Increasing the capacity of powerful actors to influence norms and public debate.

Political Momentum	Initial innovations which garner high expectations, thus attracting further support and leading to earlier widespread implementation.	Initial perceptions of power dynamics that encourage bandwagoning, creating winning coalitions for special interests, that then shape institutions to best shape their needs.	Initial perceptions of technology that lock-in perceptions of a technology's usability/feasibility.
Uncertainty driven increments	Uncertainty driven incremental research advances that eventually lead to large scale research by deployment.	Creation of institutions to address uncertainties via continued research.	Incremental advances, that individually do not seem to be relevant or consequential, that lead to rational apathy of an issue or technology.

Table 3 Lock-in Types and precursor Slippery Slopes

Escaping unproductive lock-in is a difficult and often prolonged affair, as seen with fossil fuels. SAI, and climate engineering more generally, is a nascent area with opportunity to avoid falling into lock-in that entrenches:

1. Technology with unfairly distributed costs and benefits;
2. Institutions which facilitate non-democratic decision-making and unequal distribution of costs and benefits;
3. And cognitive lock-in of uninformed and overly polarised public debate.

These lock-in conditions, briefly derived from the aforementioned types of lock-in, can form the bases of key boundaries (Baum and Handoh, 2014) and system states that governance should look to avoid. This is a starting point for further analysis. Further

clarifying aspects like ‘informed public debate’ are necessary, but these regardless can be north stars that anchor discussion and analysis.

Locking-out these undesired system states calls for action now that creates desired lock-ins (Seto et al., 2016). Lock-in is not a state to be avoided, but should be encouraged (when they function to exclude less desirable system states). de Kleijne et al., (2022) for instance recommend lock-in of carbon capture & utilisation technologies that more cheaply and reliably sequester carbon to lock-out less reliable alternatives. Active discussion of SAI’s lock-in trajectories is preferable to ‘sleepwalking’ (McKinnon, 2018) into lock-in driven by unchecked economic, social, and political forces.

Different policy instruments themselves carry different lock-in potential. A constitutional ban on SAI research would have a high lock-in. Whereas an Executive Order by a US President could be easily reversed in the future. Such flexibility can be beneficial, especially early when there is low certainty of desirable lock out. When addressing lock-in, the lock-in of specific policy instruments should be a critical consideration.

Promoting SAI lock-in does not necessarily mean promoting the research and implementation of SAI. Facilitating the lock-in of institutions that ensure fair distributions of costs and benefits may ultimately slow research, development, and implementation if SAI only provides skewed benefits with unfairly burdened costs.

6. Discussion and Conclusion

Understanding the Slippery Slope is critical to SAI research governance. There are genuine risks of overly rapid deployment that warrant attention. Even if SAI currently seems like an ‘upward struggle’ (Bellamy and Healey, 2018), initially small changes can spark non-linear feedback loops that can quickly transform an underdog into a frontrunner. Clarifying the Slippery Slope can also help better understand and allay Slippery Slope concerns when they are raised.

But extant discussion of the Slippery Slope in climate engineering literature is disorganised – different causal mechanisms are placed under the umbrella of the ‘Slippery Slope’ (see Supplementary Material). Recent calls to dismiss the Slippery Slope as outdated and unwelcome seem premature if extant discussion does not provide a clear indication of what the Slippery Slope is or how one could happen.

There are different types of Slippery Slopes that may occur (Table 2). The Slippery Slope concept is not ‘outdated and unwelcome’ solely because its core claim of inevitable deployment is incorrect. It is unwelcome because it belies the probabilistic and multifaceted nature of technological change. But this can be mitigated by disaggregating the Slippery Slope into clear causal mechanisms that can be assessed with improved accuracy and consistency.

This typology is also useful beyond the Slippery Slope concept – organising how change happens can help researchers and decisionmakers navigate the complexities of technological change and responsible innovation.

Slippery Slope dynamics do not function in isolation. They likely interact, a topic which while unexplored by this paper should be addressed in future research.

However, Slippery Slopes are not inevitable. They have counter mechanisms that can restrict runaway change. The counter mechanisms identified can help guide policy and governance to mitigate Slippery Slope risks and concerns.

Slippery Slopes are different to lock-in. Slippery Slopes are the feedback loops that can (but are not guaranteed to) lead to locked-in systems. Escaping existing lock-in is difficult and prolonged affair. As an emerging technology, there is opportunity to shape the lock-ins of SAI. SAI research should explore avenues for lock-in acceleration rather than avoidance. What should be locked-in is in question, but deployment with unfairly distributed costs and benefits, institutions with facilitate unfairly distributed costs and benefits, and norms that do not facilitate informed public debate should broadly be

locked-out. The 'threshold' for identifying when irreversible lock-in has occurred also has no perfectly clear answer. These starting points can inform future lock-in research.

A remaining analytical difficulty of the Slippery Slope is determining whether one is in progress (Walton, 2015). Identifying a Slippery Slope and lock-in may only be possible after lock-in has already occurred (Parker, 2014). The 'threshold' for whether something is locked-in has no perfectly clear answer. Actions to address Slippery Slope risks and concerns could incidentally do the opposite, for instance public engagement that incidentally sparks extremeness aversion when the objective is legitimisation. Furthermore, identification of clear and clean causal chains may be difficult in complex and dynamic issue areas. But this typology is not designed to be a prescriptive nor predictive tool of technological change. The typology is designed to bring much needed consistency to discussion when Slippery Slope concerns are raised.

This analytical difficulty also raises the issue of how to assess whether the actions taken have successfully resulted in the 'success' or 'failure' of locking-out undesired outcomes. If determining the presence of Slippery Slope or lock-in is difficult, how can one determine whether actions to prevent/achieve lock-in have been successful?

However, clear 'success' or 'failure' is a policy rarity. When is any decision or policy ever considered to have reached a state of clear success or failure? Successful policy may later fail or be reversed. Policy failures may still provide the foundation of policy development in the future. Sometimes the exact outcomes of any action are up for debate. But this does not stop policy attention in other contested and vague areas. Continuous adaptive management is critical. This paper provides a set of priorities to continuously work towards, rather than a simplistic criteria of policy success or failure.

While this paper attempts to clarify the Slippery Slope from an analytical perspective, uptake of this typology in public debate is a different issue. Unpacking the Slippery Slope's analytical basis makes sense for an academic audience, but not necessarily a non-academic one. The Slippery Slope's ambiguity may itself serve a political benefit,

precluding disaggregation and precision. How the Slippery Slope is used is just as important as what the Slippery Slope is, and is an area for further research.

Ultimately, should we research SAI? Do Slippery Slope concerns justify halting research or enacting highly restrictive research governance? A clear equivocal answer is out of the scope of this paper, and depends on subjective assessments like SAI's risks compared to climate change.

Rather, what can be said is that Slippery Slope concerns should not be dismissed. Technology is capable of rapid change. When potential risks of such change are high, they need engagement and attention. This will be most productive if vague and often used concepts like the Slippery Slope can be disentangled, rather than creating a web of inconsistency.

We can only know if we are at risk of slipping by understanding the lay of the land. This is a first attempt to survey the Slippery Slopes of researching SAI; to map the paths that could lead to a dangerous deployment.

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Foreword to Chapter 4: A Fate Worse Than Warming?

4.1. Place in the thesis

Alongside the assessment of research risks in the previous chapter, this chapter is an assessment of stratospheric aerosol injection's deployment risks. Answering Research Question 2b., and in line with the conceptualisations of risk complexity outlined in Chapter 1, this Chapter is a holistic assessment of deployment risks as a first step to risk comparison to climate change.

4.2. Key Conclusions

Under high uncertainty, looking at worst-case (or severe) risks can help provide much needed direction. Even with future uncertainty, we can be sure that we want to avoid the worst risks. This can provide the basis of a 'policy boundaries' approach to technological development, an extension of the famous planetary boundaries literature¹. Focusing on these extreme risks also allows for an initial comparison with climate change – compare the worst (or severe) risks between climate change and stratospheric aerosol injection.

This chapter puts forward a framework for holistic robust risk assessment. This includes assessing direct impacts, hazard interactions (how disasters can overlap, make other disasters more likely or severe)², systemic risk (how initial stressors can cascade and amplify across systems)³, and latent risk (risks which are only activated under different future conditions).

Discussion of severe downside and potentially catastrophic risks are not comprehensively assessed in prior literature, a pattern shared with the general climate literature⁴. The climate modelling of stratospheric aerosol injection often uses assumptions that greatly limit its broader generalisability and significance, for instance assumptions of a rational central planner⁵. Systemic risks and hazard interactions are underexplored in the literature.

Overall, the ‘average’ outcomes of stratospheric aerosol injections are likely safer than climate change. But the worst-case and severe outcomes of stratospheric aerosol injection are far worse than worst-case climate change. This is especially the case where there is a poorly coordinated ‘thick’ layer of cooling, greenhouse gas concentrations are still high, the global cooling is stopped for a sufficiently long period of time (for instance by another global disaster like extreme space weather), and there is insufficient backup infrastructure. Depending on the specific thickness of cooling, this would result in global warming of a degree or more in a timeframe of decades⁶. Worst case trajectories for climate change (RCP8.5) have a warming rate of ‘only’ 0.5°C per decade.

Over a single year, this would be highly unlikely and may be worth disregarding. However, over an entire deployment, which would last over 100 years due to the timeframes necessary for renewables and GGR, such risks become far more important. Effective governance to prevent these worst-case risks would be vulnerable to changing political interests and leadership, particularly over a 100-year timeframe.

4.3. Significance to the thesis

This chapter informs the deployment desirability of stratospheric aerosol injection. The severe risks of stratospheric aerosol injection are plausible. They are unlikely, but over a 100-year timeframe, unlikely events tend to happen. This chapter outlines contours for effective policy that would limit these worst-case risks, but these hinge on implausible long-term consistency in politics and international policy. The extreme risks of stratospheric aerosol injection, and the implausibility of comprehensively and consistently limiting them, point to undesirability as a response to climate change.

Together with the previous chapter, this chapter helps build a holistic risk profile of stratospheric aerosol injection. Chapter 3 explores the ways in which deployment risks could arise, and this chapter finds that such deployment risks would be severe.

This chapter has been published in *Frontiers in Climate*. Appendix 2 has companion article for public audiences published in *The Conversation*, and Appendix 3 is a companion article in the *Bulletin of the Atomic Scientists*.

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Chapter 4: A Fate Worse Than Warming? Stratospheric

Aerosol Injection and Global Catastrophic Risk

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Abstract

Injecting particles into atmosphere to reflect sunlight, stratospheric aerosol injection (SAI), represents a potential technological solution to the threat of climate change. But could the cure be worse than the disease? Understanding low probability, yet plausible, high-impact cases is critical to prudent climate risk management and SAI deliberation. But analyses of such high impact outcomes are lacking in SAI research. This paper helps resolve this gap by investigating SAI's contributions to global catastrophic risk. We split SAI's contributions to catastrophic risk into four interrelated dimensions:

1. Acting as a direct catastrophic risk through potentially unforeseen ecological blowback.
2. Interacting with other globally catastrophic hazards like nuclear war.
3. Exacerbating systemic risk (risks that cascade and amplify across different systems).
4. Acting as a latent risk (risk that is dormant but can later be triggered).

The potential for major unforeseen environmental consequences seems highly unlikely but is ultimately unknown. SAI plausibly interacts with other catastrophic calamities, most notably by potentially exacerbating the impacts of nuclear war or an extreme space weather event. SAI could contribute to systemic risk by introducing stressors into critical systems such as agriculture. SAI's systemic stressors, and risks of systemic cascades and synchronous failures, are highly understudied. SAI deployment more tightly couples different ecological, economic, and political systems. This creates a precarious condition of latent risk, the largest cause for concern. Thicker SAI masking extreme warming could create a planetary Sword of Damocles. That is, if SAI were removed but underlying greenhouse gas concentrations not reduced, there would be extreme warming in a very short timeframe. Sufficiently large global shocks could force SAI termination and trigger SAI's latent risk, compounding disasters and catastrophic risks. Across all these dimensions, the specific SAI deployment, and associated governance, is critical. A well-coordinated use of a small amount of SAI would incur negligible risks, but this is an optimistic scenario. Conversely, larger use of SAI used in an uncoordinated manner poses many potential dangers. We cannot equivocally determine whether SAI will be worse than warming. For now, a heavy reliance on SAI seems an imprudent policy response.

Hothouse Earth or Shithouse Earth?

Could the risks of large-scale solar geoengineering be worse than the dangers posed by climate change? Many concerns have been expressed over geoengineering the Earth's climate. These tend to centre on solar radiation management (SRM) methods, particularly stratospheric aerosol injection (SAI). These range from fears over negative unintended effects on ecology, political conflict, mitigation deterrence, to ethical objections. Given the breadth of objections, it is quite clear that SAI would be iatrogenic in some way. Like some medical interventions, SAI may have adverse side-effects and complications. The question is whether it could be worse than the problem it is seeking to remedy: climate change.

There is a wealth of information on the different risks posed by climate change (although notably little on high-end warming scenarios), yet few attempts to compare this to the potential damages of SAI. This is unsurprising since there have been limited attempts to systematically analyse the myriad of threats posed by SAI.

We address this gap by analyzing the severe downside risks of SAI. We do not directly compare the risks posed by SAI and climate change in this paper. Rather, we provide an analytical foundation for future comparative analyses. In this article we ask: what are the plausible contributions of SAI to global catastrophic risk (GCR)? To the best of our knowledge this is the first attempt to offer a novel, comprehensive framework for comprehending the contributions of SAI to GCR. As noted in section: *A Framework for Unraveling Global Catastrophe*, this is a useful and original step forward for the nascent field of studying GCRs. This is not just simply adding up SAI's potential negative impacts. It requires understanding how SAI could trigger or worsen other large-scale threats (such as nuclear warfare) or systemic risks. Understanding extreme downside risks can also help provide direction for policy and governance. The future may be hazy, yet avoiding the extreme downsides is a priority for risk management under uncertainty. To guide our investigation, we put forward a novel framework for understanding how SAI, or any other complex risk, contributes to GCR. We then use this to review and discuss the existing evidence on SAI's critical threats.

In Table 1 we provide a brief set of definitions of the key terms we use throughout this paper.

Term	Definition
<i>Climate Engineering</i>	Large-scale, deliberate interventions into the Earth system to mitigate the effects of negative impacts of climate change (The Royal Society, 2009).
<i>Extinction Risk</i>	A risk that could plausibly cause human extinction.
<i>Global Catastrophic Risk (GCR)</i>	A risk that could plausibly cause a loss in global population of 10-25% (Atkinson, 1999; Global Challenges Foundation, 2016) and a disruption to one or more global critical systems.
<i>Solar Radiation Management</i>	Measures which impact the albedo of the Earth system in order to mitigate the impacts of climate change.
<i>Stratospheric Aerosol Injection</i>	The injection of light-reflecting chemical, such as sulphur dioxide, into the stratosphere.
<i>Systemic Risk</i>	The ability for an individual disruption or failure to cascade into system-wide and cross-system failures (Centeno <i>et al.</i> , 2015) due to structural conditions.
<i>Latent Risk</i>	Risk that is dormant under one set of conditions but becomes active under another set of conditions.
<i>Termination Shock</i>	A large and rapid increase in warming after the cessation of SRM measures.
<i>Buffering</i>	A period of roughly several months following the cessation of SAI where effects of termination shock do not occur. Redeployment of SAI during this period would ensure that termination shock does not occur (Parker and Irvine, 2018).

Table 1: Definitions

Our approach makes use of a structured literature review and systems mapping exercise. We use our novel framework to structure a literature review covering studies relevant to the risks of SAI. For each area we highlight the level of evidence and uncertainty, and draw out some key implications. The nature of the risk will depend on the specifics of the geopolitical situation and the SAI intervention. We explore this through a causal-loop diagram (Figure 1) which plots out the connections between the level of risk, the amount of SAI loading, the level of international coordination and other key variables.

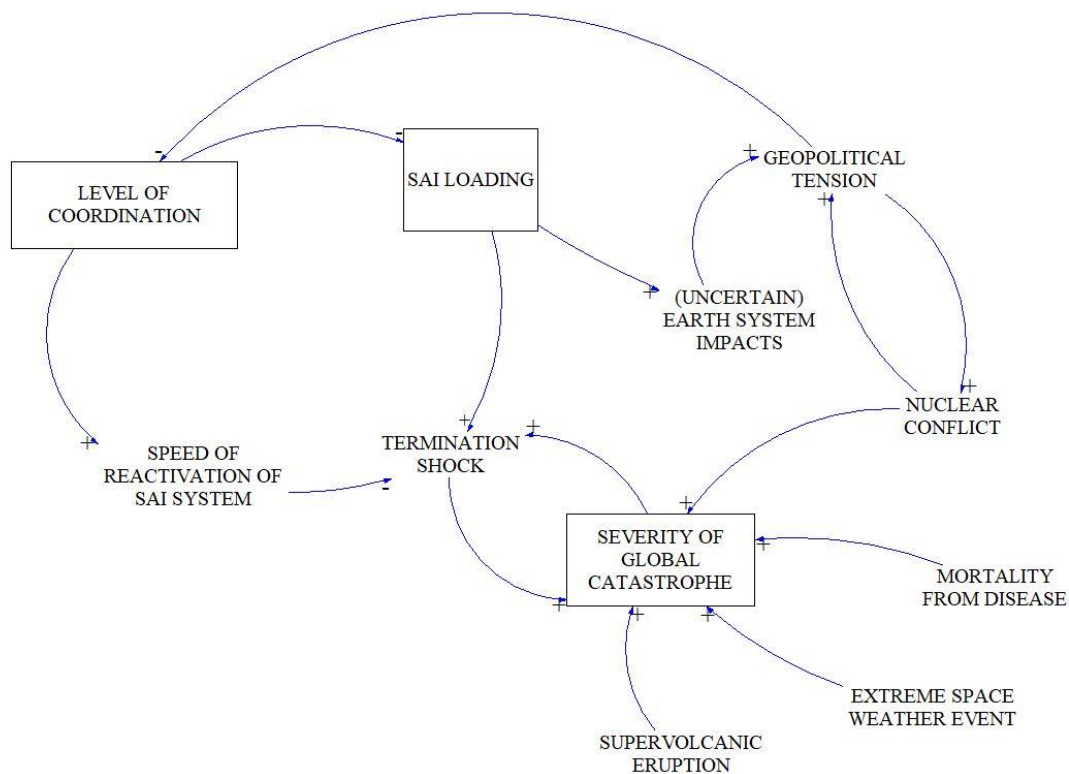


Figure 1: The SAI-GCR System⁴. This is a causal loop diagram. A complete line represents a positive polarity (meaning an amplifying feedback, not necessarily positive in a normative sense) and a dotted line denotes a negative polarity (meaning a dampening feedback).

⁴ This is a causal loop diagram. A complete line represents a positive polarity (meaning an amplifying feedback, not necessarily positive in a normative sense) and a dotted line denotes a negative polarity (meaning a dampening feedback).

Note that for most of this paper we address SAI in the abstract. The exact potential damage imposed by SAI would vary the way it is deployed. In section: *Discussion: Building the Policy Boundaries for Climate Engineering*, we discuss how the method of deployment creates different impacts. Throughout the paper we assume a “default” deployment method of SAI to be the continuous multi-decadal global use of planes with multiple injection locations, guided by a global cooperative endeavor led by states with private sector contributions, with an overall objective to respond to global warming. Deployment “thickness” (how much warming is masked) is a particularly important variable. We flag thickness throughout our analysis. Where we discuss the risks of other potential forms of deployment we directly state so.

We proceed by outlining our framework, before examining SAI's direct catastrophic risks, SAI's interaction with other catastrophic hazards, SAI's potential input to systemic risk, and finally SAI's influence on latent risk. We then discuss how different methods of deployment could lead to different risks and what the policy implications of our analysis are. To avoid the critical downside risks we consider throughout the paper, SAI governance would have to be near perfect for multiple decades.

A solution that is almost impossibly difficult to implement well, and that plausibly threatens catastrophe if implemented poorly, is not a good solution.

Whether this is preferable to climate change remains to be seen.

A Framework for Unravelling Global Catastrophe

There is no agreed framework for understanding the contribution of different phenomena to GCR. Most studies and reports on GCRs rely on analyzing a set of large-scale “GCR-level” hazards (Bostrom and Cirkovic, 2008; Global Challenges Foundation, 2016).

Usual suspects include anthropogenic risks such as nuclear weapons, climate change, and

more speculatively, Artificial General Intelligence⁵, biologically engineered pandemics, and natural risks such as super volcanoes and asteroids. While there have been some alternative frameworks for classifying GCRs (Avin et al., 2018; Liu et al., 2018; Baum and Barrett, 2019), these have yet to be widely adopted. They are also disconnected from relevant literature on systemic risk. Moreover, while they are helpful in classifying a given hazard, they do not act as aids in understanding how much a given event or system could contribute to overall levels of GCR or extinction risk.

There are several problems with the typical, hazard-centric approach. First, it is unclear how these hazards are decided on. Second, a risk is composed of hazards, vulnerabilities, and exposure, not just individual threats (IPCC, 2012; Avin et al., 2018; Currie and Ó hÉigeartaigh, 2018; Liu et al., 2018). Third, the different hazards are treated as disconnected when they frequently have similar institutional drivers. Fourth, it ignores systemic risk, particularly the ability for a set of smaller, diffuse risks to scale to a global and cataclysmic level due to the fragility and interconnectedness of critical systems. Risk is no longer just about hazards, vulnerabilities and exposures. Comprehensive risk assessment also needs to consider responses, as well as the common drivers across these four risk determinants (Reisinger et al., 2021; Simpson et al., 2021).

We incorporate all of these aspects in a four-stream framework for understanding the contribution of a system or event to GCR. Hazards are directly assessed through the first two streams, while the focus on systemic risk analyses potential vulnerabilities. Latent risk explores the often neglected possibility of vulnerabilities that are hidden in the short-term. Exposure and responses are articulated throughout the analysis. The overall analysis informs a discussion on policy boundaries. Our analysis rests not upon having a particular probability of occurring. Instead, we focus on what is plausible (rather than “merely possible”): consistent with our background knowledge of physical and social systems (Betz, 2016). Understanding risks which are plausible, high-impact, but low or

⁵ A single algorithmic system that can perform tasks at a level similar to humans across a broad range of cognitive domains.

unknown-probability is critical for robust decision making under uncertainty (Ord et al., 2010; Kunreuther et al., 2013; Wagner and Weitzman, 2015). For example, making decisions on the “better” worst case is central to the Maximin approach.

Our four-stream framework is as follows:

1. The first stream focuses on directly catastrophic impacts. A direct contribution refers to ways in which the impacts caused by SAI could alone plausibly cause sufficient mortality and morbidity without considering wider knock-on effects.
2. The second stream examines how SAI could interact with other high-impact hazards such as nuclear war.
3. The third investigates how SAI could contribute to and be affected by systemic risk. Systemic risk focuses on how structural conditions and multiple small stressors can lead to widespread collapse or synchronous, reinforcing failures (Homer-Dixon et al., 2015). Indeed, complex systems can undergo rapid degeneration even without large shocks. They frequently organize into critical states in which small perturbations quickly cascade into calamity (Homer-Dixon, 2008; Helbing, 2013).
4. The final stream focuses on SAI's latent risk. Latent risk focuses on deciphering how SAI could pose threats that manifest under post-catastrophe conditions, such as in the aftermath of societal collapse.

Together, these different factors provide a comprehensive framework for comprehending how SAI could raise or lower overall levels of GCR in the world. The framework is intended to be a first step to risk comparison, in this case climate change and SAI.

Historically, comparison between the two have been a rhetorical device to justify SAI. This is by no means a straight-forward juxtaposition since the two interact [for example, through mitigation deterrence: actors may be less open to ambitious emissions reduction if there is a “technofix” on the horizon (McLaren, 2016)] and any analysis hinges on subjective judgements about climate sensitivity, tipping points, adaptive capacity, and the likelihood of international cooperation. There is also the issue of which precise baselines should be used for comparison (McLaren, 2018): what should climate change or SAI be specifically compared against? In addition, how should the two be compared? Given the

high uncertainties for both climate change and SAI, is a Maximin analysis of the “better” worst case a prudent or viable approach? Given these difficulties, we do not look to provide a definitive answer or quantitative analysis. Ultimately, we are not just comparing two different sets of risks, but two separate Earth system states (Jebari et al., 2021) with different winners and losers. Navigating these entangled risk analyses is an area for future analysis, but analysis that this paper can hopefully inform.

Nonetheless, any public deliberation and democratic decisions need to rest on comparable evidence and information. Any action is bettered by risk assessment, even if it is always mired in uncertainty. This article provides an initial and incomplete basis for informing such discussions. Imperfectly mapping out risk trade-offs is preferable to sleepwalking (McKinnon, 2018) into a dangerous future.

Directly Catastrophic Impacts: Ecological Blowback?

Could SAI lead to directly⁶ catastrophic ecological impacts? Existing studies highlight a raft of potential negative consequences. But the specific nature of these impacts, and their contributions to catastrophic outcomes, depends on the specific SAI implementation. This is an issue of high uncertainty, particularly regionally.

The projected local ecological effects of SAI are mixed and uncertain, depending on the specific analytical approach and specific SAI deployment. Monsoon areas would likely face a drop in precipitation under large scale SRM (Tilmes et al., 2013; Reynolds et al., 2016), but this focuses on SRM in the abstract and may not be fully applicable to SAI. Many regions could face a seasonal under- or over-compensation in rainfall (compared to a high warming average (RCP8.5) from 2010 to 2030, and assuming SAI is implemented to mask 5 degrees of warming) (Jiang et al., 2019). Effects on hydrological systems

⁶ This section studies the potential *direct* catastrophic impact of SAI, not the impacts of termination shock (which are almost guaranteed to be catastrophic if used to mask high levels of warming and if SAI undergoes a indefinite suspension (Trisos *et al.*, 2018)). By directly we refer to clear causal relationships with less than two degrees of separation.

would be regionally diverse and uncertain due to potential changes in non-linear variables including surface runoff, evapotranspiration, rainfall levels, and distribution (Dagon and Schrag, 2016, 2019). These fine-grained changes in weather could then affect vegetation. Plant communities could transform their structure, traits, and geographical range, particularly under larger swifter SAI deployments (Zarnetske et al., 2021). While SAI might offer salvation to climate vulnerable vegetation it will depend on deployment timing. Some communities may already be committed to at least local extinctions before SAI is deployed. SAI would likely result in ecological trade-offs with some communities benefitting and others suffering. The exact nature of these trade-offs is uncertain and needs further study (Zarnetske et al., 2021). The key theme here is that SAI would likely have a range of impacts on many ecological systems. But how these would play out is highly uncertain, particularly at regional scales. Impacts hinge on the inherent uncertainties within complex ecological systems, varied comparative baselines, and the specific SAI deployment.

The overall direct impacts of SAI, while uncertain, do not currently seem to constitute a catastrophic threat. Whether SAI would cause greater risks in terrestrial, freshwater, marine systems than climate change is unclear and depends on SAI's specific deployment configuration. Higher levels and swifter deployment of SAI would mean greater potential for disastrous impacts (Trisos et al., 2018; Zarnetske et al., 2021). Additional considerations like seasonal (Lee et al., 2021) or hemispheric (MacMartin et al., 2017) deployment further affect potential impacts.

There is a paucity of research on SAI impacts (Irvine et al., 2016, 2017; MacMartin et al., 2016; McCormack et al., 2016; but see Schäfer et al., 2015 for an exception), particularly so for catastrophic or worst-case impacts. This has been the case for climate modeling literature in the past as well (Brysse et al., 2013). Climate modeling is often an exercise in “betting on the best case” (Geden, 2015; Jehn et al., 2021). Others have noted this idealistic tendency for SAI modeling (Low and Honegger, 2020), for example limiting SAI use to only halving warming (Irvine et al., 2019) or limiting SAI deployment to spring (Lee et al., 2021). These idealized approaches in theory could reduce negative

impacts associated with SAI. Yet their likelihood is questionable due to optimistic assumptions of multi-decadal international cooperation (see section: *Politics*).

The possibility of dangerous ecological tail-risks depends on the level of cooling. Initial game theoretic research indicates the possibility of overcooling if SAI is pursued by uncoordinated actors (Abatayo et al., 2020). Negative impacts which are projected to be relatively minor in existing studies, for example sulfate deposition impacts on terrestrial ecosystems (Kravitz et al., 2009; Vioni et al., 2020b), may become major ecological issues if SAI is deployed to far more of an extent than envisioned. Similarly, a poor choice⁷ of aerosols could result in large-scale ozone depletion (Heckendorn et al., 2009; Keith et al., 2016). It is unclear whether, in these extreme cases, biophysical impacts would revert to their pre-SAI state once SAI is removed. Modeling on “worst” cases is thus critical in informing SAI's desirability. Exploring uncoordinated scenarios with the (simultaneous) use of different aerosols, different desired extents of cooling, and implementation by a small club, would all be helpful complements to existing idealized modeling scenarios.

Regardless of how developed our understanding on SAI impacts become, there will always be inherent uncertainty. When dealing with a complex system like the climate there is always the chance that a black swan is lurking in the dark.

Some commentators have downplayed the potential of unknown impacts due to the availability of historical analogs, namely historically severe volcanic eruptions (Halstead, 2018). Improvements in modeling, a gradual implementation, and a cessation if unacceptable negative impacts are found could also lessen the likelihood of an unforeseen catastrophic tipping point.

⁷ Commonly proposed coolant agents could be benign or even increase ozone thickness (Pitari *et al.*, 2014; Irvine *et al.*, 2016) (though this is a relatively recent shift).

None of these reasons are causes for comfort. Modeling, regardless of improvements, may simply be incapable of capturing rare tipping-points and is not intended to accurately predict or foresee non-rational political dynamics (Elsawah et al., 2020). In addition, a gradual rational phase-in and phase-out relies on optimal governance conditions. Overly rapid deployment due to ‘free-driving’⁸ (Weitzman, 2015), or overly slow phase-out due to technological or infrastructural lock-in (Seto et al., 2016) are entirely plausible. Moreover, SAI impacts may also not follow the pathway of historical analogs. The core rationale of SAI is to manufacture the cooling effect of a volcanic eruption in a “safe” manner, not replicate volcanic processes. Deviance from historical analogs is especially a possibility if the choice or mix of aerosol is radically different. This is particularly the case since climate change and human-pressures are already pushing ecological systems into novel states (Williams and Jackson, 2007; Williams et al., 2021). SAI would push systems into further novel states that make unseen ecological responses likely (McKinnon, 2018).

Our understanding of both Earth systems and the likely contours of deployment are too weak for us to rule-out a potentially catastrophic form of ecological blow-back. For now, the literature points to SAI having numerous impacts. But none seem remotely capable of being a GCR, particularly if SAI deployment were limited. Nonetheless, the Spectre of an unforeseen tipping point in the Earth's climatic system remains.

⁸ A ‘free-driver’ problem (or ‘forced rider’) is where a single or limited number of actors’ actions create negative consequences that the rest of the system must endure. In this case, a limited number of actors could implement SAI, and the rest of the world’s economic and political systems would have to deal with the consequences. This can be seen as the ‘opposite’ to a conventional ‘free rider’ problem where benefits flow onto non-cooperative third parties,

Interactions with Other Global Catastrophic Hazards

The impacts of SAI, or any other catastrophic risk, should not be assessed in isolation (Baum et al., 2013). Different catastrophic hazards⁹ have interactions. One could potentially trigger another and/or worsen its effects. Climate hazards for example have been shown to compromise governments' ability to provide effective responses to COVID-19 (Phillips et al., 2020). The potential for one global shock to ignite and amplify another has previously been dubbed “double-catastrophes.” Baum et al. (2013) suggest that this could be the case if nuclear war or a pandemic were to disrupt an SAI system, leading to abrupt termination shock. GCHs which are simply a matter of probability, like extreme space weather or a volcanic eruption, may also coincide through pure bad luck.

In this section we consider both a broader array of hazards and how SAI could trigger and interact with them. This will not be an exhaustive comparative analysis of all possible GCHs. Instead, we focus on hazards that have clearly established causal relationships, relatively well-developed literatures, and some empirical track record of their impacts. Our analysis suggests that the possibility of SAI sparking other GCHs are tenuous. SAI could only plausibly contribute to large-scale conflict and potentially nuclear war. The possibilities of SAI exacerbating other GCHs are more concerning. SAI has the worrying ability to significantly heighten the impacts and mortality of any global catastrophe due to termination shock.

Volcanic Eruption

A large volcanic eruption would demand rapid SAI adjustments. While severe overcooling seems unlikely [the cooling of SAI and volcanic winter are not additive (Laakso et al., 2016)], SAI should be rapidly scaled down in a matter of weeks

⁹ Instead of Global Catastrophic Risks, this section focuses on Global Catastrophic Hazards. Risks include vulnerability and exposure. Instead, this section focuses on the interactions between different specific hazards and SAI.

(MacMartin et al., 2019). Laakso et al. (2016) assume a relatively thick SAI injection (offsetting roughly a doubling of carbon dioxide from preindustrial levels). The prudent course of action for thinner SAI is unclear. However, the SAI adjustment in a volcanic future is not simply one of scale down. SAI injection may need to increase in the opposite hemisphere to the volcanic eruption to ensure a more uniform global temperature (MacMartin et al., 2019) (a high temperature variance across hemispheres can have severe adverse impacts on precipitation and drought dynamics).

Adjusting the SAI level may seem straight-forward but depends on an informed, rapid political response. There are reasons to doubt this would be forthcoming. First, the technical demands may prove too much for cumbersome domestic and multilateral politics. These include potentially politically vexing dilemmas over the balance between scaling SAI up and down on different hemispheres, whether to inject SAI at new locations or “thicken” existing deployments (MacMartin et al., 2019), or whether SAI should be scaled down at all. A second and novel addition is that a volcanic eruption would not solely affect temperature. Many pinch points of global supply systems are near active volcanic areas. Even modest volcanic eruptions could lead to disruption and catastrophic economic system collapse (Mani et al., 2021). The difficulty of coordinating regional SAI adjustments would be compounded by sub-optimally functioning supply systems and general economic and political chaos.

While the interactions between a volcanic eruption and SAI currently seem to have only modest direct contributions to catastrophic risk, the highly political decisions of a volcanic-SAI world may lead to political ruptures and ineffective SAI governance.

Space Weather

Solar flares, coronal mass ejections, and associated solar radiation and geomagnetic storms, can lead to widespread damage to terrestrial, avionic, and space infrastructure. The fear for SAI is that a “black sky” event could disrupt and knock-out critical SAI infrastructure. Yet there have been no attempts thus far to investigate SAI-space weather interactions. We examine SAI interactions with an Earth-bound space weather event

roughly on par or worse than the 1859 Carrington Event— the benchmark for extreme space weather events (Green and Boardsen, 2006). A current day Carrington Event would likely lead to widespread electrical failure and disruption for multiple months at minimum, potentially years (Eastwood et al., 2017; Loper, 2019; Ritter et al., 2020).

Extreme solar events are difficult to accurately and timely forecast. They are essentially random events¹⁰ which provide little forewarning. Solar radiation can travel at such high speeds that an extreme coronal mass ejection would likely reach Earth in less than a day. Other radiation and energized particles travel at or close to light speed—8 min to reach Earth. Even with the earliest detection possible there would be little response time (Royal Academy of Engineering, 2013). It would be a late flinch to an oncoming blow.

The impacts of extreme space weather events are vast. Aviation, satellite, and general electronic infrastructure are especially vulnerable. Energized particles can affect memory cells, for example changing a bit from a 1 to 0 and vice versa, that lead to erroneous commands or overall hardware failure (Jones et al., 2005). Global navigation and communication systems would experience disruption and downtime that could last several months (alternative navigation systems, like the US Alternate Position Navigation and Time programme, may still be affected by electrical damage) (Goodman, 2005; Royal Academy of Engineering, 2013). Aircraft crew would have greatly limited airtime due to limits of safe radiation exposure (Jones et al., 2005). Flights at higher altitudes and closer proximity to the Earth's poles would be unlikely to continue (Dyer et al., 2003; Jones et al., 2005; Alvarez et al., 2016). The use of automated aircraft would be

¹⁰ There is unresolved discussion as to whether extreme space weather events follow a power law or lognormal distribution (Riley and Love, 2017; Kataoka, 2020). The estimated probabilities of an extreme space weather event in the next decade or so range from 0.46%-1.88% (Moriña *et al.*, 2019), 4-6% (Kataoka, 2013), to roughly 20.3% (Riley and Love, 2017). This is all to say that while there are efforts to better understand the probability of extreme space weather events, there is little agreement in what the precise probability is. Nonetheless, even the lowest estimates are not negligible. In the face of such uncertainty, we take the lead of other policy analyses in the area (Royal Academy of Engineering, 2013; National Science and Technology Council, 2019) and characterize these events as essentially random.

compromised by widespread electrical and avionic damage. Especially alarming is that SAI would likely depend on vulnerable aviation, satellite, and general electronic infrastructure for deployment, monitoring, impact attribution determination, calibration, and modulation.

Impacts of space weather events are not limited to human infrastructure. Substantially increased UV output can influence the Northern Hemisphere jet stream, ozone production (and ozone UV absorption and warming), and precipitation patterns (Jones et al., 2005). These systems, particularly precipitation, are the same systems that SAI is likely to greatly affect. Interaction between these impacts is currently unclear.

These disruptions appear enough to halt even a robust SAI system. Even with high uncertainties of potential infrastructural impacts (Royal Academy of Engineering, 2013; National Science Technology Council, 2019) and the nature of the event itself (Liu et al., 2014; Pulkkinen et al., 2015; Eastwood et al., 2017), the limited evidence so far indicates that SAI infrastructure would be vulnerable and exposed to damage, thus leading to termination shock if SAI was sufficiently thick (see section: *Latent Risk and SAI*). In the aftermath of an extreme space weather event, continued implementation or preservation of SAI infrastructure would have to compete for limited government attention. Damage would be widespread and international—ranging from railway failure (Ptitsyna et al., 2008; Wik et al., 2009; Eroshenko et al., 2010) to power failure (Royal Academy of Engineering, 2013; Juusola et al., 2015; Kai-rang et al., 2015; Matandirotya et al., 2015) to failure of satellite infrastructure (Odenwald et al., 2006). Governments and resources would be stretched thin and SAI reimplementation may be neglected. An extreme space weather event could lead to severe economic and infrastructural shocks (Loper, 2019) that make continued SAI deployment infeasible. At worst, widespread power failures could lead to ripple effects across food, health, and transport systems that extend recovery time potentially into decades, driving modern societies back to a more fractured pre-electronic state (Loper, 2019). It is unclear how SAI, with its high technical and information demands (MacMartin et al., 2019), could continue under these conditions. Troublingly, mitigation options are currently limited and highly depend on future (but

relatively well-known) scientific and engineering solutions (National Science Technology Council, 2019). Considering the speed of space weather events, SAI infrastructure would have to be built to be resilient (with technology which does not currently exist) from the offset.

SAI is ultimately highly vulnerable to extreme space weather events. Widespread electrical damage would compromise SAI redeployment, making a termination shock highly likely and worsening the already catastrophic impacts of an extreme space weather event.

Nuclear Weaponry

SAI would worsen any nuclear winter and our recovery from it. A nuclear war could occur due to either an accidental strike leading to escalation, or a full-blown exchange. Even a relatively smaller conflict between Pakistan and India would have global ramifications. The background risk of incidental or inadvertent nuclear deployment is present unless there is total nuclear disarmament (Barrett et al., 2013; Baum et al., 2018). In addition to nuclear winter, the physical blast, ionizing radiation, and electromagnetic pulse (EMP) would all contribute to widespread and severe damage of electronic infrastructure (Baum and Barrett, 2018), including SAI infrastructure. Indeed, EMPs are similar in effect to the “black-sky” events discussed in section: *Space Weather*. This leads to two key concerns. The first is the combination of SAI cooling with nuclear winter conditions, the second is the grim mixture of nuclear cooling combined with termination shock.

The combination of SAI's existing cooling and additional nuclear winter would lead to short term overcooling, followed by medium- or long-term overheating due to termination shock (Baum et al., 2013). It would be global frost followed by global furnace. Alternatively, there may be the potential for SAI and nuclear winter layering to spark non-linear or unexpected cooling effects. This is an area that justifies further study. There is modeling on the impacts of a nuclear detonation, comparison of nuclear and climate threats via the “climate-nuclear nexus” (Scheffran et al., 2016), and modeling on

the impacts of SAI deployment and termination shock. Yet so far nothing integrates these two separate bodies of knowledge. The oversight is interesting given the entangled histories of climate science and nuclear weapons research (Edwards, 2012). In any case, such rapid swings in global temperature would be unprecedented for the Earth system and humanity.

A key question is whether a disrupted SAI system could be revived during nuclear winter to prevent a termination shock summer, and whether SAI was masking sufficient warming for termination shock to occur (see section: *Latent Risk and SAI*). But there also are reasons to believe that the re-establishment of an SAI system would not be able to occur during the buffer period in the wake of a nuclear cataclysm. First, technological damage may be so severe that timely deployment is impossible. Backup infrastructure like aircraft (and associated supporting infrastructure such as air traffic control) may be damaged beyond repair or be grounded for security purposes. Second, political and policy attention would likely be focused on other post nuclear issues, such as disaster recovery and the creation of alternative food systems. As with other disasters, governments would be stretched thin and may prioritize these more short-term issues. Lastly, a post-nuclear world would likely exhibit a lack of international cohesion that is seen as an enabling condition for effective SAI (Horton and Reynolds, 2016; Chhetri et al., 2018; Jinnah, 2019). Discussions over SAI have already been deadlocked (McLaren and Corry, 2021). It seems unlikely that a world of post-conflict lessened trust would be more conducive to speedy decision-making. Different countries may drop out of implementation, further complicating SAI deployment configurations, possible regional impacts, and concordant policy responses. Disagreement over resource allocation is likely to arise, as is the case for many disaster recoveries (Platt, 1999; Cohen and Werker, 2008; Doan and Shaw, 2019).

The presence of thick SAI greatly increases the potential consequences of nuclear warfare, and vice versa. The rapid temperature swings involved with a nuclear winter and termination shock summer would likely lead to ecological disaster, and a chaotic post-nuclear world would not likely reimplement SAI in a timely sensible manner.

Pandemics

A pandemic that reaches the level of a GCR could be enough of an economic or population shock to sever an SAI system (Baum et al., 2013). Whether the system could be reactivated during the buffer period would depend on both the severity as well as the length of the pandemic. COVID-19 provides a chilling reminder that states are not rational nor necessarily cooperative during a disease disaster. COVID-19, a far cry from being a GCR, has spawned fragmented responses and cases of both vaccine nationalism and vaccine diplomacy. Such multilateral behavior does not engender confidence that a pandemic with a significantly higher mortality rate would lead to survivors coolly and collectively reactivating an SAI system whilst dealing with the outbreak. Other issues, like keeping healthcare systems afloat, would likely be an overwhelming priority. With resources and capacity stretched thin, SAI may be neglected. A pandemic would be a severe shock to political and economic systems that may preclude continued SAI use. Not least rational, well-governed, well-resourced SAI use. Whether this risks termination shock depends on the amount of warming masked.

There are also reasons (albeit speculative) to believe that SAI could contribute to a pandemic. SAI induced temperature changes and uncertain regional climatic effects can alter disease transmissions (Carlson and Trisos, 2018; Carlson et al., 2020). This could in turn affect pandemic dynamics. As with general ecosystem impacts (section: *Directly Catastrophic Impacts: Ecological Blowback?*), a larger and quicker SAI deployment can be expected to have more severe impacts. Critical nodes in urban and health systems may become exposed to diseases that are beyond typical immunity or resistance (see section: *Health* for more on SAI-health interactions). This could be the spark for a pandemic spread, particularly if decision-makers are unprepared to make early and rapid response measures. However, the most worrying (but thus far neglected) concern would be effects on animal populations. Similar concerns of low or lapsed immunity or resistance would apply to animal populations and new disease vectors. But animal populations would lack similar healthcare systems to keep disease spread at bay. Many contemporary pandemics have resulted from cross-species spill over (Christophersen and Haug, 2006; Cheng et al.,

2007; Quammen, 2012; Plowright et al., 2017; Borremans et al., 2019; Johnson et al., 2020; Morens et al., 2020), including the 2009 Swine Flu Pandemic from pigs and birds, and the 2013–2016 Ebola Epidemic and COVID-19 Pandemic from bats. Altered animal disease dynamics, particularly those stemming from unpredictable regional SAI impacts, may increase the frequency and severity of future pandemics.

The Systemic Risks of Climate Engineering

Both previous societal collapses and disasters in the modern world are marked more by the accumulation of many stresses leading to failure, rather than single abrupt shocks destroying systems (Homer-Dixon, 2008; Haldane and May, 2011; Helbing, 2013). Seemingly modest stressors can cascade to catastrophe. This section analyses the potential of SAI to create and be impacted by biophysical and political stresses which contribute to global systemic risk.

The world currently exists in a deeply interconnected, and increasingly homogenous state which is prone to systemic risk (Helbing, 2013). One ship blocking the Suez Canal in March 2021 led to losses of roughly \$6–10 billion (Russon, 2021). More serious stressors could lead to far more severe consequences. The economic and political state of the world would be central in determining whether risk cascades. It is unclear how SAI could or would adjust the structure of the globalized economy. Hence, instead we focus on a few critical systems that SAI might be expected to impact and where there have been initial attempts to gather evidence: agriculture, health, and international politics¹¹. SAI would likely not alter any of these system structures, but would rather aggravate existing systemic vulnerabilities.

Agriculture

SAI's effects on temperature and precipitation distributions would likely affect agricultural systems. The precise nature of these impacts are unclear (Kortetmäki and

¹¹ We exclude other areas such as telecommunications and finance due to a lack of published literature.

Oksanen, 2016; Irvine et al., 2017; Svoboda et al., 2018; Pamplany et al., 2020). For example, some studies have shown that the low temperature high carbon dioxide environment of a SRM deployment might increase yields: maize yields may increase in China (Xia et al., 2014), as could overall global yields of maize, wheat, and rice (Pongratz et al., 2012). On the other hand, solar dimming might reduce yields of groundnut in India (Yang et al., 2016) or offset benefits of reduced temperature (Proctor et al., 2018). These effects would all further differ across crop and area. The differing approaches to analysis [Xia et al. (2014) focus on SRM to offset a 1% increase in carbon dioxide from preindustrial levels for 50 years, whereas Pongratz et al. (2012) focus on SAI masking carbon dioxide concentrations of 800 ppm] as well as use of outdated equatorial injection in these studies (see section: *The Means of Deployment*) make clear conclusions difficult to discern. The main point is that SAI would affect agriculture, but the precise impacts are unknown.

Regardless, the sensitivity of these key staple crops is alone cause for concern. Small variations in yields of staple crops could induce disproportionate price fluctuations and cascades into socio-political violence, particularly in areas with political instability and weaker governance (Natalini et al., 2015, 2017; Richards et al., 2021). Additional uncertainties with attribution between SAI and agricultural yields could compound potential political difficulties.

Even in the case that SAI provides agricultural benefits, these are likely to be marginal if other issues affecting agricultural productivity, such as habitat loss and soil degradation, continue unabated (Kortetmäki and Oksanen, 2016). An SAI high carbon dioxide low sunlight world would also require additional adaptation on the part of agricultural actors. This does not look likely given agricultural adaptation to climate change has so far only been modest (Proctor et al., 2018). Large-scale changes in yield and precipitation are likely to create at least short-term food insecurity. There is evidence that existing population density and economic growth are closely tied to the existing climate niche. The narrow climatic envelope of $\sim 13^{\circ}\text{C}$ has provided beneficial environmental conditions within which most humans and societies have tended to historically cluster (Burke et al.,

2015; Xu et al., 2020). Our agricultural systems almost certainly are similarly tied to this niche, and any sudden change at a global level is likely to affect short-term yields and prices.

Politics

SAI could feasibly spark conflict and instability. There are already some emerging empirical links between food price shocks and socio-political violence. Moreover, the very act of undertaking SAI could be grounds for dispute. States may look to develop their own SAI capabilities before others do, creating more extensive backup infrastructure to avoid dependencies on others, or even construct counter SAI capabilities (see Horton and Reynolds, 2016 for a review in this area; McKinnon, 2020). Existing political order may become undone by SAI (Keith, 2013; Corry, 2017). A novel and interesting example could be high historical emitters like the US using the Common but Differentiated Responsibilities and Respective Capabilities principle as an instrument to assert SAI control or leadership (“we are mostly responsible for climate change, therefore it is ‘just’ that we lead the response”). Manipulation of the climate could become a new frontier for political conflict or even warfare. Different cross-boundary impacts on different regions would create large sets of winners and losers, alongside questions of attribution (MacMartin et al., 2019) and compensation. Whether such disputes could snowball into conflict is beyond prediction. Nonetheless, it is reasonable to say that unless enacted as altruistic, cooperative endeavor over multiple decades, the project of SAI would load further pressure onto existing international tensions. But even in the most altruistic cooperative scenarios, there may still be sub-national tensions in and/or between “donor” and “recipient” populations.

There is also the possibility that politics would worsen SAI. SAI and politics is a two-way street. Political conflict can cascade to affect SAI deployment and its impacts. Previous studies have made a compelling case that the direct weaponisation of SAI is unlikely (Fleming, 2007; Olson, 2011; Horton and Reynolds, 2016; Lin, 2016; Halstead, 2018). High impact uncertainties, management difficulty, low precision, and preferable alternative weaponry make SAI an unappealing instrument in state arsenals. However,

this does not mean that SAI has limited military use. SAI may not have usefulness as a direct weapon, but can function as a support system or a threat. Indeed, early attempts at cloud seeding were used by the US military during the Vietnam War as a tactical weapon to extend the Monsoon Season and disrupt North Vietnamese supply lines (Operation Popeye).

Another avenue for political dynamics to worsen a SAI deployment, and that has received relatively little attention, is via cyberwarfare. In May 2021, a ransomware cyber-attack forced a US fuel pipeline out of service. A \$5 million ransom was paid to restore service (BBC, 2021). As a globally critical (and potentially highly politicized) piece of infrastructure, SAI would likely be a target for private or state actors. SAI deployment dependent on any software or advanced algorithmic system (Rolnick et al., 2019; Schroeder de Witt and Hornigold, 2019), which is likely given the high technological and informational demands of deployment (MacMartin et al., 2019), would be vulnerable to cyberattack.

Cyberattacks do not need to come from external forces. For instance, the notorious 2000 Maroochy Cyberattack was from a disgruntled ex-employee (Slay and Miller, 2008). SAI would likely depend on a large workforce and have numerous reasons for controversy.

These political dynamics would have decades to play out. A cooperative and benevolent deployment of SAI could crumble into chaos with a change in actor preferences (or vice versa). Politics and its broader conditions are likely to change substantially over coming decades. Interactions between future geopolitics, warming and emissions, and technology are all nigh impossible to predict or even foresee (Wells, 2001), but would be of critical importance to SAI and its governance. Relying on one set of optimal political assumptions would be greatly unwise.

Health

SAI could negatively impact human health by both changing disease vectors and range (and therefore pandemics, see section: *Pandemics*), and by undermining existing health

system infrastructure. The regional variations of SAI's impacts on temperature and other ecological factors would likely affect disease transmissions. SAI-induced reductions in monsoon rainfall may increase cholera risk (Carlson and Trisos, 2018), and temperature changes can affect transmission of vector borne diseases like malaria (Carlson and Trisos, 2018; Carlson et al., 2020). Yet such health impacts are chronically understudied: currently only 4 papers focus on the health impacts of SAI (Effiong and Neitzel, 2016; Carlson and Trisos, 2018; Eastham et al., 2018; Carlson et al., 2020). The lack of coverage is significant since these studies have critical limitations, namely an assumption of equatorial injection (see section: *The Means of Deployment*). The impacts of other forms of deployment are largely unknown. Similarly, there is little research on the health impacts of exposure to SAI aerosols (Effiong and Neitzel, 2016), and the few quantitative assessments of mortality related to air quality and changes in UV exposure carry significant uncertainty (Eastham et al., 2018).

Despite these limitations, the research to date does point toward potential dangers. Alterations of disease transmission are especially important because diseases may reach populations which have lapsed or little immunity or resistance (Carlson and Trisos, 2018), or may have relatively weak or vulnerable public health systems. These critical nodes in health and urban systems, which otherwise would be less exposed, may amplify health risks and impacts: an epidemic may be amplified to become a pandemic (section: *Pandemics*). The uncertainty of SAI's potential deployment configurations, associated impacts, and state of existing health systems means that early identification of different critical nodes would likely be difficult and insufficient. Overall, systemic effects between health and SAI currently seem modest and carry high uncertainty. However, they are not negligible.

Latent Risk and SAI

Latent risk refers to risks that are dormant, but could become manifest during times of heightened societal vulnerability. The most obvious example would be the additional risks that arise in the aftermath of a collapse (widespread, significant, and enduring loss of life, political organization and economic capital) or another global catastrophe, for

example violent conflict over food and water. Latent risks are particularly important as they can provide one tangible way in which recovery from global shocks could be undermined and spiral toward extinction risk (Cotton-Barratt et al., 2020). We have already dealt with these partly in sections: Directly Catastrophic Impacts: Ecological Blowback?—The Systemic Risks of Climate Engineering. In short, latent risk is perhaps the largest risk factor for SAI. SAI changes the nature of climate risk by making the “likely” outcomes less severe, but making “less likely” (or “fat-tail”) outcomes substantially more severe. The risk of termination shock thickens the tail. Large amounts of SAI loading could create a precarious condition in which any sufficiently large global shock is likely to be compounded by a tumultuous termination shock.

While there is subjectivity as what a “threshold” for termination shock would be, Parker and Irvine (2018) put it at termination that causes at least 0.3 degrees warming per decade. This would imply an SAI cooling threshold of around 0.6 degrees. Kosugi (2013) puts the threshold at 0.2 degrees, implying a cooling threshold of around 0.4 degrees.

The speed of termination shock depends on the form of SRM. SAI has a half-life of ~8 months (approximately half the levels of coolants would still be present after 8 months) and warming would still take several years to reach its unmitigated levels (Parker and Irvine, 2018). Depending on the amount of warming masked, SAI has a distinctly high latent risk due to termination shock. A temperature rise of 6 degrees in the space of centuries would be an order of magnitude faster than the warming experienced during the Great Permian Dying (Lynas, 2020). If experienced in a period of decades, it would be an order of magnitude faster still. Current warming rates are geologically unprecedented; this speed would be chillingly rapid.

Critics have framed termination shock as an overblown problem for numerous reasons. These include that countries are unlikely to willingly reverse SAI, that there would be a sufficient buffer period to resume SAI, and it is unlikely to be hiding a large amount of warming (Reynolds et al., 2016; Parker and Irvine, 2018). These all seem to align with the inclination for both modeling and analysis of geoengineering to focus on the “best-

case.” That there would be sufficient cooperative governance and deployment of SAI, that there would be rational responses to any system lapse or shock, and that SAI would be used to only shave-off a small amount of warming (Ott, 2018). Yet, SAI is widely portrayed as an emergency response: it is most likely to be used in a worst-case high warming scenario, not a best-case limited warming one. Moreover, the likelihood of high-end warming, governance fragmentation, or another GCR occurring are all disarmingly large.

The likelihood of a catastrophe curtailing SAI efforts and causing termination shock is usually dismissed as very low. This is likely mistaken. We have covered some of these catastrophes in section: *Interactions With Other Global Catastrophic Hazards*. While there is considerable uncertainty, the likelihood of a GCR in the coming centuries does not appear to be vanishing. Estimates for a large-scale space weather event over the next decade or so range from 0.46% (Moriña et al., 2019) to 20.3% (Riley and Love, 2017). Estimates of the probability of nuclear war are few and vary, but one model of inadvertent conflict between the US and Russia using historical data put it at 0.9% per year (Barrett et al., 2013). SAI could also be slowly scaled-back as mitigation and CDR efforts increase (Reynolds et al., 2016). But this would likely require multiple additional decades which would (assuming no mitigation of other global threats) incur a higher likelihood of another catastrophe striking.

A more compelling retort is that SAI could be reintroduced within years at a reasonable cost. Some have suggested that given that SAI could be run at >1% of the GDP of the G20 and hence even losses of 75% of GDP (an unprecedented economic disaster) would be insufficient to keep an SAI system deactivated (Parker and Irvine, 2018). Such analysis overstates the coherence and rationality of states responding to crisis. The value of an extra 3 billion doses of COVID-19 vaccines would provide benefits of \$17.4 trillion, at a cost of around \$18–120 billion (Castillo et al., 2021). Yet vaccine production remains chronically low. Even in far less dire circumstances we can clearly not trust decision makers to take the optimal course of action.

SAI can be seen as one vast project to make the climate system more tightly coupled and synchronized with the global economic system. From a resilience perspective, such efforts are a liability. It makes it far more likely that the failure of one system will spill-over into another, sparking non-linear feedback loops that result in “synchronous failures” (Homer-Dixon et al., 2015). There are of course ways to make such complex engineering systems more resilient and robust, namely via backups and redundancies. However, current economic incentives for efficiency (particularly via cost reduction), mean that strong redundancies are rarely in place. SAI redundancies specifically are likely to be expensive and thus inconsistently implemented (Halstead, 2018; Parker and Irvine, 2018; McKinnon, 2020). In any case, it is unclear what redundancies would be effective at making an SAI system catastrophe-proof. Making SAI resilient to natural disasters or terrorist attacks seem relatively straight-forward (Parker and Irvine, 2018; Rabitz, 2019), but the same cannot be said of a true global catastrophe.

The inherent unknowns of highly complex technological systems also contribute to the possibility of termination shock. Highly complex systems, like SAI would be, are prone to “Normal Accidents” (Perrow, 1999; Maas, 2019). Large-scale accidents and disruptions are to be expected in sufficiently complex and tightly coupled systems. Unforeseen technological failures are simply a fact of life.

While latent risk is a genuine concern, it is a danger for only the greatest threats on the horizon and the “thickest” SAI deployments. A true, dramatic, global calamity would be needed to both disable an SAI system masking a large amount of warming and keep countries either preoccupied or incapable of reinstating it for several years.

In Table 2 we summarize our analysis of direct impacts, GCH interactions, systemic risks, and latent risk.

Contribution to catastrophic risk	Type of contribution	Nature of evidence base and uncertainty	Dependency on mode of deployment
Destabilising ecological systems	Ecological Blowback	Limited evidence base and high uncertainty. Lack of study on worst-case ecological impacts. High regional variations and uncertainty.	High dependency. Direct SAI impacts vary with thickness and other injection variations.
Volcanic eruption leading to political SAI difficulties	GCH Interaction	Limited evidence base. Study of SAI interactions with a volcanic eruption is limited.	Medium dependency. Dependent on potential SAI supply routes, but also external political dynamics.
Extreme space weather event damaging SAI and global electronic and power infrastructure.	GCH Interaction	Limited evidence base. No specific study of the impact of an extreme space weather event on SAI. Varying estimations of space weather probability.	Medium dependency. External SAI support systems are vulnerable. Thick SAI leads to more severe termination shock.
SAI-nuclear winter overcooling or nuclear frost-termination furnace.	GCH Interaction	Limited evidence base. Existing study on nuclear winter effects and SAI effects, but nothing that studies interactions between both.	Medium dependency. Dependent on external political dynamics. Thick SAI leads to more severe termination shock.
Political instability of a post-nuclear world on SAI redeployment	GCH Interaction	Limited evidence base. Existing study on post-nuclear politics and SAI politics, but nothing that	Low dependency. Dependent on external political dynamics.

		studies interactions between both.	
Pandemic leading to population or economic losses that make continued SAI infeasible.	GCH Interaction	Limited evidence base. Limited study of SAI-health intersections and no study of SAI-pandemic interactions.	Medium dependency. External pandemic, economic, and political factors are critical drivers. But thick SAI leads to more severe termination shock.
SAI weakening agricultural systems.	Systemic Risk	Limited evidence base and high uncertainty. Little study of SAI's agricultural impacts and high regional variance is likely.	High dependency. SAI's agricultural impacts are highly dependent on deployment configuration.
SAI sparking political conflict	Systemic Risk	Initial study of the political dimensions of SAI, but high uncertainty of how these political effects would play out.	Low dependency. Dependent on external political dynamics.
Political dynamics that compromise SAI safety	Systemic Risk	Low uncertainty that international geopolitics over a multi-decadal timescale is not ideal for optimum SAI governance. High uncertainty and limited evidence base as to how specifically this would play out.	Medium dependency. Political instability and conflict is core to the multilateral system, but nature of uneven SAI impacts and differing objectives contribute to political instability.

SAI affecting disease transmissions	Systemic Risk	Limited evidence base and high uncertainty. Limited study of SAI-health intersections and highly dependent on external urban and health policy dynamics.	Medium dependency. Thicker SAI more likely to affect disease dynamics. But external pandemic, economic, and political factors are primary drivers.
Termination shock	Latent Risk	Limited evidence base and high regional uncertainty of precise termination shock impacts. But low uncertainty that termination shock would be catastrophic.	High dependency. Thick SAI leads to more severe termination shock.

Table 2 Summary of SAI's direct impacts, GCH interactions, systemic risks, and latent risk.

Discussion: Building the Policy Boundaries for Climate Engineering

The Means of Deployment

Our analysis thus far has assumed a “default” deployment of optimal conditions of a global material approach to mitigate climate change. This is not necessarily the most likely scenario and the means of deployment and context will dramatically impact SAI's catastrophic risk profile. One of the critical variables to consider is the overall objective of a SAI deployment.

There are multiple potential objectives of SAI deployment, ranging from temperature reduction (of different extents), precipitation impact management, to biodiversity conservation (Lee et al., 2020; Zarnetske et al., 2021). These objectives will also depend on existing emissions reduction policies. There are also multiple potential “design”

options for deployment configuration (Kravitz et al., 2016), ranging from deployment timing, extent, placement, to aerosol selection (Pope et al., 2012; Keith et al., 2016; MacMartin et al., 2017, 2019; Kravitz et al., 2019; Vioni et al., 2020a). The extent of cooling for example not only depends on how much aerosol is released, but the height of injection in atmosphere (lower stratosphere injection produces more cooling) (Bala, 2009; Krishnamohan et al., 2020). Much of the existing study on SAI assumes injection along the equator (MacMartin et al., 2017). Equatorial injection is the most efficient if the only deployment objective is to maximize Earth's overall cooling. However, this would lead to high variance in temperature distributions, namely overcooling of the tropics and undercooling of poles (MacMartin et al., 2017). Impacts discussed in section: *Directly Catastrophic Impacts: Ecological Blowback?* also can change with a non-equatorial injection – Arctic SAI for instance would have less of an effect on Monsoon precipitation (Sun et al., 2020). Across all these there are key caveats. Neatly framed and optimized objectives found in modeling will not necessarily be reflected in messy and contested real life preferences, nor will SAI necessarily perfectly result in desired “design” outcomes (Wiertz, 2016; McLaren, 2018).

It is also important to consider that SAI may not be used solely to respond to climate change. The multiplicity of potential SAI goals opens the door to hidden agendas (McConnell, 2018), self-interest, and misuse. In addition, even if SAI is deployed under an idealistic scenario of climate altruism, there is no guarantee that this would persist. Considering that political preferences are unlikely to remain static over decadal timescales (see section: *Politics*), SAI functions may slowly “creep” (Koops, 2021) into currently unknown possibilities of misuse. Such ‘Function Creep’ and potential misuse are highly understudied in current SAI literature¹².

¹² There is the ethical question of how research in SAI misuse should be undertaken. Will researching SAI misuse possibilities inadvertently encourage SAI misuse? This is a question beyond the scope of this paper but is regardless a critical issue that deserves future attention.

Predicting or even foreseeing potential future SAI functions will forever be mired in uncertainty. This is part of why Function Creep is such a difficult policy problem. Initial study in this area for SAI highlights the potential to use SAI to “optimize” or create “designer climates” (Preston, 2013; Talberg et al., 2018). Actors may for example advocate for deployment configurations that lead to more favorable conditions for critical staple crops, especially in response to warming impacts. These decisions may be the product of misjudgement or misinformation on SAI's causal nature. SAI may also be used to justify the continued existence of fossil fuel industries. This is a potential adverse incentive core to the “moral hazard” problem (Reynolds, 2015; Lockley and Coffman, 2016). This could even create a new atmospheric political economy. The fossil fuel industry and other vested interests benefiting from the SAI system would have incentives to both use it as a way to slow decarbonisation and perhaps even thicken SAI deployment over time. This would heighten latent risk. Assuming SAI as a benign climate change response is unwisely narrow. Other more sinister SAI uses, whether purposeful or inadvertent, are critical determinants of SAI's desirability.

There are many more SAI deployment options that are not currently well-captured in extant governance literature. SAI risks for example take a drastically different form if artificial intelligence (AI) is one of the central aspects of deployment design. With the vast amounts of information feedback and constant operational adjustments required (MacMartin et al., 2019), an advanced deep reinforcement learning system may be used to manage SAI deployment (Rolnick et al., 2019; Schroeder de Witt and Hornigold, 2019). This would introduce a raft of new issues, for instance “black box” opacity of decision processes (Rudin and Radin, 2019) or inappropriate generalizations of incomplete data (Martin et al., 2012; Christie et al., 2020).

Given the high variance of potential SAI objectives and potential deployment configurations, a highly political, uncoordinated, and decentralized (Reynolds and Wagner, 2020) “Wild West” deployment scenario, with unclear direct impacts, is possible. States and private sector actors are not likely to find agreement on a single defined “set” of objectives, how they should be prioritized, and how these objectives

should manifest in deployment configuration. These intensely political and self-interest driven considerations are likely key determinants of SAI deployment impacts, and should be priority areas for future governance research.

The means of deployment for other GCHs also affect SAI risks. An intentional weaponised pandemic may intentionally leverage SAI dynamics, like changes in disease transmission via changes in temperature distribution, to target critical nodes in health and urban systems. Such potential for now is speculative, but ultimately plausible.

Interconnections

Our analysis has focused on individual pathways for SAI to contribute to GCR. However, none of these are mutually exclusive. Each of the four streams overlap and feed into the same waterway. For instance, uni- or minilateral deployment of SRM systems could be driven by geopolitical distrust and conflict. This would likely be a world in which other GCHs are more likely, SAI deployment is less coordinated and damaging, critical systems are less resilient, and the world is less likely to quickly and effectively deal with a termination shock.

There is also an important intersection between systemic and latent risk. Most mechanisms that increase systemic risk will tend to raise latent risk as well. For instance, just in time delivery systems and tightly coupled systems with few back-ups, while efficient, are both more susceptible to shocks and can impede recovery. In Figure 1 we provide one brief attempt to map some of the linkages between different risks and factors in SAI deployment. More interconnected systems mean a higher chance of synchronous failures, and SAI is likely to be a highly interconnected system.

Building the Policy Boundaries

Analysis of catastrophic downside risks can help illuminate the contours of what “effective” SAI governance would do. This is a useful complement to the policy literature

that has focused mostly on structure and architecture (Reynolds, 2019). We add to the knowledge on policy instruments by providing further detail on policy approach.

To effectively mitigate against the (limited) number of threats and systemic risks outlined in this paper, SAI governance would have to be wide ranging, robust, and persist over decades. SAI and its backup infrastructure would need to be built to be resilient to extreme space weather or nuclear EMP events. Effective SAI governance is also not limited to SAI itself, but encompasses other policy areas like health, agriculture, AI, and energy. Ensuring ambitious emissions reductions and greenhouse gas removals would be needed to ensure SAI did not continue indefinitely. Effective SAI governance would also prevent future misuse and balance shifting preferences and multiple deployment goals in a just and inclusive manner. Governance arrangements to ensure SAI deployment or reimplementation in the wake of a major shock like a recession, pandemic, or nuclear attack would also be necessary. These would all be in addition to the herculean technical informational demands necessary for SAI deployment, which alone may be a larger undertaking than an IPCC report (MacMartin et al., 2019). This optimistically assumes that SAI's climatic outputs can be clearly and cleanly measured, that there would be widespread international capacity for effective monitoring (McLaren, 2018), and that this monitoring would also be resilient to critical shocks. Substantial advancements in climate science and observation, as well as additional international capacity building for monitoring and transparency, would be needed. All of these would then have to be maintained over the course of decades.

These altogether represent an incredibly challenging governance task. The lack of success of the climate regime, with its similarly intense political and wide-ranging nature, does not inspire confidence in the feasibility of wide-ranging and long-term governance for an issue as political as SAI. Basic discussion over climate engineering as a whole has been stymied under the UN Environment Assembly (McLaren and Corry, 2021). Future and more consequential SAI debate will be subject to more severe political hurdles. Even less complex and smaller scale governance arrangements, like COVID-19 mask mandates, have been mired in politicization and competitive dynamics. A “mask” over the Earth,

and its associated governance considerations, would face even tougher political challenges to effective implementation.

What would happen if a SAI deployment went ahead without these governance safeguards? It could very well be the case that agricultural or health impacts of SAI are limited or even positive. A disaster like an extreme space weather event may not happen in the decades where SAI is implemented. The stars of international politics could align and allow for a smooth SAI implementation and cessation.

There is indeed no guarantee that the catastrophic pathways outlined in this paper will materialize. But if they do, they would likely result in severe and cascading consequences. SAI has many extreme downside risks. “Imperfect” SAI governance can be compared to living without health insurance. The extra safeguards and protection aren't strictly necessary...until something goes wrong. Given what we know about the instability of international geopolitics, SAI with imperfect SAI governance puts the world in a precarious position and introduces a climatic Sword of Damocles. The ultimate question becomes: are we willing to bet the climate that no catastrophe or systemic cascade will trigger SAI's downside potential over the coming decades?

In a world of imperfect safeguards, two interconnected options are available to alleviate catastrophic risks. The first option is thinner SAI deployment. Thin SAI has a lower risk of catastrophic termination shock, thus posing less of a threat even if triggered by another calamity or systemic cascade. The second option is to ensure diversity in the overall climate engineering portfolio. Reducing reliance on SAI would better allow for a thinner SAI deployment. Other climate engineering approaches, particularly those which are less technology based, would also not necessarily share the same vulnerabilities as SAI. Trees for instance are not vulnerable to extreme space weather. These would reduce the potential of an SAI termination, but ultimately would not completely remedy the political complications SAI would create.

There seem to be three major¹³ pathways moving forward. The first is living in a highly vulnerable scenario of imperfect SAI governance—the “Damocles Pathway.” This is clearly undesirable. The second is living with well-governed SAI that will not exceed policy boundaries of catastrophe—the “Miracle Pathway.” This seems infeasible. The final middle ground is to accept the inevitably imperfect contours of SAI governance, but greatly limit the extent of SAI deployment—the “Limited Pathway.” But this again would rely on robust and resilient governance and is still vulnerable to geopolitical shocks. SAI may be thickened or thinned along changing political tides.

A core conclusion here is that there is little use in asking whether SAI is a GCR or not. It depends on the level of loading and wider geopolitical landscape. All risks, especially latent risk, will increase with greater loadings and political conflict. This is a critical insight for the wider study of GCRs. A risk cannot be judged in a vacuum. Its severity will inevitably be determined by the scenario and system in which it unfolds.

Conclusion: The Frying Pan and the Flame

We map the different contributions of SAI to Global Catastrophic Risk (GCR). The direct risks through irreversible extreme ecosystem impacts are currently unknown. No mechanisms for this have been identified. But extreme ecosystem impacts cannot be confidently ruled out given the nature of the Earth systems. SAI could have numerous diffuse impacts on critical systems such as agriculture, politics and health. These currently appear modest, but we cannot rule out the possibilities of systemic cascades or synchronous failures. It appears unlikely that SAI would trigger any other calamitous hazards unless it ignites geopolitical conflict between great powers. Instead, SAI's greatest contribution is through latent risk: the ability for termination shock to significantly worsen any other GCR. For each of these areas the evidence base is significantly underdeveloped.

¹³ There are likely going to be many ‘in-betweens’, but these seem like the major contours of SAI’s future.

Is SAI worse than the initial problem of climate change? The question for now is largely unanswerable and lies outside the scope of our analysis. This paper represents a first step in understanding the multitude of risks of SAI. But critical gaps in understanding of both high-end warming and SAI remain. The climate comparison also depends on specific details, such as level of warming, state of politics, and availability of alternatives to SAI (such as rapid large-scale carbon dioxide removal). SAI is also deeply dependent on governance and the level of use. A constrained use of SAI with coherent, coordinated governance would most likely be benign and beneficial. Yet it is in a scenario of extreme warming, political fragmentation, and a search for an escape clause that SAI use appears most likely. Such thick and uncoordinated use of SAI is unwise and an unwise risk management strategy. We would face a planetary Sword of Damocles.

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Foreword to Chapter 5: Removal Down Under

5.1. Place in thesis

With the extreme risks of stratospheric aerosol injection, we may simply try to make GGR work as best as possible. We can investigate the policy frameworks for timely GGR policy. When discussing policy, political windows should be an integrated aspect of discussion^{1,2}. An analysis of international policy relies on the country preferences and political windows of specific countries (the structure of the Paris Agreement was for instance intentionally designed to allow the US to join via Presidential Executive Order³). However, country preferences are not yet developed, making an integrated political-policy analysis difficult.

This chapter therefore investigates GGR policy at a country level, specifically Australia. Australia's land, marine, and mineral resources give it strong comparative advantages in GGR⁴, and is thus a prime candidate to investigate ambitious GGR policy. Australia's political dynamics relating to GGR, while nascent, are also more clear (especially from the vantage point of a Canberra based academic). By interrogating potential policy settings for incentivising GGR, with potential lessons for other countries, this chapter helps answer Research Question 1b.

5.2. Key Conclusions

Australian GGR policy is on the political periphery. However, this may change in the near future, since influential organisations like the Climate Change Authority and Australian Academy of Science are publicly emphasising the importance of GGR⁴⁻⁶.

The current climate policy regime in Australia is poorly suited to GGR. The 2022 Climate Change Bill legislates a 2050 net zero target, but does not provide explicit substantive detail of intended GGR strategy. The Emissions Reduction Fund, Australia's carbon offset mechanism, can provide large near term GGR funding. However, it has been subject to recent controversies regarding its offset integrity and additionality⁷.

Finally, there is a patchwork of different GGR-related issue areas with their own tangles of policy, like Carbon Capture and Storage⁸.

This chapter puts forward four key priorities to ensure timely GGR implementation (within the considerations of existing politics and policy): create a government ‘home’ for GGR to ensure systemic GGR coordination; leverage cooperative and competitive federalism to coordinate policy between States and Territories; ensure robustness and trust in funding and regulatory mechanisms for land-based sequestration, such as the Emissions Reduction Fund, to avoid unproductive polarisation and distrust of GGR; and spark GGR innovation by attracting international partners and developing domestic GGR innovation capabilities.

However, if fully implemented, a big political and policy ask, this would not necessarily spark the necessary speed for GGR. It relies on Australia putting forward a separate removal target as part of its Nationally Determined Contribution in 2025, whereas massive GGR investment is needed now (and ideally, yesterday).

5.3. Significance to the thesis

High and wide-ranging ambition within the constraints of existing political considerations may not provide sufficient GGR acceleration. Political and policy systems simply lack the necessary urgency to act on GGR. This chapter analyses Australia, a country with perhaps the world’s strongest comparative GGR advantages. The policy challenges will be far harder for other countries. Looking beyond conventional political constraints may provide a pathway for sufficient acceleration.

This chapter’s recommendations also provide guidance on how to apply the holistic risk assessment of Chapter 4 into practice. Namely, undertaking systemic and holistic analysis, as well as formulating and coordinating implementation of systemic policy responses, is best served by a coordinating function that can see the big picture and is directly mandated to identify interlinks across complex topics. This is also reflected in other countries’ administrative approaches to national risk assessment. The UK’s National Risk Register is managed by the Cabinet Office and New Zealand’s National

Risk Register is managed by the Department of Prime Minister and Cabinet, both of which are tasked with big picture coordination across the whole-of-government.

This chapter is under review at *Climate Policy*.

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Chapter 5: Removal Down Under: Australian Greenhouse Gas Removal

Policy

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Abstract

The world must develop greenhouse gas removal (GGR) capabilities to limit global warming to under 1.5 or 2°C. Most GGR approaches are at an early stage of development and most policy analysis has focused on Europe and the US while overlooking Australia. This is a missed opportunity: Australia has the potential to be a leader and leverage point in accelerating GGR innovation and deployment. Australia has substantial land, mineral and marine resources, as well as world leading renewable energy potential. But, like many other countries, Australia has no systemic policy for GGR acceleration, nor is GGR a political priority. This paper is a first step in addressing the Australian GGR policy gap. It identifies four priorities for Australian GGR policy: create a government ‘home’ for GGR to ensure systemic GGR coordination; leverage cooperative and competitive federalism to coordinate policy between States and Territories; ensure robustness and trust in funding and regulatory mechanisms for vegetation-based sequestration, such as the Australian Carbon Credit Unit Scheme, to avoid unproductive polarisation and distrust of GGR; and spark GGR innovation by attracting international partners and developing domestic GGR innovation capabilities. These leverage points for acceleration should be priority areas of focus for Australian GGR research and policy, and can inform GGR policy development in other jurisdictions too. However, GGR is not a panacea. These policies must accompany stronger efforts to cut emissions rapidly, deeply and consistently.

Key policy insights

- This paper advances four pillars for effective GGR policy in Australia: create a coordinating hub for GGR in federal government, leverage cooperative and competitive federalism among states and territories, ensure robustness and trust in existing land-based sequestration, and spark GGR innovation by attracting international field testing and reforming domestic innovation capabilities.
- At the time of writing, GGR is on the Australian political periphery, but this may change if the Climate Change Authority recommends a separate emissions removal target for Australia’s 2025 Nationally Determined Contribution.

- GGR calls for a holistic and systemic policy approach, focusing on key leverage points and feedback loops rather than individual technologies or policy instruments.

Key words

Greenhouse gas removal; policy; coordination; federalism; offsets; innovation

1. Introduction

Greenhouse gas removal (GGR) is necessary to limit or stabilise warming at 1.5°C, and highly likely to play a role in meeting the 2°C target. GGR is the active capture and storage of greenhouse gases from the atmosphere. GGR differs from carbon capture and storage (CCS), which is the sequestration of carbon emitted at point-source, rather than from the atmosphere. There are multiple GGR methods, such as direct air capture (DAC), enhanced weathering (EW) and vegetation-based methods (Figure 1). These methods and implementation options vary in capture mechanism, storage mechanism, and durability of carbon storage and measurability. No single method can remove enough to meet temperature targets. A portfolio of different methods is required to achieve robust and sufficient GGR.

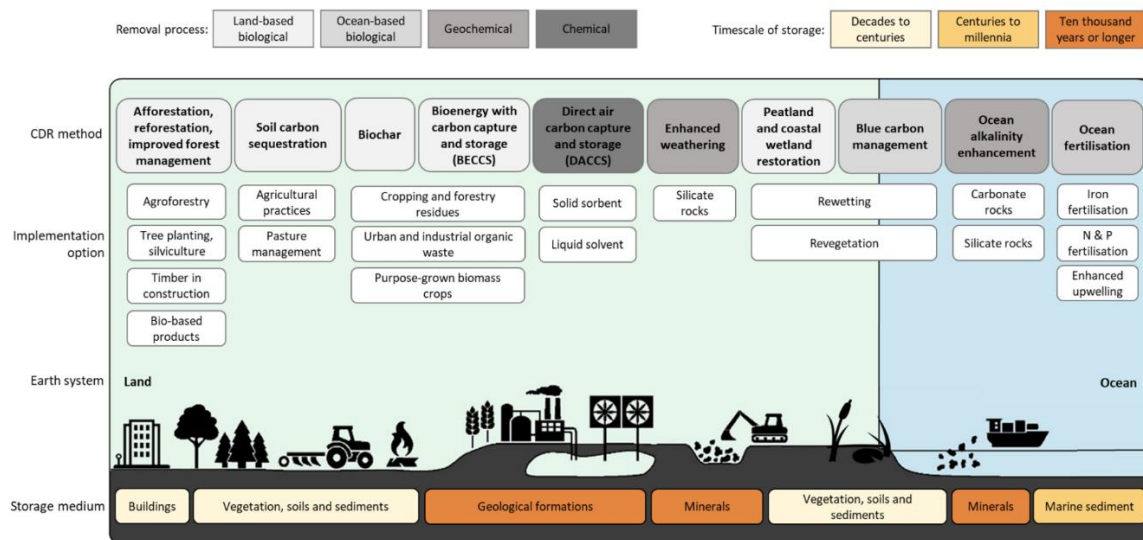


Figure 1 GGR methods from IPCC AR6 WGIII, Ch. 12, Box 8, Fig. 1. Lists of implementation options for each method are limited.

Despite the diversity of methods, global GGR capabilities are limited. Policy that enhances GGR capabilities is critical. Especially important is to accelerate GGR development in countries which are strongly suited to GGR. Countries with GGR advantages must provide global leadership.

Australia is one such country. Despite strong comparative advantages in land, mineral, and marine resources (Section 2), and despite existing policy in related areas like land-based sequestration or underground storage (Section 3), Australia has no overarching systemic GGR policy. An overarching systemic policy approach is important because GGR development is about creating an entire GGR ecosystem, encompassing everything from net-negative supply chains, sufficient infrastructure and renewable energy, to a GGR workforce.

Most Australian GGR engagement thus far has been purely academic, with less engagement from political and policy-making circles (Boyd *et al.*, 2018; Colvin *et al.*, 2020; Cook and Martínez Arranz, 2020; Schenuit *et al.*, 2021; McDonald *et al.*, 2023).

Following the release of the IPCC's 6th Assessment Report, in 2023, with the Australian Academy of Science (the Academy), Climate Change Authority (the CCA), and Commonwealth Scientific and Industrial Research Organisation (CSIRO) all releasing analyses of Australian GGR. The Academy emphasised the importance of developing novel GGR approaches (Australian Academy of Science, 2023a), the CCA identified broad policy insights (Climate Change Authority, 2023), and CSIRO (commissioned by the CCA) assessed Australia's sequestration potential (CSIRO, 2023) and the potential contribution of individual technologies. While Government is not bound to act on their recommendations, these organisations are influential: CSIRO is Australia's main national scientific body which carries high legitimacy, and the CCA (as mandated by Australian legislation) and Academy (as a representative body for Australian science) directly advise the Australian government.

With the authoritative advice of these influential organisations, GGR policy may to continue to gain prominence in Australia, particularly if GGR is included in Australia's next Nationally Determined Contribution (NDC).

However, there has not yet been an analysis of the systemic policies necessary to ensure timely and responsible implementation of Australian GGR. Most policy progress thus far can be found in the CCA's 2023 report, which identified 23 policy priorities (called

insights) (Climate Change Authority, 2023). The next step is to identify key linkages, interactions, and leverage points. This is important because policy that accelerates broad scale innovation, as needed for GGR, is not about the construction of discrete and disconnected policy instruments, but rather how systemic policy *packages* spark feedback loops in society and the economy (Roberts *et al.*, 2018; Victor *et al.*, 2019). The UK's efforts to increase energy efficiency for instance involved the concurrent use of measures that increased supply (such as the Low Impact Buildings Innovation Platform) and demand (such as energy efficiency loans for small and medium sized enterprises) for energy efficiency infrastructure (Kern, Kivimaa and Martiskainen, 2017). Building a timely, responsible and effective GGR regime is therefore not about single policy instruments, but about packages of policies that act as leverage points to enable outsized and lasting change,

We explore the leverage points for the development of a responsible, timely, and effective Australian GGR policy regime. This paper first outlines Australia's natural advantages in GGR, then describes the existing policy environment in which GGR policy would be positioned. Finally, we propose a four-pillared strategy for effective Australian GGR policy: create a coordinating hub for GGR in federal government to ensure sufficient interdepartmental engagement, leverage cooperative and competitive federalism among States and Territories to ensure multi-level consistency and alignment, ensure robustness and trust in existing sequestration to best ensure social licence for GGR, and spark GGR innovation by attracting international field testing and reforming domestic innovation capabilities to accelerate GGR research and development. While we focus on Australia, this paper provides insight to assessing and crafting GGR policy in other countries.

2. The Australian Advantage

Australia has several comparative advantages in GGR, identified by experts convened by the Academy (Australian Academy of Science, 2023a):

- Abundant land and a long coastline, presenting opportunities for supporting infrastructures like renewable energy and underground storage, and to field-test GGR approaches.
- Abundant mineral resources, alongside established minerals infrastructure, workforce, and expertise are advantages for enhanced weathering approaches and underground storage.
- Strong rule of law with clear property rights, allowing for financial and market-based incentives (especially for land-based GGR).
- Good relations with GGR first-movers, such as the US and Europe.

These comparative advantages can position Australia as an international leader in GGR. Other countries do not have the same combination of space, storage and energy that would allow them to independently meet their own GGR needs. For example, Switzerland, a first mover in GGR innovation and policy, has a removal target of 7Mt per year by 2050. Of this, 5 Mt will be met through internationally supplied removals. Switzerland, and many other countries, will rely on an international supply of GGR. With its space, storage, resources, renewable energy, and policy capabilities, Australia can play a leading role in meeting the world's GGR needs.

However, Australia's storage comes with important caveats. The additionality and storage integrity of Australia's current vegetation-based removal is contested and controversial (Macintosh *et al.*, 2022 and see Section 3.2). As a policy area closely related to GGR (Schenuit *et al.*, 2021), these currently unresolved controversies have the potential to damage social licence for GGR by undermining trust and governance. Measurement and sustainability of marine removals remains uncertain (Jiao *et al.*, 2018), which may raise further integrity challenges. Further, underground carbon storage, and possible concerns of carbon leakage, may intersect with First Nations' land, raising valid social licence and Indigenous Land Use Agreement concerns. Australia's renewable energy potential is capped by deficiencies in Australia's grid system (Yu *et al.*, 2022), limiting high on-grid energy demand approaches like some DAC technologies. While Australia's research contributions are strong, support for science and innovation is mired with funding cuts and administrative bloat (Howard, 2020; Australian Academy of Science, 2022). Finally, a lack of domestic manufacturing capabilities (Dean *et al.*, 2021)

makes creating efficient carbon neutral supply chains difficult. Ultimately, GGR is a nascent industry; these barriers may limit the development of some approaches, but it is difficult to predict which. Australia's natural advantages in a wide range of GGR technologies greatly increases the likelihood that it will support the growth of the most successful technologies.

While Australia may have several comparative GGR advantages, substantial efforts are required to capitalise on Australia's unique strengths.

3. The Australian Policy Context

Despite its potential, Australia has no explicit policy framework to accelerate and implement GGR at scale. Instead, it has big-picture emissions targets and with diffuse incentives for land-based sequestration for offsetting purposes. Central to the extant GGR-relevant policy regime are the 2022 Climate Change Bill, the Australian Carbon Credit Unit (ACCU) Scheme, GGR-intersecting policy areas such as the forthcoming Nature Repair Scheme and GGR-relevant policy areas such as underground storage.

3.1. The Climate Change Act 2022

The cornerstone of Australian climate policy is the *Climate Change Act 2022* (the Act). The Act legislated an emissions reduction target of 43% reduction from 2005 levels by 2030 with updates every 5 years (Commonwealth of Australia, 2022). This incorporates existing Paris Agreement commitments into Australian law and links future targets to CCA advice, rather than the preferences of the incumbent political party. Beyond short-term climate targets, the Act also enshrines a 2050 net zero target. The Act provides important stability following over a decade of failure to maintain effective federal climate policy (Crowley, 2021a).

While a 'net' zero target implicitly requires removals, the Act offers no removal strategy. Indeed, an audit by the Australian National Audit Organisation (ANAO) has since found that "*key components including national plans, strategies, and frameworks are yet to be*

delivered for use” for meeting emissions reduction commitments (ANAO, 2024). This is concerning given initial modelling for reaching net zero by 2050 indicates that removal rates may have to reach over 100 Megatons per year by 2040, with one scenario requiring over 500 Megatons of removal per year by 2040 (Net Zero Australia, 2023),

Implementation of the Climate Change Act may later result in a clearer GGR approach, but this is at the moment a speculative option. Short-term amendments to more explicitly integrate GGR policy into the Climate Change Act itself are likely infeasible – undermining the legislative stability that has taken a decade to reach is not likely a politically appealing option.

GGR opportunities lie instead with the CCA’s recommendations for future targets. The CCA’s next recommendation, due in 2025, may include separate reduction and removal targets. This could elevate GGR and the need for a coherent policy regime from the political periphery, as refusing the Authority’s advice requires a public explanation and justification, and may therefore be politically challenging.

3.2. The Australian Carbon Credit Unit (ACCU) Scheme and the Safeguard

Mechanism

Other major pillars of Australian climate policy are the Australian Carbon Credit Unit (ACCU) Scheme (formerly known as the Emissions Reduction Fund (ERF)) and the Safeguard Mechanism. The ACCU Scheme allows approved mitigation activities (thus far primarily via land management (Clean Energy Regulator, 2023)) to earn Atradeable carbon credits (ACCUs). At February 2024 there were 33 active methods, 11 of which included sequestration activities (Clean Energy Regulator, 2024). There are a raft of grant mechanisms encouraging ACCU Scheme project development, for example New South Wales’ AU\$10 million High Impact Partnerships program (NSW Government, 2023), the federal government’s AU\$17.5 million Carbon Farming Outreach Program (Australian Government, 2023a), and Queensland’s AU\$500 million Land Restoration Fund (Queensland Government, 2023).

The Federal government is the primary purchaser of ACCUs, but private ACCU demand is growing (Carbon Market Institute, 2023). The Safeguard Mechanism establishes emissions limits on industrial facilities that emit over 100,000 tonnes of carbon dioxide equivalent per year. Recent reforms require emissions decreases of 4.9% annually until 2030. Controversially (Denniss, 2023), there is no limit on how many carbon credits facility operators can purchase and retire to meet Safeguard Mechanism obligations. Indeed, Australia's 2023 emissions projections foresee increased Safeguard facilities' demand of offsets, with more emissions abatement coming from offsets (mostly met with ACCUS) than on-site emissions reductions in the 2024-2035 period (DCCEEW, 2023a).

Like other offset mechanisms, questions have been raised about ACCU integrity. Controversies centred around allegations that many ACCU Scheme projects were non-additional had been over-credited, and that ACCU Scheme governance exhibited significant conflicts of interest (Macintosh *et al.*, 2022). These resulted in an independent review (the Chubb Review) released in January 2023. While recommending significant changes to governance arrangements, the Review found that "ACCU scheme arrangements are essentially sound" (Chubb *et al.*, 2023). This was based on, at the time, publicly unavailable project data and generated further controversy (Macintosh and Butler, 2023). The public release of these data has not quelled integrity concerns (Macintosh *et al.*, 2023). ACCU Scheme integrity remains a matter of political and academic debate, weakening the Scheme's social licence and the activities, current or proposed, that the Scheme supports. Further, the Chubb Review's recommendation of a 'proponent-led process' for ACCU Scheme method development, opposed to the existing centralised government-led approach, could allow many GGR proponents to more easily participate in the ACCU Scheme.

With a likely increase in ACCU demand due to increases in compliance obligations under the Safeguard Mechanism and voluntary corporate climate action, and a potentially more diverse suite of activities with forthcoming proponent-led methods, increasing carbon prices to incentivise novel higher-cost abatement and the obsolescence of existing

methods, the ACCU Scheme will likely continue as a major source of financial support for GGR activities.

Unfortunately, it can only provide funding support, not targeted policy or innovation assistance. The ACCU Scheme supports existing and already low-cost approaches, not high cost innovative GGR initiatives. Specific GGR innovation support, like building networks or facilitating public demonstrations, is needed (Australian Academy of Science, 2023a).

Funding based on market prices is also insufficient. Government ACCU purchases operate via *reverse* auction – there is a race to the bottom for the cheapest abatement, not for the most novel, sustainable, or reliable abatement. The current price of roughly AU\$35 per tonne of carbon and a proposed price cap of AU\$75 (for Safeguard Mechanism buyers, increasing by 2% each year and to be reviewed in 2026-2027) are unlikely to sufficiently enable early- or late-stage GGR. The US Carbon Negative Shot for instance aims for endgame removal at US\$100 per tonne (US Office of Fossil Energy and Carbon Management, 2022).

Lastly, there is no guarantee that ACCU Scheme sequestration will result in sufficient removal to meet net zero targets (nor has there been quantification of the potential growth of ACCU Scheme sequestration). Offsets are a high-risk policy instrument that often do not represent lasting and additional removals (Aldy and Stavins, 2012; West *et al.*, 2020; Badgley *et al.*, 2022). Bundling the ACCU Scheme, and its associated controversies, with GGR may also preclude political GGR acceptance (Colvin *et al.*, 2020). The ACCU Scheme alone is insufficient to meet Australia’s GGR needs. It is fundamentally an offsetting mechanism rather than a mechanism for sequestration above and beyond compensation for residual emissions, as is needed for national and global climate goals

3.3. Patchwork Issue Areas

In addition to big picture direction, there is an uncoordinated patchwork across many different GGR issue areas.

For an illustrative case, Dixon *et al.* (2015) highlight the patchwork of underground storage legislation in Australia. The patchwork ranges from the Federal 2005 Regulatory Guiding Principles for Carbon Dioxide Capture and Geological Storage and the 2006 Offshore Petroleum and Greenhouse Gas Storage Act, to a suite of different legislation at State and Territory levels (the 2008 Greenhouse Gas Geological Sequestration Act in Victoria; 2009 Greenhouse Gas Storage Act in Queensland; 2009 CCS amendments to the 2000 Geothermal Energy Act in South Australia; and the 2003 Barrow Island Act in Western Australia). Adding to this patchwork have been legislative amendments to allow for geological storage overseas (Australian Government, 2023b), which is further complicated by links to CCS and associated controversies of fossil fuel extension (Morton, 2021; Fitzgerald, 2023).

Aligning this constellation of legislation, policies and politics across the GGR portfolio (not just underground storage) and across the states and territories, will be necessary for effective acceleration. GGR will slow if innovators and project operators must navigate a maze of overlapping legislation, regulation and policy. Addressing this an enormous task, calling for a leverage point approach that identifies early priority areas (Abson *et al.*, 2017).

4. An Australian GGR Policy Strategy

This section highlights priority leverage points that could underpin an effective Australian GGR policy. Following GGR policy analysis for Europe (Geden *et al.*, 2019), it also briefly covers political feasibility. This is prudent given Australia's complex history of polarisation on climate change (Crowley, 2021b). Understanding effective GGR policy is not possible without considering enabling political factors (Green and Reyes, 2023). We discuss four pillars for effective GGR policy in Australia (Figure 2):

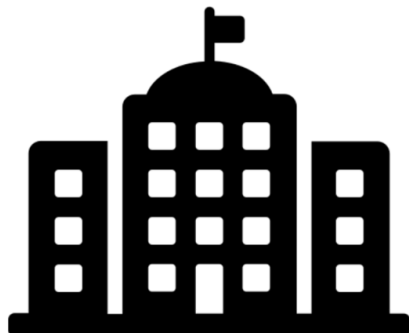
- Create an organisational home at a federal level for interdepartmental policy and strategic coordination;
- Coordinate a strategic balance between cooperative and competitive federalism between states and territories to avoid delay and fragmented GGR policy;

- Secure a high integrity offset market and land sequestration to ensure social licence, political trust and to avoid unproductive ideological bundling with GGR; and,
- Leverage Australia's comparative advantages to attract international GGR innovators, reform its domestic innovation system, and develop a GGR innovation strategy to spark novel GGR approaches.

These can be seen as critical enablers of an effective GGR regime. Horizontal and vertical coordination is necessary to ensure the efficient use of resources and to build a shared responsibility of GGR acceleration across governments, ensuring a high integrity offset market is needed to best ensure that Australian GGR does not fall into unproductive political controversies, and leveraging Australia's innovation system is necessary to accelerate the research and development of existing and new GGR approaches.

Leverage points for accelerating Australian GGR

**Create a government home
for systemic coordination**



**Leverage federalism between
states and territories**



**Ensure robustness and trust in
land sequestration mechanisms**



Attract and spark GGR innovation

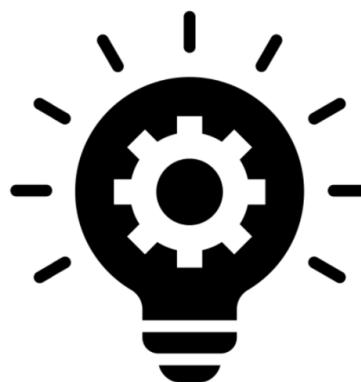


Figure 2 Leverage points for accelerating effective GGR in Australia.

4.1. An organisational home for GGR in federal government

A foundational step is to create an organisational ‘home’ for Australian GGR policy: a Commonwealth government function dedicated to whole-of-government and whole-of-nation GGR coordination and strategy.

There is currently no organisational GGR ‘ownership’ across the federal Departments of Prime Minister & Cabinet (PM&C); Climate Change, Energy, Environment and Water (DCCEEW); Industry, Science and Resources (DISR); and Foreign Affairs and Trade (DFAT). Informal engagement by the authors across these departments also confirms the

lack of internal GGR ownership. This comes despite each department having teams that cover similar issue areas, for instance, DCCEEW's Net Zero Innovation Section or PM&C's Net Zero Authority.

Without an organisational home for whole-of-government coordination, there cannot be a coordinated and systemic policy approach to accelerating and scaling a robust GGR portfolio, risking patchwork GGR initiatives, insufficient development of diversity, and limited systemic policy learning (Trein *et al.*, 2019). A shared cohesive vision of GGR innovation is needed to best align and accelerate GGR innovation (see Section 4.4). Alongside broad coordination, strategic specialisation development will be needed since different aspects of GGR require different expertise.

Such a home should have strong interdisciplinary capabilities. Australia's GGR advantages mean that essentially the *entire* potential GGR portfolio can see development and scale-up in Australia. This potential demands capabilities in areas including large scale stakeholder engagement, innovation policy, land care, resource management, coastal management, and emissions accounting.

Given the breadth of its mandate for whole-of-government and whole-of-nation coordination, this organisational home should not be responsible for direct GGR implementation (which should be tasked to regional and local governments). It should be a strategic hub for policy coordination. The overall Australian GGR portfolio will require some element of design to ensure it best takes advantage of Australia's natural advantages (Section 2). These coordination roles point to PM&C or DCCEEW as suitable homes. A model reflecting the emerging Net Zero Authority may be well suited to GGR, given its intended focus on coordination, engagement. And strategy rather than direct implementation (Colvin, 2023), an area for further research.

There may be lessons from international counterparts. The US has recently established the Office of Fossil and Carbon Management (OFCM). The OFCM administers the US's 'Carbon Negative Shot' initiative, managing and delivering DAC funding, and supporting

GGR research and innovation. However, there has yet to be analysis of the OFCM's effectiveness. Of particular interest is the bundling of carbon management and fossil energy, and whether this becomes a gateway for vested interests or erodes social trust. The organisational framing of a GGR function is important to consider – GGR framed in the context of industry building may garner different types and levels of support compared to GGR in the context of climate commitments.

Adopting a separate removal target in 2025 would signal GGR as a shorter-term and explicitly political and policy priority, with a corresponding GGR function potentially to follow in order to implement.

An important consideration is where GGR priority lies. The specific 'ownership' by a Minister can affect a GGR function's organisational placement. For instance, GGR being the remit of the Minister for Climate Change and Energy would point to DCCEE. GGR being the priority of the Minister for Industry and Science would more likely lead to GGR being placed in DISR. GGR as a personal priority for the Prime Minister would point to PM&C (as was the case for the Net Zero Economy Taskforce and its successor Net Zero Authority). This would have substantive policy implications. PM&C for instance prioritises whole of government coordination, whereas DISR carries a more technology and industry-focused perspective.

However, the mandate and funding of such a body is just as important as the organisational home. Departments are frequently shifted, renamed, merged, and abolished, especially when a new political party comes into power. A future focus should be on ensuring lasting features, powers, and resources of a GGR hub (for instance, exploring legislative options for GGR policy).

4.2. Navigating Australian Federalism

A second priority is navigating Australian federalism. Powers are divided between the federal government and State and Territory governments. Effective systemic GGR policy requires effective coordination at multiple levels and jurisdictions.

The federal government is bound by Section 51 of the Australian constitution. If an issue area is not mentioned in Section 51, responsibilities only lie with the States and Territories. In these areas, large-scale policy coordination is difficult. The Australian energy transition for instance has been hampered by federalism issues (Kallies, 2021). Energy, environment, and climate are not part of Section 51. These coordination challenges have resulted in delay and uncoordinated patchwork policy.

GGR may run into similar issues. GGR is not mentioned in Section 51, and neither are critical areas like innovation, science, or land management. Given the diversity of GGR approaches, GGR may encounter more severe federalism bottlenecks than energy.

A treaty that imposed GGR obligations may provide a workaround. ‘External affairs’ are a Section 51 responsibility – if an action is required for international obligations, responsibility can flow back to the federal government. For instance, environmental protection is the remit of States and Territories, but environmental protection for world heritage (commitments of the World Heritage Convention) is the remit of the federal government (High Court of Australia, 1983). However, treaty creation is a long-term process that does not match the rapid timeframes necessary, and existing relevant treaties such as the Paris Agreement (whose main substantive commitment is just submitting an improved Nationally Determined Contribution) lack strong substance (Allan, 2019; Hoffman *et al.*, 2022) that would justify strong external affairs powers (Senate Legal and Constitutional References Committee, 1995).

Federalism must be managed head on. Kallies (2021) provides a suite of measures for responding to Australian federalism. Constitutional amendments or explicit conferring of responsibilities from States and Territories to the Commonwealth could rewrite the division of responsibilities, but these are substantial political investments that only seem feasible under the most drastic conditions. Existing Section 51 responsibilities could be leveraged at the federal level where possible (like using the banking responsibility to increase GGR investment from banking institutions). However, this would call for a

highly liberal interpretation of Section 51 and does not provide the necessary systemic and holistic policy approach.

One final option is to turn federalism into an advantage. There are two pillars of such an approach.

First, cooperative federalism is necessary to reduce miscoordination. This involves deliberate cooperation between states (Zimmerman, 2001), in this case in order to increase GGR development and implementation. Mirror policies (like consistency in ACCU Scheme or research grant application requirements) are important to reduce the transaction costs of innovation. Deliberate knowledge and network sharing can also help expedite the creation of important innovation networks (Acemoglu *et al.*, 2016). Different States and Territories can lean into collective strengths to address individual weaknesses. For example, New South Wales has limited geological storage capabilities (Watson *et al.*, 2015). But the Surat Basin, just over the border in Queensland, could overcome storage concerns (La Croix *et al.*, 2020). Importantly, this is an area where the government has recent experience. The federal government plays a leading and coordinating role in creating a cohesive national vision of an Australian hydrogen industry, formalised in the National Hydrogen Strategy (which is currently undergoing review in collaboration with the states and territories (DCCEEW, 2023b)).

Conversely, competitive federalism is necessary to leverage diversity and market competition between states (Brown, 2012). This involves deliberate competition between states for GGR firms, investment, and development. Rather than creating collective advantages, competitive federalism would look to carve out and develop the individual comparative advantages of states (for instance, different mineral deposits or research infrastructure). This is a ‘race to the top’ for attracting GGR innovation.

The potential for effective Australian federalism is particularly high post-COVID. COVID-19 saw the Council of Australian Governments, noted for its administrative burden and inefficient bureaucratic red tape (Conran, 2020), replaced with the National

Cabinet. Unlike COAG, which met only twice a year and had a protracted negotiation process for discussing out-of-session issues, the National Cabinet met as frequently as needed and was able to more dynamically discuss issues raised by public service officers, and thus has been praised for its improved efficiency (Tulich *et al.*, 2020; Murphy and Arban, 2021). The National Cabinet has a set of priorities for coordination, like housing and electricity transmission, that are reviewed every year. GGR could be a listed priority in future. The National Cabinet should prove politically resilient moving forward, having received bipartisan support thus far.

4.3. Earning trust and funding for high quality vegetation sequestration

Despite its controversies, the ACCU Scheme will play a large role in Australian climate policy and GGR in the short term. ACCU Scheme reform will be critical to ensure removals are real, robust, and have social trust.

Social trust of the ACCU Scheme is important since GGR may be in the early stages of political bundling with the ACCU Scheme, which heavily relies on removals from vegetation. Genuine concerns of low-integrity ACCU Scheme offsets may leak into GGR, with GGR garnering a perception as simply fuelling low-integrity offsets. The bundling (Colvin *et al.*, 2020) of nascent GGR with the intense controversy of the ACCU Scheme is a risk to the success of GGR in Australia. Hence, addressing ACCU Scheme controversies are critical.

Key to ensuring social trust is to robustly implement the recommendations of the Chubb Review. The Chubb Review's recommendations could substantially improve the performance (Macintosh *et al.*, 2023), and thus social trust, of the ACCU Scheme. It recommends substantial reforms to improve ACCU Scheme transparency and data confidentiality rules, allowing third parties to verify project integrity (Chubb *et al.*, 2023). However, the implementation of these transparency provisions has not quelled integrity criticism (Macintosh *et al.*, 2023).

Hence, going *further* than the Chubb Review’s recommendations would best reinforce integrity and broader trust in the ACCU scheme, position Australia as an international leader in carbon market integrity (particularly in project-credit markets), and provide new opportunities to incentivise GGR.

A differentiated ACCU system is one such mechanism that can hit at both integrity and GGR support. ACCUs are currently completely fungible – they are priced and traded the same across different methods of creation. However, if the objective is accelerating innovation, novel GGR with more permanent storage should not be treated the same as unreliable, well-known land based GGR (Burke and Gambhir, 2022). ACCU’s could be differentiated into separate tiers, with a higher tier of ‘prime’ ACCUs created by projects with high integrity, permanence, and novelty, as well as providing clear social or biodiversity co-benefits. This would result in a higher price for high quality GGR projects, increasing GGR funding (as well as ensure alignment with emerging best practice disclosure standards like the ISO Net Zero Guidelines).

‘Prime’ ACCUs could be coupled with additional policy nuances, for instance a removal of price cap (more generously awarding high quality project operators), having a conventional auction instead of reverse auction for government purchases (a more climate risk averse pricing approach (Daniel *et al.*, 2019)), or being the only form of ACCU that Safeguard Mechanism facilities can buy (mitigating the risk and perception that carbon markets incidentally increase emissions).

The Chubb Review’s recommendations have been accepted by government. What is unclear is the extent to which the recommendations will be implemented. The feasibility question, therefore, relates to the political feasibility of implementing the Chubb Review *well*, as well as the prospect of further reforms to best ensure trust and integrity.

The nature of the Chubb Review implementation is an evolving issue, with DCCEEW releasing an implementation plan in June 2023 and hosting stakeholder engagement sessions throughout 2023 and 2024 to guide Chubb Review implementation (DCCEEW,

2023c). A full analysis of reform implementation cannot be covered here but should see ongoing focus from academia and policy analysts.

Going further than Chubb-recommended reforms seems unlikely in the short term. The ACCU Scheme plays a major role in providing flexibility to industrial facilities that must reduce emissions under new Safeguard Mechanism requirements. Substantial reforms that greatly restrict ACCU supply may not be politically feasible.

The longer term is where prospects for further reform lie. There are initial legal action challenging carbon neutral commitments that rely on low-integrity offsets (Jeong, 2023). The increasing threats of legal action and reputational costs of low-integrity offsets may allow for increased political and industrial acceptability of even more stringent ACCU controls in future.

4.4. Sparking innovation for new technologies

Existing vegetation and soil-based methods of removal will be insufficient to tackle the magnitude of the removal task (Smith *et al.*, 2023). New removal methods with greater permanence, and that can capture short-lived greenhouse gases such as methane, require development (Rau, 2019).

There is an early suite of novel removal methods in development, mostly based in the US, UK, and Europe, focusing on bioenergy with carbon, capture, and storage (BECCS), biochar, and DAC (Smith *et al.*, 2023). Although not an initiator in technology development, Australia can still lead in advanced demonstrations, creating niche markets, and accelerating scale up.

With a boom in GGR firms reaching demonstration and deployment stages (Smith *et al.*, 2023), Australia should encourage international GGR actors to invest in Australian demonstrations and reap the benefits of Australia's comparative GGR advantages.

A priority for both research and policy moving forward is to identify opportunities to integrate international developments into Australian contexts. For example, the development of ‘DAC Hubs’ in the US (Twidale *et al.*, 2023) could be mirrored in Australia, integrated with Renewable Energy Zones under development that would provide world leading renewable energy production (Climate Council, 2020; DIgSILENT and Baringa, 2020). Similarly, enhanced weathering projects could be integrated into the new critical mineral mines in development around Australia, to operate synergistically rather than competitively (Stutt, 2023). Australia has an abundance of minerals important to renewable energies, like nickel, that are often co-located with ultramafic rocks that are suitable for enhanced weathering (Wilson *et al.*, 2014). The environmental impacts of critical mineral mining must be appropriately considered in this process.

Attracting existing overseas innovation is important, but it does not create new GGR approaches. Australia must spark domestic GGR innovation due to the specific regional optimisation required by many GGR approaches. DAC sorbents for instance must be optimised for Australia’s climatic conditions, dust levels, and humidity (An *et al.*, 2022). A concerted GGR innovation strategy, like the recently released Electric Vehicles or Quantum Strategies, could incentivise Australian GGR innovation.

Others have flagged the importance of additional funding under the Australian Renewable Energy Agency (ARENA) or the Clean Energy Finance Corporation (Australian Academy of Science, 2023a; Climate Change Authority, 2023). These initiatives would require (re)amendment of these organisations’ core mandates: ARENA for instance no longer funds carbon capture and soil carbon (ARENA, 2022).

Increasing research funding in GGR is critical, for instance with an Australian Research Council (ARC) Centre of Excellence on GGR, expanding CSIRO GGR project funding, and developing ARC-supported interdisciplinary PhD and postdoctoral GGR fellowships to develop the future GGR talent pool. Adding GGR technologies to the List of Critical Technologies in the National Interest could be a useful first step to aligning and priming Australia’s innovation system for GGR acceleration.

Lastly, the National Reconstruction Fund (an off-budget investment platform of AU\$15 billion to develop Australian industry) and the Industry Growth Program (AU\$392.4 million for funding early enterprise) may provide scope for GGR funding. At least AU\$3 billion is earmarked for “renewables and low emissions technologies”. This is an intentionally broad categorisation, of which GGR could potentially fit. Such funding mechanisms are critical levers in supporting GGR innovation and mobilising private finance (especially where early technology costs are high). Such private sector collaborations are critical in identifying early market opportunities (Coles *et al.*, 2018).

An important niche market for GGR development would be a carbon utilisation market. Carbon utilisation does not guarantee reliable carbon storage (carbonating drinks (Heilers, 2019) means stored carbon is released when a can is opened), nor do utilisation industries always have strong scalability (there is a limit to how many carbon-made sunglasses (Trendafilova, 2021) should be manufactured), but these industries may still be useful in diversifying and increasing economic and social support for GGR. Utilisation that can offer more permanent storage and displace other carbon intensive activity, like carbon storage in cement (Li and Shi, 2023) (already under development in Australia (MCi Carbon, 2023)), should be priority areas of development. Government can contribute to building this market through procurement preferences, regulation, or facilitating business networks (Coles *et al.*, 2018). However, not all utilisation industries are desirable – enhanced oil recovery for instance would compromise emissions reduction efforts and court unnecessary controversy (de Kleijne *et al.*, 2022).

Another important factor is the development of GGR tradespeople. Australian renewable energy and housing markets have both been bottlenecked by a lack of tradespeople (Rutovitz *et al.*, 2022; Visontay, 2023). Reducing technology costs ultimately has a limited impact if there is no development of key supporting ecosystems (Adner and Kapoor, 2016), like a GGR workforce.

As important as these initiatives would be, there is an underlying innovation need that must be addressed. Namely, Australia's innovation system at its foundations requires reform. A new funding stream for GGR, supporting individual projects, will not change the fact that Australia is ranked 61st in the world for entrepreneurship (Cilento, 2022), nor that Australia's innovation system is split across 202 programs and 13 portfolios (Industry Innovation and Science Australia, 2021), or that in 2023 Australia's investment in research and development as a percentage of GDP is its lowest on record (Australian Academy of Science, 2023b).

Australia's industry is instead reliant on minerals, farming, and real estate (Australian Bureau of Statistics, 2023). This follows a historical lack of ambition in Australia's industrial strategy (Horne, 1964). Sparking Australia's core innovation capabilities is important not only for GGR, but for other priority areas like Artificial Intelligence.

Effective innovation isn't simply creating a new focus area of investment, but ensuring social, economic, and regulatory systems permit and attract innovators (Geels *et al.*, 2017; Atkinson, 2020). There is also a need to ensure a balance between a shared innovation and GGR vision that aligns innovators, and a sufficiently diverse innovation system to ensure the multiplicity that GGR requires. Fixing Australia's innovation system is out of the scope of this paper, but raising this is important to connect GGR to the broader innovation ambitions of Australia. Whether for GGR, quantum computing, or advanced biomedicine—the industries of tomorrow require strong innovation foundations.

The feasibility of a GGR innovation program and overhaul of Australian innovation is unclear. These hinge on the political interests and promises of the 2025 election, which for now is uncertain. If the 2023 Review of Australia's Science and Innovation Priorities highlight GGR as an innovation priority, that could give credence to a GGR innovation strategy. Reform of Australia's core innovation capabilities, however, would be a larger process requiring long-term political buy-in and stability. This has been difficult to achieve over the past decade.

Also important is addressing government fragmentation. Industrial policy is in DISR, research funding is managed by the Department of Education, and climate strategy is in DCCEEW and PM&C – capabilities that were not always split (Australian Public Service Commission, 2013). An integrated GGR innovation strategy, even with political will, is difficult with such fragmentation.

GGR framing can affect its acceptability and feasibility. GGR can be framed as an additional task necessary for emissions commitments and temperature goals, or it can be framed as an issue of innovation and industrial opportunity (Cox and Edwards, 2019). The carbon credit market, of which GGR can play a major role, could be worth US\$50 billion by 2030 (Blaufelder *et al.*, 2021), which Australia could lead. Not only would such a framing emphasise economic opportunity, it could also target much-needed technological GGR approaches, rather than simply expanding existing vegetation-based GGR approaches. This is not to discount the conventional climate framing. Both frames can be politically effective, and the urgency of climate change requires using every potential ‘sell’ available.

5. Discussion & Conclusion

This paper proposes four priority pillars of research and action:

1. Create an organisational home at a federal level for interdepartmental policy and strategic coordination;
2. Coordinate a strategic balance between cooperative and competitive federalism between states and territories to avoid delay and fragmented GGR policy;
3. Secure a high integrity offset market and land sequestration to ensure social licence, political trust and to avoid unproductive ideological bundling with GGR; and,
4. Leverage Australia’s comparative advantages to attract international GGR innovators, reform its domestic innovation system, and develop a GGR innovation strategy to spark novel GGR approaches.

While these are designed for Australia, these principles can be applied overseas:

- Effective coordination is important for any GGR leader, yet these procedural aspects have received limited academic attention. ‘Do we have the necessary policy capabilities?’ is just as important to consider as ‘What are effective policy frameworks?’. It is important to provide early GGR policy movers, such as the UK, Denmark, and Switzerland, with the best available knowledge and analysis of how to most effectively and efficiently coordinate the wide-reaching issues of GGR and prosecute GGR ambitions.
- Ensuring vertical integration across different jurisdictions and stakeholders is central to GGR implementation. Actors at different scales have different roles to play, ranging from high level political leaders who create big picture targets that align policymakers and innovators, to local and regional authorities that implement and prosecute policy consistent with social licence. Coordination of all these levels, via the simultaneous leveraging of cooperation and competition, is necessary for effective multi-level governance of GGR. The US for instance will have to align the interests of historically laggard Republican states, for instance by repeating the Republican-target funding approach that opened the policy window of the Inflation Reduction Act (Tamborrino and Siegel, 2023). As another example, the EU will have to ensure the creation of a comprehensive common operating picture of GGR across its countries and subnational authorities. Different actors should be aware of their specific roles in GGR, and of the roles of others across different levels and jurisdictions in order to ensure an GGR ecosystem that efficiently maximises collective advantages, rather than being duplicative, disjointed, and piecemeal efforts.
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- Ensuring carbon market trust to prevent unproductive political bundling is similarly important in other constituencies. Other international and domestic (UK Government, 2023) carbon markets are already integrating GGR into carbon market mechanisms, however the social licence and political bundling implications of this integration has thus far received limited attention. Ensuring that GGR-carbon market integration is a mechanism of acceleration rather than polarisation is critical. Australia may have a comparative advantage in that entirety of Australia has a single national system for carbon offsets. The EU and US both have differing national and subnational carbon market systems that can increase the difficulties of ensuring whole-of-system robustness and trustworthiness of offsets, and therefore of GGR. The EU is developing a continent-wide certification framework for removals, but this framework is voluntary and in practice may still result in highly differing standards for removal, potentially compromising the trust and social licence of GGR.

- The GGR innovation literature is limited, and this paper helps advance this nascent and vital corpus. Most importantly, GGR innovation is not just a matter of increasing funding for GGR projects. GGR innovation is about creating a general environment of risk-taking and entrepreneurship. Addressing systemic barriers to long-term paradigm-shifting innovation is important not just for GGR, but also for other innovation priorities. Framing is also a related and important issue. GGR can be framed, justifiably, as an additional task to enable low temperature emissions trajectories. But it is also a major industrial and economic opportunity – many countries will need to source removals from international sources to meet net zero and temperature targets. Australia, and other comparable countries such as the US (which is already investing billions into DAC development), are uniquely positioned to meet this international demand.

For Australia, there is a difference between what can be done in the next one to two years, and what can be done once GGR is a core political priority. Policy effectiveness is not just the design of policy approaches, instruments, or packages. Sequencing is important (Kallbekken, 2023). Much of our analysis hinges on GGR becoming a political priority, relying on the Climate Change Authority recommending a removal NDC to break open the political window. However, the interim steps, either in policy or in political advocacy, are an important enabling factor to policy effectiveness in the future.

Furthermore, effectiveness will ultimately be a function of implementation. The key is not just implementing individual technologies, but creating entire supporting infrastructures around them. DAC for instance is not just implementing DAC units. It involves creating entire carbon neutral or negative DAC systems – including renewable energy, carbon transport, and storage. Underdeveloped technological ecosystems can bottleneck technologies even when they have strong price-performance (Adner and Kapoor, 2016). Ongoing transparency, monitoring, and evaluation are also important to develop moving forward. Policy mistakes and innovation failures always happen. Learning from them to best accelerate and scale up GGR, and building policy processes to do so, is critical.

Especially important to effective incentivisation is earning social licence and ensuring tenets of responsible innovation are embedded in decision-making (Owen *et al.*, 2013;

Moffat *et al.*, 2016). Imposing on bottom-up agency, for instance in agricultural management or land use decisions, can spark unproductive political divisions or polarisation. Negative side effects of GGR activities, or their *assumed* effects, can also contribute to such outcomes (Honegger *et al.*, 2021).

Across each pillar presented is thus a need to centre the pursuit of social licence.

- Big picture budgeting and design should engage local communities early and genuinely, avoid negative side effects, and calibrate GGR deployment to community values and preferences.
- States and Territories (and indeed more local government actors) implement such consultations to ensure they are genuinely participatory and deliberative.
- DCCEEW has flagged identification of co-benefits for ACCU Scheme projects as a priority (DCCEEW, 2023c), but should take the extra step of considering how projects can create societal, and particularly local, co-benefits while reducing negative impacts (Jassim *et al.*, 2022).
- The alternative framing of innovation *opportunity* can be a gateway for broader social engagement and support. Social licence isn't just avoiding negative impacts, but creating a shared and inclusive vision of social and ethical desirability.

The strategy of this paper is a big ask from multiple levels of the Australian government, innovators, and researchers. But even if fully implemented, it may not spark changes large nor fast enough to limit global warming to 1.5 or 2°C. Much of the action ahead hinges on the prospects of, and political response to, a 2025 GGR NDC. But concerted climate action, included GGR, is already well overdue.

This strategy has been designed with political feasibility in mind. A useful area of research could look to flip this. Based on climate needs, what are the transformational political and policy changes necessary for GGR to fit the high bars of speed and magnitude? Both incremental pragmatism (Rothmayr Allison and Saint-Martin, 2011) and transformational political change (Patterson *et al.*, 2017) can be effective.

Whatever the approach, Australia has a strong role to play. It can lead a fair go at net zero, or it can again resign itself to the back of the pack.

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Foreword to Chapter 6: Need for Speed

6.1. Place in the thesis

The previous chapter explored GGR policy with contemporary political windows in mind. However, even its ambitious goals for Australian GGR policy would likely be insufficient to adequately accelerate GGR. This paper is another attempt at GGR policy, this time focusing on the most extreme instances of technological acceleration. Exploring the reasons for delay answers Research Question 1a., and exploring the avenues for extreme acceleration helps answer Research Question 1b.

6.2. Key Conclusions

GGR is likely to err slow, and certainly is not likely to match the lightspeed development of GGR in emissions trajectories that limit global warming to under 1.5 or 2°C. This is because: large scale technological changes, like GGR would be, have development timespans that take many decades and often over a century^{1,2}; GGR requires changes in surrounding technological, social, and political systems that have slowed innovations in the past^{3,4}, and the diversity of GGR approaches limits the economies of scale advantages⁵ of reaching a ‘dominant design’.

However, there have been instances where early-stage innovations were forced into rapid deployment. Synthesising historical accounts of these extreme accelerations can help inform acceleration of GGR. The examples of the Manhattan Project, Apollo Program, and COVID-19 vaccine development have the following similarities: a lack of a hostile incumbent regime, massive investment, and overwhelming political will.

Manging GGR’s incumbent relationship with the existing climate regime, and determining how to create overwhelming political will for GGR may allow for extreme GGR acceleration that better fits 1.5 or 2°C timelines.

However, rapid accelerations come at a high social price. When moving fast, principles of responsible innovation are further marginalised. Extreme acceleration of GGR would hurt many communities. These risks should be considered in the context of, and in comparison to, the risks of climate change, maladaptation, and stratospheric aerosol injection.

6.3. Significance to thesis

This chapter provides an avenue for GGR to be plausibly accelerated to align with 1.5 or 2°C pathways (if coupled with ambitious emissions reductions - a big if). However, whether this is desirable is unclear. This also has implications for the desirability of stratospheric aerosol injection. As bad as the risks of extreme GGR acceleration are, they do not threaten impacts worse than worst case climate change.

This chapter also relates to discussions in Chapter 3. Chapter 3 emphasises that there are multiple different mechanisms of change. Technological change is not necessarily accompanied by (necessary) social, political and policy change. The analysis of Chapter 6 is essentially an analysis of power-based forcing of technological change, rather than change being the product of price-performance improvements or democratic societal needs.

The findings of Chapter 6 also reinforce Chapter 3's discussion of lock-in, emphasising the need to explore lock-in of democratic representation, procedural and distributional justice, and holistic and complex risk assessments in decision-making. The additional nuance that Chapter 6 adds is that it raises the question of to what extent these principles should be locked-in compared to ensuring sufficient pace and acceleration (achieved through power-based and top-down forcing).

Chapter 6's discussion of a need to integrate different risk assessments is informed by the approach to holistic risk assessment discussion in Chapter 4. Integrating risk assessment for GGR acceleration, SAI and maladaptation and climate change itself is core to the compound hazard approach that was used in Chapter 4's risk framework. Chapter 4

applied the holistic risk assessment to SAI, but Chapter 6 takes the argument further in arguing for the need to apply it across the gamut of climate risk and response.

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Chapter 6: Need for Speed? Extreme Acceleration of Greenhouse Gas Removal

Abstract

Greenhouse gas removal (GGR) capabilities must be developed in a nearly impossibly short timeframe. Large systemic technological changes usually take roughly a century. The complexity and diversity of GGR further increase the difficulty of rapid GGR deployment. However, peppered throughout history are instances where technology innovation broke the proverbial speed limit. The rapid innovations of Manhattan Project, Apollo Project, and development of COVID-19 vaccines were driven by overwhelming political will, allowing for massive investment. This points to political will as the key leverage point for timely GGR implementation (and renewable energy transitions). However, such acceleration would worsen negative GGR social and ecological side effects. These risks must be discussed in the context of, and in comparison to, the catastrophic risks of climate change or alternative policy responses like solar geoengineering. There are no good options. We can only aim for the least-worst ones.

Introduction

Insufficient emission reductions mean we very likely need greenhouse gas removal (GGR) to limit global warming to Paris Agreement targets¹. We need GGR fast, particularly to avoid or reverse overshoot of the 1.5°C target² and given the possible underestimation of climate risks thus far^{3,4}.

Emissions trajectories can belie the urgency of developing GGR. They show net emissions. Previous analysis has shown that when reductions and removals are split for scenarios that limit global warming to under 2°C, with net zero in 2070, the world starts seeing removals at a gigaton (billions of tonnes) scale starting from 2030⁵. Since 2016 there have only been more emissions and more warming, yet many net zero targets (which will require GGR) aim for 2050 or sooner. The inescapable truth is that, in addition to a substantial increase in mitigation ambition, we need large scale development of GGR today.

However, current worldwide GGR is roughly 2 gigatons per year⁶. The vast majority of this is from conventional land management. These will become increasingly unreliable stores of carbon due to climate change⁷, and are greatly limited by land use constraints⁸. 0.002 gigatons of removal is a result of novel GGR approaches, like bioenergy with carbon capture and storage (BECCS)⁶. These novel approaches can result in carbon storage on the timescales of hundreds to thousands of years. Such technological approaches require massive and rapid scale up (growing four to six orders of magnitude by 2050, and with a growth rate of over 50% a year⁹) if we are to limit global warming, or minimise dangerous overshoot of temporarily going over temperature targets. Innovation acceleration of GGR is critical.

However, innovation acceleration has received limited attention in the extant GGR literature. Most analysis is still technical^{10,11}. The extant non-technical literature focuses, justifiably, on the negative side effects of implementation¹². Responses to climate change are themselves sources of risk¹³.

As a result, important insights of innovation, and its acceleration, are not well represented in the literature^{9,14}. This is a dangerous omission – there is a rich literature on innovation acceleration that can provide useful perspectives to the GGR debate, especially given the innovation gap that threatens the feasibility of low temperature emissions trajectories.

The extant GGR innovation literature frames innovation as a linear progression from supply to demand, emphasising a need for demand-side research in areas like niche markets, demand pull, and public acceptance¹⁴. The literature emphasises the importance of incentives like direct investment, a carbon price, or creation of utilisation markets^{9,10,14–18}. There is also a strong emphasis on bottom-up community engagement, emphasising the tension between optimistic model demands and constrained realities^{19–23}.

Further opportunities lie in investigating not just general acceleration and incentivisation, but acceleration at the fastest paces. There has been initial scholarship that flags emergency deployment of hydrogen energy²⁴. Extending this approach to the GGR portfolio would greatly inform the effective policy approaches for GGR.

Furthermore, while discussing selection and design of policy incentives is important, understanding the enabling leverage points of ambitious innovation and acceleration policy is the key gap to address. The acceleration ‘playbook’ is relatively clear. We have decades of scholarship and practice that tell us to increase investment, create safety nets, and facilitate demonstration projects and niche markets^{25–28}. Understanding how they fit into, or even open, ambitious political and policy windows is far less developed, more difficult, and far more important^{29–31}.

Further developing connections between the innovation and GGR literatures, this paper starts by asserting that the GGR has specific characteristics – diversity, system complexity, and incumbent resistance - that make GGR likely to err slow. The seemingly impossible timelines of removal in prominent emissions trajectories seem even more impossible.

However, there are instances across history where change has happened rapidly quick. The next section identifies the enabling conditions for these rapid changes, informing effective support for GGR acceleration.

Erring Slow

GGR is not just individual technologies. GGR is about changing entire social, political, and economic systems. Understanding the pace of change is thus not just about assessing individual technologies or products (the focus thus far³²). We must consider how quickly entire technological, social, political, and economic systems change. The evidence of such systemic shifts points to a timeframe incompatible with climate ambitions.

Major *systems* level transitions take roughly 95 years^{33,34}. Industries like coal and oil took over 100 years to develop from early commercial implementations through to global dominance. Most GGR technologies are not even commercial, they are at earlier and more volatile stages of research. Even a relatively quick system transition, from candles to gas and kerosene lanterns, still took roughly 30 years. This was only a specific sector, lighting, which had a high demand for change and little lock-in to overcome. Candles were just no competition for lanterns, and there was no candle lobby in the British countryside. There is a subjective analytical question of when you define the ‘start’ and ‘end’ of an innovation, but these do not change the overarching fact that most empirical experience has been delay.

While we may hope that GGR bucks the trend, there are characteristics specific to GGR that point towards slowness.

First, the diversity of GGR demands creating multiple ‘dominant designs’. Early innovations generally are characterised by diversity and experimentation. We aren’t sure ‘what’ a specific innovation looks like. The early smartphone market for instance saw variance in the use of physical keyboards and landscape orientations (for instance, the Nokia 9500 Communicator). Then came the iPhone, whose design included a portrait full multitouch screen, as well as integrated iPod and mobile web surfing capabilities. This

solidified the idea of what a smartphone was. Competitors flocked to this new dominant design, sparking the meteoric growth of the smartphone industry³⁵.

GGR shares this challenge, but to a more difficult extent. The whole idea of GGR is that there is no single bullet. A mix of different approaches, determined by contextual fit, is necessary. Even a ‘single’ technology, Direct Air Capture (DAC), is currently represented by a range of different approaches³⁶. These altogether mean that there is no single dominant design to aim for. *Multiple* dominant designs are necessary. This makes acceleration difficult, especially since dominant design can be a strong engine of speed.

Dominant design is important because one of the best predictors of the pace of technological change is surprisingly simple – economies of scale³⁷. As systems become larger, they become more efficient, attracting additional growth. Yet a GGR ‘cocktail’ by nature limits economies of scale advantages. Rather than having a singular large GGR method that can be supercharged, we require multiple GGR methods that, owing to their smaller scale, will be less efficient and will grow less quickly.

Another major determinant of the speed of technological change is the extent to which an individual technology demands changes in broader technological (and social, political, and policy) systems³⁸. Light bulbs can be upgraded quickly because a new light bulb does not demand changes in urban energy infrastructure or policy support. You just unscrew an old light bulb, and screw a new one in. Conversely, renewable energies and electric vehicles both require substantial changes in supporting infrastructure – everything from upgraded grid transmission systems to deployment of battery charging stations – and have concordantly seen deployment delay even when they individually show strong performance³⁹.

GGR approaches require substantial ecosystem changes. GGR requires the development of entire (carbon neutral) carbon transport and storage systems³², and in the case of DAC, requires development of entire renewable energy systems with excess energy to spare. Broader still, GGR requires policy systems with updated and consistent reporting,

standards, and verification. Conversely, the current landscape is a wild west of inconsistent approaches⁴⁰. These changes in supporting ecosystems take time, and greatly add to the complexity of an already complex set of emerging technologies.

A final factor to consider is resistance from incumbent actors. Incumbent resistance is one of the prime drivers of delay²⁷, as seen with the fossil fuel lobby undermining renewable energy transitions⁴¹. The conventional logic of incumbent resistance is survival – incumbents want to prevent new disruptive technologies, preserving their place in the economic and political hierarchy.

Such a technological incumbent does not seem to exist for GGR. GGR would be a new industry, with its main revenue sources being a sufficiently high carbon price (likely from carbon market-based mechanisms) or large utilisation industry that both do not exist. There is no powerful entrenched lobby to fight against.

However, there is an alternative incumbent resistance. The current climate policy regime can be framed as a *political* incumbent, different to the conventional technological incumbent framing. Concerns of mitigation deterrence, fossil fuel extension, and poor removal integrity may compel GGR resistance^{42–44}.

For these reasons, we can expect the GGR transition to be slow. At the very least, these provide further credence to the idea that the timelines of removal commonly used to drive policy are infeasible.

Extreme Acceleration

However, peppered throughout history are significant technological changes that broke the proverbial speed limit. Synthesising historical accounts of these can help inform policy to sufficiently accelerate GGR. Indeed, given the magnitude and importance of the GGR task, the *extent* of acceleration is critical. It is important not just to assess the lessons from previous accelerations, but to assess the lessons from the most extreme instances of acceleration.

This paper looks at three such accelerations: the Manhattan Project, Apollo Program, and development of COVID-19 vaccines. In each of these instances, early science and technology was aggressively pushed through to deployment: nuclear fission was only discovered in 1938⁴⁵ (with nuclear deployment in 1945); the US only had Project Mercury under its belt when Apollo was launched in 1961; and vaccine development usually takes decades due to longitudinal testing (whereas multiple COVID-19 vaccines were developed in roughly 9 months). These are especially salient selections for GGR because they each involved multiple lines of research and required the bottom-up creation of entire industries and supply chains^{46–48}.

As the following sections show, these examples share common characteristics: a lack of a hostile incumbent regime, massive investment, and most importantly overwhelming political ambition. These can guide identification of the key enablers of extreme acceleration. However, across each of these examples also were major negative side effects.

Lack of hostile incumbent regime

An innovation's relationship with the incumbent industrial industry is critical. If an incumbent sees a threat, it may undermine the new entrant to ensure its own survival²⁷. Conversely, if an incumbent sees an opportunity to be strengthened, it may develop a symbiotic relationship that accelerates change⁴⁹. None of Manhattan, Apollo, or COVID-19 vaccines had a hostile incumbent, avoiding a critical barrier to speed.

The Manhattan Project's closest incumbent was the US's military and nascent military industrial complex and its international partners. This incumbent had every interest in developing nuclear weapons (especially after the British sent Mark Oliphant to lobby the US in 1941)⁴⁶. There was also no alternative weapon and associated lobby that had an interest in displacing nuclear research. Later in the war, nuclear development was necessary to avoid a full direct invasion of Japan⁵⁰ (though some argue this was driven by

pre-Cold War interests⁵¹). The bomb was the clear goal. There was no incumbent resistance. If anything, there was strong incumbent pull.

The Apollo Program similarly lacked a hostile incumbent. NASA was only three years old when the Apollo Program was launched (and had only Project Mercury as its experience). While the US had an existing aviation industry, Boeing was for instance a major contractor with the US military during World War 2, they were not going to be displaced by the creation of a space industry. Their experience in aeronautics would in fact prove invaluable (and lucrative) contributions for the Apollo Program's mission. Space was a new frontier, thus lacking a clear industrial incumbent and which had incentives for incumbent participation.

COVID-19 vaccines also had no hostile incumbent. The closest precursor industry was the pharmaceutical industry, who had strong interests in developing COVID-19 vaccine capabilities. While there was certainly social and political resistance to vaccine development⁵², this resistance was not borne from a concerted industrial special interest strategy.

Massive investment

Accompanying an open innovation environment was substantial investment that forced substantial innovation.

The 'secret' to the Manhattan Project was simple. Pay whatever it took to develop a nuclear bomb. Manhattan's original budget was \$400 million, personally approved by President Franklin D. Roosevelt, done with little external consultation⁴⁶. Costs soon ballooned, but the US continued paying. The four bombs that Manhattan produced ended up costing roughly US\$2 billion⁵³, around US\$23 billion in today's values. At its peak, Manhattan directly employed over 120,000 people, with many more unaccounted subcontractors, and established a research network of hundreds of project sites and over two dozen universities⁵³. And since scientists were unsure of whether to research electromagnetic, thermal diffusion, or gaseous diffusion to enrich weapons-grade

uranium, Manhattan developed all of them (endgame enrichment ultimately used a combination of all three since they were each individually insufficient)⁵⁴.

The massive investment in Manhattan pales in comparison to the investment in the Apollo program. From 1961 to 1962, the *contractors* for the Apollo Program alone earned over \$1.5 billion, with yet more funding going directly to NASA⁴⁷. Funding continued to increase throughout Apollo, even after Kennedy's assassination. The Apollo budget only decreased in 1968, after the Apollo 1 cabin fire disaster. Regardless, the billions the Apollo was still receiving was sufficient to successfully achieve moon landing in 1969. Apollo in total cost US\$26 billion, amounting to roughly US\$257 billion in today's values⁵⁵. These investments were made, in Kennedy's own words, as "an act of faith and vision" – it was unclear what benefits the Apollo investment would even provide (beyond putting a man on the moon)⁵⁶.

COVID-19 vaccines saw massive emergency funding, particularly from the public sector⁵⁷. The US government spent at least \$31.9 billion on mRNA vaccine development (of which only \$337 million was spent pre-pandemic, most of this was an emergency rush), funding everything from clinical trials to advance purchase commitments⁵⁸. Similar patterns can be found internationally. In 2020, the all-important crunch time for timely vaccine development, the UK government provided roughly 96% of the funding for the Oxford-AstraZeneca vaccine⁵⁹. Germany similarly spent nearly a billion funding the research of BioNTech and CureVac⁶⁰. These countries were representative of the *worldwide* investment in vaccine development.

Overwhelming political ambition

The policy approach of massive funding was enabled through overwhelming political ambition, particularly by political elites. Especially pertinent to the Manhattan Project and Apollo Program, the directions of massive funding do not always align with public interest.

The Manhattan Project was driven heavily by political ambitions. The early life of Manhattan was a more a conventional research effort with modest funding, a far cry from the vaunted nuclear weapon program we associate with today. It was only after Mark Oliphant in 1941 lobbied prominent American scientists like Vannevar Bush that the US recognised the feasibility and potential of nuclear weapons. With this understanding soon came the realisation, and fear, that Nazi Germany could be well on their way developing nuclear weaponry⁴⁶ (though we have since learned that Nazi investment in nuclear weapons was only modest, and heavily constrained by a damaged academic system and talent pool⁶¹). The American ‘Uranium Committee’ soon after saw a substantial increase in funding, as well as a name change to the more inconspicuous ‘S-1 Committee’. America had set its sights on nuclear, and would do anything to get it.

The spark for Apollo was the infamous ‘Sputnik Moment’ – when the Soviet launch of Sputnik 1 publicly signalled that the US was lagging behind the Soviet Union. Political actors, most prominently then Senator John F. Kennedy, used Sputnik to advance the public idea of the ‘missile gap’ – that the US was severely lagging behind the Soviet Union in intercontinental ballistic missiles⁶². Kennedy would leverage the political capital of the missile gap in his successful bid for the US presidency. Upon winning the presidency, Kennedy soon learned the missile gap did not exist. Constrained by his election promises and the public crisis he helped create, Kennedy saw a need to create a new arena that the US could publicly beat the Soviets in, thus resulting in the Apollo Program⁶³.

Even the execution of Apollo was heavily tinged with political interests. The Apollo program was closely designed by the United States Information Agency (an agency for ‘public diplomacy’, more accurately propaganda), ranging from astronaut selection to post-return international tours, with the objective to build an international coalition against Soviet and Communist influence⁶⁴. The drivers for Apollo were political. As Kennedy himself said to NASA Administrator James E. Webb – “I’m not that interested in space”⁶³.

The political impetus for COVID-19 vaccines was clear, end a world stopping pandemic that caused a worse financial crisis than the 2008 Global Financial Crisis. Governments enacted historic stimulus and investment policies to keep their economies afloat. The \$2 trillion stimulus package in the US was implemented without a deduction from a National Reserve. They *created* trillions to pay for essential goods and services⁶⁵, simply because they needed to.

Dark Side

However, there is a Dark Side of ambition⁶⁶. Accompanying large centralised infrastructural developments is often the unspoken pain that people without power are forced to endure⁶⁷. Questions of responsible innovation already occupy only a marginal space in innovation policy⁶⁸. When the name of the game is going as fast as possible, responsible innovation is thrown out the window.

The Manhattan Project achieved its goal of increasing destructive power, but created a new era of nuclear risk. While war and conflict are mainstays of human history, the difference now is that conflict brings the risk of nuclear destruction. One estimate for the probability of inadvertent nuclear conflict between the US and Russia was 0.9% per year⁶⁹, a probability now surely higher with the Russian invasion of Ukraine⁷⁰. The Manhattan Project itself was built off of the exploitation of African-American construction labour, who were subjugated to Jim Crow laws that created additional and lasting housing and economic inequalities⁷¹. The Manhattan Project also left a poison legacy. Improper safety and storage led to radioactive contamination of soil and water^{72,73}, especially prevalent at Native American reservations⁷⁴. Looking further, the Manhattan Project contributed to the creation of the American military industrial complex^{75,76}, now a major source of both greenhouse gas emissions⁷⁷ and existential risk⁷⁸. Indeed, the confidentiality and information suppression systems pioneered as part of the Manhattan Project are the foundations of modern regimes of government secrecy that undermine democratic accountability⁷⁹.

While Apollo has gleaned a reputation for one of star-gazing inspiration, it in reality lacked popular public support⁸⁰. Publics preferred that government increased spending in other areas, like social services, public safety, or reducing inequality. The US Government was criticised for ignoring its mantle of social and civic responsibility in favour of putting a “Whitey on the Moon”⁸¹. The Apollo Program only gained over 50% of public agreement that it was worth its cost at only one point in its existence - when Apollo 11 landed on the moon⁸². Otherwise, it was known to be spending money that could have gone to hospitals or schools. Apollo was a product of strong support by a small political coalition, not one of effective policy for the public good.

The negative impacts of rushing COVID-19 vaccines are less stark, and are subject to a far more nascent corpus, but they exist. COVID-19 vaccine mandates were necessary to raise vaccination numbers⁸³, but also sparked distrust and polarised^{84,85} healthcare systems that may ultimately undermine broader and future healthcare initiatives^{86–88}. COVID-19 vaccines could not fix ugly politics. They simply reflected (and potentially worsened) the politics that already existed. Vaccine development also saw the rise of vaccine nationalism and stockpiling, reinforcing economic inequalities and undermining pandemic responses^{89,90}. Despite costing \$1-3 per dose, existing vaccine inequalities, and being predominantly funded by the public sector, Moderna and Pfizer plan to charge over \$110 per dose in the US (with similar international moves likely to follow)⁹¹. COVID-19 vaccines unfortunately were not altruistic.

A Need for GGR Speed?

The historical precedent is clear. Technological change usually moves slow, but there is still possibility for breaking the speed limit, though this comes at a high moral cost. This section discusses the applicability of such an approach to GGR.

Critical to note is that the policy instruments used for these instances of extreme acceleration are not novel. Indeed, they simply reflect the advice of decades of innovation scholarship: increase investment, create safety nets and networks, and encourage risk taking^{25–28}.

Rather, the critical leverage point was political will. If powerful political elites want something badly enough, like escaping a severe crisis, they will spend as much as necessary to get it. This follows the experience with contemporary energy transitions. The bottleneck of transformational energy and climate policy is political capital, not knowledge of the necessary actions⁹². In the language of policy analysis: GGR policy instruments are important, but creating GGR policy *windows*⁹³ is the leverage point to aim for.

The primary objective therefore may not be to influence policy design (the focus of extant GGR policy research), but instead to investigate how to create the enabling political will for ambitious GGR policy. This also carries implications for applying the lessons of Apollo and Manhattan into broader climate action.

Attempts thus far at sparking a ‘Global Apollo Project for Climate’ focus on policy design, specifically the importance of public sector funding⁹⁴. However, the lessons to learn are primarily in the nature of their political preconditions, not the details of policy. Others are sceptical of deriving lessons from Manhattan or Apollo for climate, since they had clear problem structures (win the war, win the space race) with clear solutions (build a bomb, put a man on the moon)⁹⁵. As a notoriously wicked problem, climate does not share these luxuries. However, problem and solution framing are both a matter of political framing, not unchangeable policy realities. The problem framing of greenhouse gas concentrations in climate is a product of the language of scientists in the 1980s, which carried over into the design of the IPCC, and more importantly the UNFCCC⁹⁶ (where alternate frames around energy independence or battling a fossil lobby could have been more effective). The example of Apollo shows how political actors can invent entire crises to serve political goals. The problem and solution framings in climate were not borne from deliberate strategy, but rather the language of science.

Rather, the barrier to applying the lessons of extreme acceleration to energy change lies in the presence of a notoriously entrenched fossil industry incumbent, which all extreme

accelerations reviewed here lacked. Achieving historic speed with the presence of such a hostile incumbent seems unlikely.

Thus, in addition to unlocking massive political will, a related goal will be to ensure a healthy incumbent relationship. As argued previously, GGR has no technological incumbent, but rather a *political* incumbent in the climate change regime. A key priority in future is to determine how to ensure an open, or even symbiotic, relationship between the existing climate regime and GGR. Investigating how to overcome genuine concerns of mitigation deterrence, as well as how to avoid political bundling⁹⁷ with fossil interests are first steps.

There are also economic advantages to a large rapid acceleration. Massive investments early can spark earlier and more aggressive learning curves. These can be cheaper than early investments in low hanging fruit, and only investing in difficult emissions reductions or GGR later (especially given the high costs of unabated climate change)^{98–100}. The economic logic of lowest cost abatement does not always apply, especially when timeframes are short.

The obvious drawbacks of acceleration are the negative ecological and social consequences of such accelerated deployment. The extant literature already highlights such side effects as a priority issue to address in decisions and policy^{101–103}. They will be even worse for extreme acceleration.

However the issue is one of trade-offs¹⁰⁴. Negative and deeply unequal side effects of GGR implementation may help avoid the potentially worse, and also deeply unequal, impacts of climate change (whose catastrophic and existential risks are dangerously underexplored^{3,4,105}). Furthermore, more quickly addressing global warming may avoid the risks from solar geoengineering, whose risks are again deeply unequal and which may be even more severe than worst case climate change¹⁰⁶.

For yet another layer of risk complexity, rapid GGR developments may take efforts away from emissions reduction, or worse, provide fossil actors an avenue for continued extension and deliberate mitigation deterrence. GGR will have limited impact if there is not a concordant increase in ambition to decarbonise (which does not look to be on the horizon). Further, more extreme accelerations may create clearer and more concentrated political and economic losers, galvanising more intense resistance and undermining acceleration feasibility.

Such risk discussions already have a nascent role in GGR. Others have emphasised how early emissions reductions avoids massive and more damaging dependence on GGR in the second half of the century. However, risk horizons should be expanded to include the entirety of the climate portfolio – the risks of climate change, the risks of maladaptation, the risks of GGR, and the risks of solar geoengineering. They are all interconnected and there are no perfect solutions. There are arguably no *good* solutions. The issue is choosing the least worst approach.

This should not assume a central planner that makes rational risk trade-offs across renewable energy, GGR deployment, and solar geoengineering. However, the GGR and solar geoengineering discussions are nascent. There is still opportunity for academia to frame the corridors of discussion. Scientific developments, sometimes with limited practical considerations (like bioenergy crops the size of India or Australia enabling prominent 1.5-degree trajectories¹⁰⁷), have substantially influenced policy directions. There is still opportunity to highlight the directions and options that should be under consideration, whether that be extreme innovation acceleration or solar geoengineering deployment.

This initial paper cannot provide the answers to this complex risk analysis. Rather, this is a call to action to integrate climate, GGR, and solar geoengineering risk analysis. Extreme risks call for desperate solutions. Judging the extreme responses to climate change call for uncomfortable and difficult discussions. If we are to navigate them effectively, we at least need to start.

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Chapter 7: Last Call

As we continue to emit, we will continue to search for the lifelines of a world that avoids catastrophic climate change. GGR and SAI are two such lifelines. They are, in principle, important additions to the climate policy portfolio. Emissions reductions alone are no longer sufficient - GGR can enable low temperature emissions trajectories and minimise overshoot risks, but (alongside renewable energy transitions) may take some time to complete. SAI can stave off the impacts of global warming and buy critical time for decarbonisation and GGR.

However, technology is no panacea. These new responses to climate change have risks, and indeed may not even be able to fulfil the promises that they make. If they prove to be false hopes, we may have to contend with the possibility that we have reached the end of the line – that we have exhausted all options to effectively respond to the climate crisis and now face catastrophe.

This section summarises this thesis' explorations of their plausibility and desirability, discusses concordant implications, flags the limitations of the thesis, and outlines next steps for future research to build on.

7.1. Answers to Research Questions

This thesis has explored the plausibility and desirability of GGR and stratospheric aerosol injection. This is reflected in this thesis' research questions:

1. What are the prospects for implementing greenhouse gas removal at the scale and speed indicated in climate modelling and emissions reduction targets?
 - a. What are the reasons for delayed greenhouse gas removal innovation?
 - b. What are the policy and political settings that are necessary for sufficiently timely implementation?
2. What are the research and deployment risks of stratospheric aerosol injection research and deployment?
 - a. What are the prospects of a slippery slope from research to deployment?

- b. What is the holistic risk profile of stratospheric aerosol injection, and how does this compare with climate change?

In answering Research Question 1, this thesis finds that that GGR is likely to err slow. Historical large scale technological changes are on the order of decades, if not centuries^{1,2}. Furthermore, the complexity and diversity of GGR add additional layers of challenge that are likely to add delay (Chapter 6).

GGR policy, within the context of contemporary political boundaries, that sufficiently accelerates GGR seems implausible (Chapter 5). GGR, and broader climate action for that matter, demand a level of political ambition that policy leaders thus far have been unable to provide. Policy nuance can't fix politicians who don't care.

Only breaking open new levels of political ambition will provide the necessary impetus for change. The Manhattan Project, Apollo Program, and COVID-19 vaccines are not notable because of the policy structures that were used to accelerate innovation, but rather in the sheer political will that drove them (Chapter 6). Political elites wanted technology and would pay anything to get it. The name of the GGR acceleration game is not centrally about science or innovation policy, but rather how to unlock the necessary political will.

However, such extreme acceleration would come with severe negative social side effects. These risks, the risks of climate change, and of SAI should be discussed in an integrated manner. Extreme GGR acceleration is ultimately plausible, but whether it is desirable is deeply unclear. As a result, it is prudent to assess SAI, which can stave off climate risks and allow for a slower and less risky deployment of GGR.

In answering Research Question 2, this thesis finds that there are many pathways a slippery slope can take. Whether it be in normalising an otherwise extreme policy option, or by sparking political arms races, or by attracting Silicon Valley-type hype, initial research can plausibly slip to later deployment (Chapter 3). This is alarming because stratospheric aerosol deployment at worst can expose the world to risks worse than worst

case climate change. The conditions for effective governance, what Chapter 4 calls the ‘Miracle Pathway’, is vulnerable to changing political interests. Effective governance that minimises the most severe risks of SAI may be, at one point in time, plausible if political stars align. However, over a potentially century long deployment, such policy consistency becomes implausible.

These together ultimately mean that we have not yet reached the end of the line, but we are very close. While long-term effective governance of SAI seems implausible, extreme acceleration of GGR opens a narrow corridor for effectively limiting global warming. This narrow corridor has a caveat – GGR must be coupled with a similarly rapid improvement in decarbonising entire economies. While this may be plausible, it does not seem to be on the horizon.

Yet the more difficult question is whether we even want to go down this path. Extreme GGR acceleration would lead to substantial negative side effects. Whether these are worth the possibility of avoiding the risks of worsened climate change or SAI are unclear.

7.2. Broader implications

The implications of this thesis are manifold. This section flags the theoretical, methodological, and practical novelties of this thesis.

7.2.1. Theoretical

The argument in Chapter 6 for integrated climate, GGR, and SAI has substantial theoretical implications, not least because these are very much separate (and increasingly disconnected) areas of study. GGR and SAI were previously grouped under the umbrella term of ‘geoengineering’, and have rightfully been split apart to avoid conflation³. However, improvements in terminology do not justify the creation of academic silos. Responding to climate change is all about navigating and building policy packages, not individual approaches or instruments. Decarbonisation, catastrophic climate risk, GGR, and SAI should not be (again) conflated, but should see an integrated risk discussion.

This would help best inform the inevitably flawed responses to climate change we must assess.

Beyond big picture interactions, this thesis recontextualises the GGR discussion. Where others discuss ‘incentivising’ implementation⁴, this thesis highlights a different and more ominous story. If the goal is to limit global warming to 1.5 or 2 degrees, the name of the game is *extreme* acceleration. Acceleration like the speed of a Manhattan or Apollo project, and larger in scale.

This thesis also has major theoretical implications for the study of SAI. It emphasises the importance of the slippery slope (which some have previously argued should be disregarded⁵) and provides a framework to ensure its consistency (where previously the slippery slope term has been used very inconsistently). This thesis also helps provide a theoretical framework of slippery slopes that can help organise the research risks of SAI. Its focus on worst case or severe risks can help provide direction to SAI critiques, risk comparisons, and governance.

7.2.2. Methodological

This thesis’ application of systems thinking to GGR and SAI is novel, not least because this approach has not yet seen much explicit application in either field. Chapter 3’s use of systems thinking allowed for the identification of the mechanisms of the slippery slope. Chapter 4’s approach of identifying systemic interactions helps provide a clearer holistic picture of SAI’s many interconnected risks. The leverage point approach helped emphasise important procedural roles of GGR policy that have thus far been little discussed in Chapter 5, and helped identify the importance of political window (and not necessarily just policy design) for sufficient GGR acceleration in Chapter 6.

This thesis also has implications for broader study of technologies and responsible innovation. The framework of robust risk analysis in Chapter 4 was applied to SAI, but could also see application to other emerging technologies whose risks assessments are nascent (and contested), like quantum computing or artificial intelligence. This

framework is especially useful given the lack of robust risk assessment in the catastrophic and existential risk research field⁶. Its focus on systemic and compound risks fills an especially important gap in technology risk analysis. Chapter 3's typology of how research can lead to deployment can inform the governance of other emerging technologies, and helps contribute to our understanding of how technological changes happen.

7.2.3. Policy

This thesis further develops policy approaches for SAI deployment that can be considered even with the future uncertainties at play. Chapter 3 bucks the trend of previous literature and emphasises the importance of *encouraging* SAI lock-in, namely lock-in of democratic principles and principles of responsible innovation. The details of implementation would be subject to further study, but development of this unique approach is a substantial contribution to the SAI policy discussion.

Chapter 4 provides the basis for policy that reduces the extreme risks of SAI – these may seem unlikely, but we can be sure that these are risks worth reducing. It also discusses the enabling conditions for such policy effectiveness, namely multi-decadal political stability, that seem implausible. Chapter 4 thus enables decisionmakers to make more informed decisions on SAI policy. While risk perception, tolerance, and appetite are inevitably subjective and contested, the findings of Chapter 4 at least provide additional substance to subjective and uncertain risk governance. Chapter 4 also provides a framework for holistic risk assessment that can be applied to other key risks and policy issues, such as AI governance.

This thesis also expands the range of GGR policy to consider. Chapter 5 emphasises important procedural aspects of policy, namely effective central coordination and managing federalism. Exploring these procedural aspects of policy are vital, since governments are now trying to create GGR capabilities (as seen with the US Office of Fossil and Carbon Management) and have no academic literature to guide them. Chapter

5 is a first step to such guidance. Chapter 5 also can directly inform the priorities for future GGR policy in Australia.

Particularly important to note is that this thesis, particularly Chapter 6, does not put forward extreme GGR acceleration as a policy recommendation. Such accelerations would have severe trade-offs and side effects. Further study of these trade offs, again ideally via an integrated discussion with climate and SAI risks, is necessary before an extreme acceleration can be recommended. Rather, this thesis recommends further research on this approach and recommends a more holistic and integrated discussion of climate risk and response.

7.3. Limitations

PhDs must navigate limited time and resources. Such navigations are especially difficult after a pandemic hits. This limited the range of foresight techniques that could be used. More participatory foresight approaches (which ideally would include in-person participation) would have been a strong complement to the structured expert elicitation approaches used throughout the thesis. Chapter 6's discussion on integrated risk discussions between climate change, GGR, and stratospheric aerosol injection especially would have been interesting in a diverse group discussion. As a single author with many unknown blind spots and biases, the most I could do was flag the importance of such a discussion rather than individually make claims about what would likely be a deeply contested discourse.

Especially important to investigate in future work would be risk tolerance and appetite. This thesis has shown, from a more empirically informed analytical perspective, that there are major risks on the horizon. But that is truly important is what should be done about those risks, and that can only be informed by asking the difficult and subjective question of 'what types of risk and how much risk are we willing to accept, and for who and under what timeframes?'. This would call for a much broader set of far more participatory analytical techniques.

Along the same lines of analytical pluralism, this thesis' treatment of technological change could have been complemented with a statistical approach of change. While there are major analytical questions of when a transition 'starts' and 'ends'¹, such analyses can still produce useful insights. They can help to more precisely answer *how fast* is fast? This is especially relevant to Chapters 3 and 6. How fast does a slippery slope lead to deployment? How fast is 'extreme' acceleration compared to 'conventional' acceleration, and is there a middle ground?

While Chapter 5's discussions on Australian policy were broadly informed by insights gleamed from my networks within the Australian Public Service and the Labor Party, much of these insights unfortunately were not appropriate for public writing. There are important discussions to be had about the political strategy of Australian GGR, especially in the lead up to Australia's next Nationally Determined Contribution and in the case of a future change of government. These discussions will unfortunately have to take place without the contributions of peer-reviewed research articles. This unfortunately limits the extent to which academia can inform effective policy, but is a broader issue of government-academia collaboration rather than a single PhD to resolve.

7.4. Next Steps

This thesis has identified overwhelming political will as a key lever to acceleration. Next steps of this research would interrogate how to enable and create such political will.

The first pillar of such lies in social movement strategy. The climate movement has been previously criticised for failing to attract sufficient, and lasting, social attention. The framing and focus on greenhouse gas concentrations was not a matter of deliberate social strategy, but the path dependency of the language of early scientists⁷. Climate change has no clear identity ownership⁸, has a time horizon too long to galvanise sufficient short term attention (which makes climate easily displaced by other issues)⁹, overly focuses on breaking bad habits rather than presenting a superior alternative (this is changing with the framing of renewable energies, but still is very much the case with issues like diet)¹⁰, and simply has made too many strategic mistakes (for instance focusing sustainable finance

on stock-picking and divestment rather than changing banking institutions)^{11,12}. While much of the failure of climate policy and politics can be attributed to the influence of the fossil lobby^{8,13,14}, the climate movement has not put its best foot forward. Galvanising popular demand for decarbonisation and GGR is critical.

However, even with the most optimistic strategic advancements in social movements, healthy public engagement and dialogue is not possible without improving on the contemporary media landscape, the second pillar. Productive public discourse and effective representative democracy is not possible when the media landscape is controlled by an increasingly small set of special interests. News Corporation, owned by Rupert Murdoch, for instance is notorious for its denialist (and now delay) reporting on climate change^{15–18}. Such media influence influences voting patterns of electorates, which limits the political window for climate ambition. Exploring avenues for media reform^{19,20} would be critical to opening sufficient political windows for climate action.

A related next step is to address the issue of incumbent relationships. GGR has no clear technological incumbent (as discussed in Chapter 5 and 6), but may have a political incumbent in the existing climate regime. It is unclear whether GGR will find a place alongside the pillars of mitigation, adaptation, and loss & damage, or whether GGR will see resistance and rejection as a tool of mitigation deterrence (even if inadvertent). Exploring this dynamic will be critical to the future of GGR.

Furthermore, the incumbent of the climate regime, the fossil fuel industry, has for too long undermined efforts to decarbonise. Christiana Figueres, previous Executive Secretary of the UNFCCC and paragon of diplomacy, has recently argued that fossil firms are incapable of change²¹. Exploring avenues for displacing this incumbent regime, or at the very least limiting its grip on politics, will be critical to the enabling the narrow corridor of extreme GGR acceleration (noting that GGR itself still requires decarbonisation to be effective).

Furthermore, the suite of emerging climate responses extends beyond GGR and solar geoengineering. There are other emerging responses to climate change that also warrant attention. Economic degrowth for instance advocates for shrinking parts of the economy which are unproductive or parasitic (like the real estate or fossil fuel industries) and growing productive valuable sectors of the economy (like renewable energies or social services)²². This approach of shrinking emissions intensive sectors is said to be more efficient than aiming for green growth. Degrowth raises many questions, not least how to determine what parts of the economy are ‘valuable’ or not, and how to specifically implement deliberate shrinkage of a sector. The hunt for lifelines does not stop with this thesis, and nor should their critical examination.

7.5. Promises, Promises

Resisting the allure of new technological saviours is difficult, especially when considering the failure of emissions reduction policy thus far. Who doesn’t want to fix climate change? Yet these emerging options deserve our most stringent gaze. To work in climate is to be in the business of heartache and broken promises, and we should be careful to not create yet another round of hope and disappointment.

We particularly should be mindful that new solutions may not solve root problems. New technologies don’t fix bad politics. A lack of political ambition has bottlenecked and corrupted climate action thus far. If not addressed, the ambition bottleneck could corrupt emerging technological responses too.

But even through the lens of stark realpolitik, this much is clear: there exists a narrow corridor to effectively limit global warming and thus limit the catastrophic impacts of climate change. Extreme GGR acceleration can be a final climate hope, if coupled with rapid improvement of decarbonisation efforts. We have not yet reached the end of the line.

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Chapter 8: Author's Postscript and Acknowledgements

Whew, what a journey!

This PhD deals with some heavy stuff. Studying the end of the world certainly isn't easy. Yet the conclusion I landed on, even with all the nuances and trade-offs, was a surprisingly hopeful one. Facing catastrophic and existential threats, we must do some extraordinary things. Luckily, humanity's specialty is in doing extraordinary things. We aren't always perfect, and we have many injustices to undo, but I am convinced that a just future worth fighting for is very much in reach.

I am convinced of this because so many of our problems are self-inflicted. They are a result of short-sighted and selfish thinking. Since the problems are ours, so are the solutions. We just need to put ourselves in the position to get some extraordinary things done, and done well. John F. Kennedy created a Soviet 'missile gap' crisis to put a man on the Moon. If he could pull that off for a Moon landing, we can figure something out for climate.

Grappling with big questions like these makes for an unconventional, and admittedly impractical and difficult PhD. But I've always thought these big impractical questions were too important to ignore. My supervisors empowered me to take risks and fully throw myself at lines of investigation I truly believed were important.

Luke Kemp first inspired me to explore research. I was just a nervous undergrad when he encouraged me to make something of my own design. Luke also gave me my first teaching gig. What a cliché to say how transformative it is to have someone believe in you, but it's true. Luke has been not just a supervisor, but like a big brother. Every young person should be so lucky to have such an empowering presence in their corner.

Bec Colvin somehow always knows exactly what to say. She knows when to give me supportive encouragement and when to give me a (metaphorical!!) kick in the butt to get

me into gear. Whether in constructing the building blocks of nuanced argument, or navigating the politics of academia, there is no better mentor. Alongside Luke, Bec was one of the first to really invest time and effort into my development. I hope that the scholar and person I have become does you proud.

Katherine Daniell found every moment to support me, despite having the most vicious calendar I have seen a human endure. What other supervisor will hold an impromptu supervisor meeting on thesis structure at 8:45pm in Supabarn? Indeed, there is no one else who can compare to Katherine's intimidating talent in providing the most insightful, cogent, and mind-blowing insights in the most off the cuff and casual fashion.

Mark Howden has provided opportunity at every stage of my PhD. Mark is the reason I got to meet many wonderful people (as well as experience my first and hopefully last dose of tear gas) in Santiago, Chile; and was the reason I was lucky enough to see the UN up close in person. Related and very important: please like this photo on Instagram if you haven't already: <https://www.instagram.com/p/B8sCq3zhse4/>

Steve Dovers has supervised me since my Honours, and has taught me a fair bit about both policy and the Australian folk festival circuit.

Frank Mills, while not a supervisor on paper (I dropped Frank like a bad habit one week into my PhD), has been right by my side every step of the way. Frank was there for my notoriously chaotic Honours year, mostly to watch in horror, and has supported my development as a lecturer since then. The 1:00am email telling me to sleep in all caps is forever immortalised in my memory, in multiple backup hard drives, and in cloud storage.

I would be remis to not mention my very first academic inspiration, Semele Yuan. She put me on the path to studying Geography in university, and to this day is still the ideal educator I strive to be.

As difficult as this PhD has been, I am a product of and have enjoyed tremendous privilege. That privilege is the result of my parents, who by virtue of pure talent, and not privilege, were both first generation university graduates. Everything you have read here and everything I will ever write is a continuation of my lineage. Their sheer force of will to persevere in the most difficult of circumstances is probably where I got my own unrelenting stubbornness that allowed me to reach the end of my PhD.

I have also been lucky enough to be surrounded by a group of irreplaceable friends. Jerry has been my partner in crime both in an intense dedication to turning ambitions into reality, and in getting consistently destroyed in League of Legends. Our friendship is a reminder to balance intense academic focus with a strong dose of tomfoolery. Rachael, alongside Matt, has always been in the office when I decide that the amount of silly jokes and overpriced LeBron jerseys is too low, and the level of productivity is too high. Kristy is there to always remind me that the world isn't all work, and that life is meant to be lived (preferably wearing all pink). Lamis is a master of unhinged conversation, rivalled only by Frank Mills in the command of all caps. Rory and I will forever navigate our hate/hate friendship. Ginny, even from Sydney, somehow finds a way to lend a helping hand. Vincent Tank goes to the gym and now proudly holds the ANU's overhead press record. Caitlyn somehow makes the craziness of Australian climate policy bearable - the secret is the smiling emoji with a single tear. Gigi's dinner parties and company are best in class. Ali was a fantastic UN (and now Canberra!) buddy. Darian is still a source of silly humour and cogent life insights. Jack faithfully reminded me to complete my PhD. Last, and certainly not least, Aashna masterfully turned impending doom into a positive. Her love and support is woven into every page of this thesis (original words). I am the luckiest person in the world to somehow end up in such incredible company.

I am also grateful to the many institutions that have supported me throughout my journey.

One of my first university experiences was arriving 20 minutes late to my first Fenner School lecture. I am now completing a Fenner School PhD, right on time. I grew up at the Fenner School, and I'm grateful for the support and opportunity Fenner has given me as

an undergraduate, PhD, and as a lecturer. Katie Liesinger and Veenita Vido especially deserve thanks for their support over the years.

The Australian Academy of Science provided me with an eye-opening internship, allowing me to truly develop my practical policy chops. Lauren Sullivan and Indigo Studwicke both especially supported my practical transition into policy. I am proud to say that I have achieved the celebrity of being a Slack reaction.

The Centre for the Study of Existential Risk, found at a little-known university along the famous River Cam, has been my home away from home. It is a place where I did not need to justify looking at extreme risks (see Appendix 4), where I could truly be my academic self.

The then Climate Change Institute (now Institute for Climate, Energy and Disaster Solutions) were the first to encourage me to study greenhouse gas removal, and provided invaluable guidance as I first navigated the craziness of a life in research.

Beyond university institutions, much my writing was done in Canberra's cafes, some of the best in the world. Harvest Café likely saved this PhD after the disruption of COVID-19 (I am still very sorry for breaking one of your plants), and Idelic Café's homely family atmosphere was just what was needed as I reached the final stretch.

Lastly, to my reader, I hope you can find the explorations here insightful. It is my life's work, and a genuine labour of love. Underneath the catastrophe and chilling realism is a genuine hope to improve the world. This hope gives us purpose. It reminds us that when we feel the weight of the world on our shoulders, all we need to do is plant our feet.



Appendix 1

When looking is dangerous: How solar radiation management research can harm



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Vapes are only four percent as harmful as cigarettes, so said a [famous 2014 study](#) assessing the health risks of e-cigarettes, the first of its kind. This figure was at best an educated guess. By the authors' own admission, it lacked hard evidence.

Regardless, that number stuck. In 2015, Public Health England, an executive agency of England's Department of Health and Social Care,

used this journal article to authoritatively say that [vaping is 95 percent less harmful than cigarettes](#), encouraging the use of vapes as a transition treatment off of cigarettes. The maxim “vapes are safer than cigarettes” contributed to the meteoric growth of e-cigarette use, particularly [among young people](#).

It’s now clear that e-cigarettes pose [many health risks](#), that the chemicals in them and their power output have [changed over time and are poorly regulated](#), and that using e-cigarettes [seldom results in quitting cigarettes](#).

Those initial researchers didn’t intend to define British health policy, and they certainly didn’t intend to [market vapes to children](#). The article’s authors simply tried to perform an imperfect risk assessment of a new technology. But such is the nature of research. Ideas, even if initial and incomplete, can cascade into repugnant actions. The path ahead contains a similar flashpoint for a far more impactful set of risky technologies intended to address climate change.

To research, or not to research. Solar radiation management involves deliberately reflecting sunlight back into space to cool the Earth. It’s intended to avoid the worst impacts of global warming and buy time for implementing renewable energies and removing greenhouse gases from the atmosphere. The solar radiation management technique of most interest is stratospheric aerosol injection, which involves using fleets of planes or balloons to inject particles in the atmosphere to slightly whiten the sky.

The rationale for researching such technologies is understandable. Responding to climate change requires as large of a toolbox as possible, and in a hypothetical deployment, decision makers could be helped by understanding solar radiation management risks and how to respond to them. Research *can* inform effective decision making. The operative word is “can.” Robust analysis and information can help decision-making, but certainly not always. Research can [also harm](#).

However, the risks of research are not always communicated by solar radiation management research advocates. A [2023 open letter](#) in favor of research emphasized transparency and independence, but not the risks of misrepresentation or normalization. Prominent researchers who

argued that “[climate engineering research is essential to a just transition and sustainable future](#)” similarly do not also flag the risks of research.

And given that research is already underway—sometimes [field testing without sufficient oversight](#) (and where the researcher in question simply bemoaned leakers “led astray by diabolical temptation” when contacted by the press)—understanding research risks is especially important. Some types of research can be useful. Others can dangerously mislead.

Welcome to model land. Idealized climate models, in particular, can erroneously misrepresent stratospheric aerosol injection as technology that can be centrally “designed” and easily deployed. Much of the science so far emphasises how a well-designed deployment could lessen the risks of modifying the Earth’s atmosphere.

Yet the design of stratospheric aerosol injection is often Panglossian. The models investigate the impacts of injection if implemented in a consistent manner over decades, sometimes flawlessly coordinated to only use a [smaller amount of injection](#) or slowly increase injections focused [15 degrees south of the equator](#).

In these highly optimistic outlooks lie assumptions that there would be perfect international coordination on how much, how quickly, and where to inject what substance, *and* that these decisions would be consistent over many decades. Given that this assumption contradicts the foundations of international politics and policy, this imagined version of solar radiation management isn’t of core relevance.

More realistic and [messy deployments](#)—driven by the rhyme, rhythm, and chaos of geopolitics and electoral cycles—are of more interest. Continuously presenting an idealized fantasy version of solar radiation management can make it seem like a better option than it actually is.

These aren’t unfounded future-focused concerns. There is a strong track record of idealized modeling damaging climate ambitions.

DICE—Dynamic Integrated Climate-Economy, a prominent climate economic model—assumes that industries which are indoors won’t be

affected by climate change, and requires the modeler to put forward a subjective percentage of how much less the future is worth compared to the present. These assumptions result in severe underestimation of climate risks, contributing to delays of climate action. Researchers should be mindful to avoid similar mistakes for solar radiation management.

Models are grainy snapshots of the world. Despite their imperfections, however, models often define the corridors of discussion. Research dominated by idealized, best-case versions of solar radiation management would be a poor guide for handling the complex risks at play.

Policy playhouse. Similar simplifications occur in policy research. Policy research can at times overly focus on analysis of effective policy instruments and structures. But coming up with abstracted policy instruments has limited usefulness unless those instruments can be aligned with a political window for adopting them. Time and time again, initially good policy ideas have been warped and refracted by political voices and political interests, until what is eventually implemented is a shell of its potential.

Carbon markets are, for instance, *in principle* the most efficient tool to reduce emissions. However, without the political will to create stringent rules and safeguards, carbon markets can actually increase emissions. (This happens if carbon offsets which do not reduce or remove emissions are used to extend or expand fossil activity.)

Pledge and review—the strategy of putting forward non-binding targets for collective review—is another example. This process *can* be an effective design for international cooperation. The World Health Organization used pledge and review in its successful campaign to combat tuberculosis.

But its use in climate policy has simply become another avenue for countries to drag their feet. There was stronger political consensus with tuberculosis responses than with climate change. That's because responding to that infectious disease didn't require grappling with vested interests and energy politics. The Paris Agreement's pledge and

review system bypassed such political firewalls by letting countries do whatever they want, locking in inadequate incrementalism.

The best available analysis of carbon markets and pledge and review couldn't overcome the political forces that corrupted them. If an idea is good in theory, but ultimately poor in practice, it's a bad idea.

Simply creating abstracted policy instruments and structures can present solar radiation management as more governable than it actually is. The name of the game is not just developing our understanding of what effective governance instruments or structures would be (the focus thus far). Rather, researchers and policymakers must determine effective governance that would work *within* a system of political compromises and self-interests.

Complex politics can raise another core objection to research. There seem to be insurmountable political barriers (namely, political and policy consistency over a hundred years) that would make effective endgame deployment dead before arrival. Better climate models and discussions of effective policy cannot fix realpolitik. This would seem to make research a non-sequitur. If a desirable endgame is out of reach, what is the purpose of research?

Slipping away. Simplifying the science and governance related to solar radiation management is especially important given concerns that research is a first step on a slippery slope to eventual deployment.

But some are dismissive of these concerns, saying that a slippery slope is just as likely for other technologies, and that the experience of research in this area thus far has been social resistance and controversy. So, they say, where's the slippery slope?

These rebuttals miss the point. A slippery slope may be just as likely for other technologies, but other technologies don't directly control Earth's climate. Furthermore, just because there is resistance now does not preclude changes in the future, particularly if researchers continue to present a best-case, idealized vision of solar radiation management. Opinion, politics, and policy can, and will, change.

Most important, different people inconsistently use the notion of a slippery slope to mean different things.

The slippery slope is not one single nebulous concept. It's best understood as the idea that initial actions can spark many different non-linear feedback loops. These feedback loops can be technological, social, political, hype-based (akin to the hype and funding of Theranos or Juicero), or incrementally expand field tests to at scale deployment (though this is less likely for stratospheric aerosol injection).

Worsening risks of a slippery slope to a general deployment of solar radiation management is a disconnect between those who research and those who deploy. Decision makers will not be scientists and researchers, but more likely politicians and government officials. While decision makers could be partially guided by science and evidence, they will almost certainly be driven primarily by political interests. If researchers correctly say that solar radiation management shouldn't be a replacement for emissions reductions, that doesn't guarantee solar radiation management will not be used as a substitute for emissions reductions. Researchers could spark feedback loops that will ultimately be out of their control.

But while there are many risks in conducting this research, abandoning the study of solar radiation management might not be the route to take either. There genuinely could be the chance that some research helps inform good policy, reducing the risks of dangerous deployment. For instance, the research community for nuclear weapons was critical in arguing for what eventually became the 1972 Anti-Ballistic Missile Treaty.

However, research certainly is not risk free. Researchers do not passively create a bank of knowledge for the world to draw from. They shape ideas and the way that issues are discussed. Presenting idealized best cases, both in science and policy, can leave the world ironically unprepared for the worst that could come.

This article is available online at: <https://thebulletin.org/2023/06/when-looking-is-dangerous-how-solar-radiation-management-research-can-harm/>

Appendix 2

Trying to cool the Earth by dimming sunlight could be worse than global warming

A group of 60 scientists called for a moratorium on solar geoengineering last month, including technologies such as stratospheric aerosol injection (SAI). This involves a fleet of aeroplanes releasing aerosol particles – which reflect sunlight back to outer space – into the atmosphere, cooling down the Earth.

SAI might make the sky slightly whiter. But this is the least of our concerns. SAI could pose grave dangers, potentially worse than the warming it seeks to remedy. To understand the risks, we've undertaken a risk assessment of this controversial technology.

A cooler Earth means less water would be evaporating from its surfaces into the atmosphere, changing rainfall patterns. This could produce ripple effects across the world's ecosystems – but the exact nature of these effects depends on how SAI is used. Poor coordination of aerosol release could lead to extreme rainfall in some places and blistering drought in others, further triggering the spread of diseases.

SAI could also make natural catastrophes worse than they currently are. A volcanic eruption, like that of Iceland's Eyjafjallajökull volcano in 2010, could naturally cool the Earth as plumes of ash block sunlight from reaching the planet's surface. If this happened while SAI was deployed, it would have to be urgently adjusted (not an easy feat) to avoid overcooling one hemisphere and producing extreme weather patterns as a result.

Similarly, although nuclear war may seem unlikely, global nuclear capabilities continue to grow, and bad political decision-makers are in no short supply. A "nuclear winter",

during which global temperatures drop for years due to soot clouds from nuclear-triggered fires, could be deepened by SAI.



The consequences of SAI could be far worse than a whitened sky. Negative Space

Termination shock

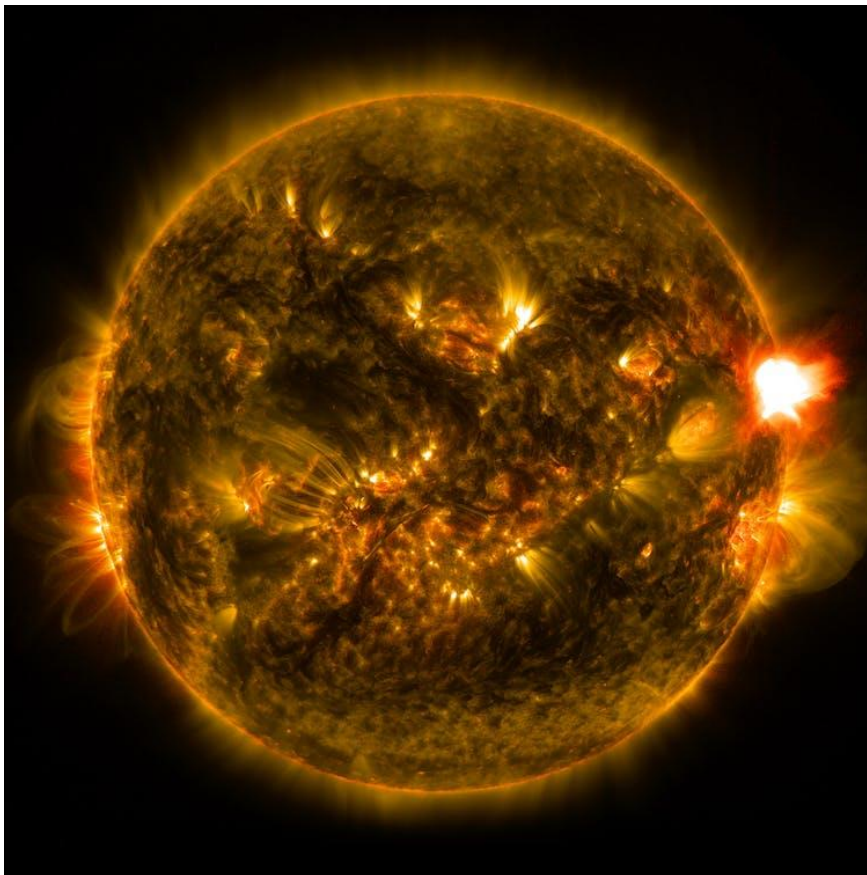
SAI would likely rely on aerosols being consistently sprayed into the atmosphere by a fleet of aeroplanes, as the particles have a half life of approximately eight months. Satellites would be needed to coordinate these efforts and help monitor any atmospheric changes.

Any disaster severe enough to permanently disable these systems could trigger a “termination shock”. If an SAI system effectively “hiding” global warming were suddenly removed for an extended period, the Earth could heat up by multiple degrees in a matter of decades. If we’re already seeing fires, heatwaves, and flash floods across the world with around 1.1°C

of warming since 1850, just imagine what warming of 3-4°C would do.

There are numerous ways in which an SAI system could be disrupted. An unprecedented explosion of solar matter, related to a solar flare, could knock out the world's electrical systems by smashing into the Earth's magnetic field. This could damage the aviation and satellite systems needed for SAI.

Hoping that catastrophes will simply not occur in the coming century would also be a mistake. One model estimating the likelihood of nuclear war between Russia and the US puts that probability at 0.9% per year. Estimates of large-scale space weather events range from 0.46% to 20.3% per year.



Unexpected solar activity could knock out SAI systems. NASA_Goddard/Flickr, CC BY-SA

SAI could also be an attractive target for cyberattacks. In 2019, a group of hackers named DarkSide took the US oil company

Colonial Pipeline hostage by launching a ransomware attack on their computer systems. Fearing widespread fuel shortages across the US, operators were forced to pay £3.7 million to DarkSide in exchange for reactivating their systems.

And in 2000, the automated sewage system in the small coastal Australian region of Maroochy released hundreds of thousands of gallons of sewage into the sea. These “leaks” were actually caused by a single disgruntled ex-employee of the company that installed the system. An international infrastructural system masking global warming would attract more reasons for controversy, have a larger workforce than a local sewage system, and could likely fetch an even higher payoff.

Political mess?

Of course, it’s possible that SAI will end up being used responsibly. But if one thing goes sufficiently wrong – such as one unpredictable solar storm taking place – the hidden risks of SAI could be unleashed. Predictions of SAI’s average or “most likely” outcomes are generally fine. But although far less likely, SAI’s worst case scenarios could be calamitous.

If SAI is used sparingly to offset a smaller amount of warming, any negative impacts would be minimised. Most SAI models assume ideal conditions, where a cooperative group of countries rationally and carefully deploy SAI. Unfortunately, international politics is messy.

A small group of countries that prefer a cooler Earth could start to use SAI without international agreement. Yet there is little research on what the effects of this more disorganised use of SAI might be.

In an ideal world, those governing SAI would ensure that its infrastructure is resilient against catastrophes, operated cooperatively between countries, has extensive backups and is

closely monitored for the duration of SAI deployment (likely decades and potentially over a century). And to ensure we don't get trapped into relying on SAI indefinitely, we'll still have to reduce greenhouse gas emissions to net zero, as well as removing excess emissions from the atmosphere.



Poor governance during the pandemic may leave low hopes for SAI governance in the future. VP Eremen

But assuming this kind of governance would be naive. Just consider the pandemic. From underinvesting in COVID testing and vaccine development to placing misguided trust in herd immunity, policymakers have not proven reliable decision-makers. Imagine the conflict over placing a chemical mask over the Earth.

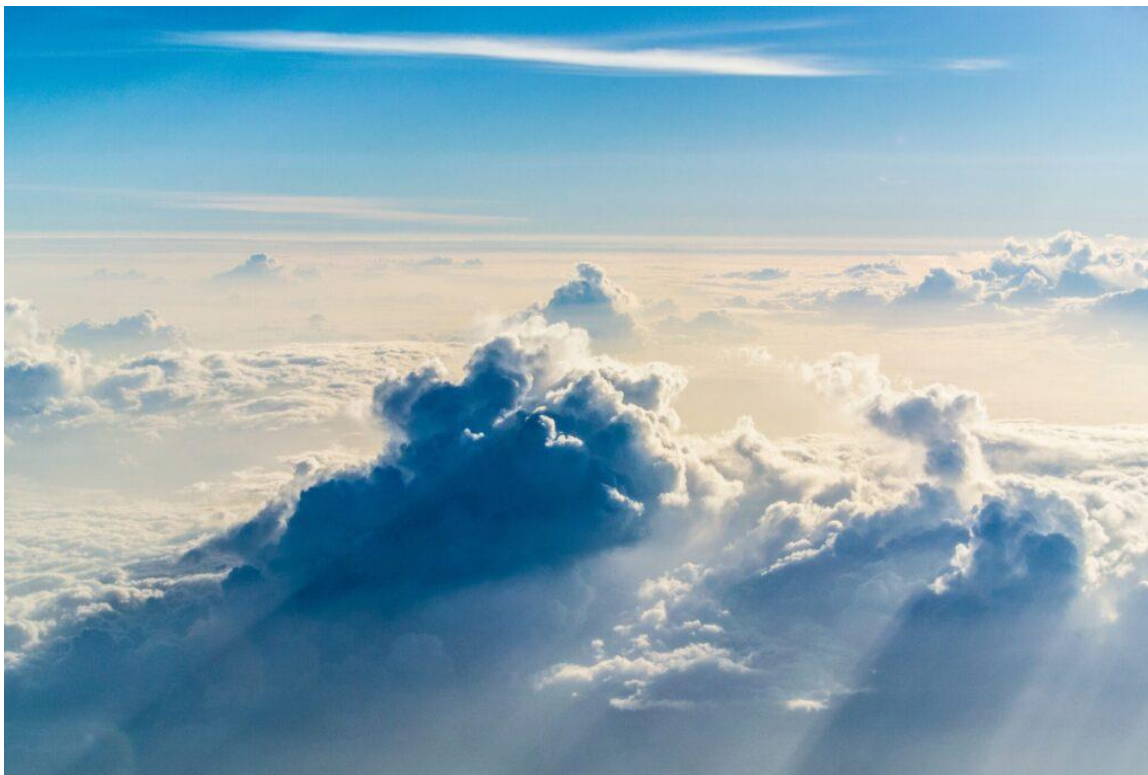
SAI could become a highly politicised issue, with changes in SAI use driven by political swings rather than sound science. And the fossil fuel industry and its supporters may well develop a vested interest in using SAI to delay the use of renewables.

Is SAI worse than climate change? We're still uncertain. What we can say is this: in a world where things don't go wrong, SAI is a prudent response to the climate crisis. But we live in a world of complexity and chaos, where relying on SAI would be deeply unwise. By tightly coupling the climate system to the global economic and political system, using SAI would be hoisting up a planetary Sword of Damocles.

This article is available online at: <https://theconversation.com/trying-to-cool-the-earth-by-dimming-sunlight-could-be-worse-than-global-warming-175455>

Appendix 3

Dead before arrival: The governance of stratospheric aerosol injection



Clouds in the stratosphere (Photo by [Kaushik Panchal](#) on [Unsplash](#))

In the wake of [six destructive hurricanes between 1954 and 1955, and billions of dollars in damages](#) along the East Coast, the United States Government began conducting field tests with the aim of weakening tropical cyclones. Called Stormfury, the project was based on the idea that [seeding clouds](#) with silver iodide could reduce cyclone intensity. Science eventually showed the hypothesis was incorrect and seeding the clouds didn't work to weaken the structure of hurricanes.

But the mechanism's ineffectiveness [didn't stop Fidel Castro of accusing the United States of weaponizing hurricanes](#). And it didn't stop some communities in Florida and the Gulf Coast who were hit by Hurricane Betsy in 1965 [from blaming Stormfury for the damage](#).

(Scientists originally had thought of seeding the storm but decided against it. This caused the media to incorrectly report the facts, leading to confusion.)

Although both Castro and the communities affected by Betsy were not correct in those instances, their concerns were valid. If the flap of a butterfly's wings could theoretically snowball into a hurricane weeks later, as attributed to mathematician and meteorologist Edward Norton Lorenz, what effects could altering an atmospheric component have on communities near and far?

Stormfury ended in 1983. Since then, average global temperatures have risen 0.32° F (0.18° C) per decade. And with years of messy politics holding back effective climate action, scientists are once again revisiting ways to alter Earth systems. One of these is stratospheric aerosol injection, a type of solar radiation management that would mimic the cooling that occurs after volcanic eruptions. The idea is to inject materials such as sulfur into the stratosphere, to make particles that reflect sunlight back into space and cool the Earth. And since aerosols in the stratosphere have a limited lifespan, such aerosol injections would have to be renewed on an approximately annual basis for a long time.

But what Stormfury and other scientific endeavours have repeatedly shown is that idealized science does not easily overcome messy realpolitik.

Stratospheric aerosol injection would be an extraordinarily dangerous way to attempt to lessen the impacts of global warming—one that most climate scientists roundly warn against. But if leaders around the world ignore those warnings and proceed with a stratospheric injection program or other solar radiation management initiative, that program or initiative will need to be managed. And history shows that environmental policy regulation is insufficient and unreliable as a means of controlling any technology with the catastrophically high-risk profile that stratospheric aerosol injection carries.

What would deployment governance look like? Even an initial and very incomplete canvas of governance for a stratospheric injection program

paints a demanding picture, raising questions like who gets to control the world's climate and how they would go about it.

At a fundamental level, decision-makers would have to determine how to best ensure broad international representation and democratic processes. They would need to agree on who gets to make deployment decisions, and how. This would pose the difficult question of how to govern not only countries, but also decentralized private-sector actors. Such actors do not have a mandate to act in the public interest and instead could be driven by profit. One early example is Make Sunsets, a start-up that is controversially selling "cooling credits" to release particles into the atmosphere.

Decision-makers would then have to determine deployment objectives and configurations. How much warming should stratospheric aerosol injection moderate, and what ecological side effects can be mitigated? Where should injections therefore happen, what substances should be used, and how much of these particles should be released?

A critical related issue is making sure deployment is not used as a perpetual offset or in attempts to make "designer" climates. Some might argue that stratospheric aerosol injection is too uncertain to deliberately make optimal regional climates, but decisions (and political promises) are often made based on incomplete information, poor knowledge, and misguided intentions. Such misguided intentions are especially dangerous when deliberately modifying the entire climate.

Furthermore, a robust international system for monitoring and evaluation of global and regional ecosystems is necessary to identify and address side effects. This optimistically assumes that ecological consequences will always be clearly measurable. And given solar radiation management's potentially enormous negative side effects, decision-makers may be called to develop accountability and liability mechanisms (again assuming that clear attribution of ecological impacts of stratospheric injection is even possible).

Most important, absent a quick and enormous breakthrough in carbon dioxide removal technology, all of these policy elements would have to be maintained for an exceedingly long time—quite possibly a great deal longer than a century. Continuous stratospheric aerosol injections

would last for however long it takes to reduce greenhouse gas emissions to zero and to remove excess greenhouse gases from the atmosphere. Its associated policy and regulation regimes would need a similarly long life.

The fairy tale ends. Such policy consistency is a pipe dream, as evidenced by human history. Societies, politics, and governments change. An election won or lost can be the birth, death, and resurrection of disparate policy approaches. Donald Trump won the American presidency in January 2017 and announced withdrawal from the Paris Agreement five months later. Joe Biden rejoined the treaty on January 20, 2021, his first day in office.

Inconsistency is unfortunately a part of the core nature of policy. This is especially the case for intensely political issues, which stratospheric aerosol injection would certainly be. A reasonably well-governed initial deployment could be undone by political shifts. Solar radiation management with whiplash-inducing policy shifts does not bode well for effective implementation—or Earth's climate.

The needed policy depth, while not strictly impossible, seems improbable given the difficulties of international governance. For example, one option is to house stratospheric aerosol injection governance within the UN climate system. An amendment to the Paris Agreement or a new post-Paris treaty could include such governance provisions. However, the UN climate system is notoriously ineffective. It works by consensus—each country essentially has a veto vote. Any policy that gets passed is heavily watered down. This does not bode well for robust and stringent governance required for technologies as potentially catastrophic as solar radiation management.

Some may suggest alternative UN bodies or organizations for a governance home, like the UN Environment Programme (UNEP). But such bodies would also run into similar consensus related issues. Where broad consensus is present, robust policy cannot be.

An alternative would be to work in a treaty system with exclusive participation. Instead of being broad and shallow, stratospheric aerosol injection governance could be deep and narrow. The downside is that this could mean a lack of adequate representation. A small number of

countries taking it upon themselves to engineer the world's climate would run counter to principles of democracy and create an almost certain backlash among countries that have no say in the process.

Power and politics define the rhyme and rhythm of international governance. Ultimately, the specific governance approach and structure for a stratospheric aerosol injection program would not be the remit of a group of well-informed and well-intentioned academics. Politics define policy windows, and thus policy discussions.

For instance, housing the governance of aerosol injections would not be solely based on rational calculations of policy effectiveness. Countries will “forum shop,” intentionally choosing which structure best fits their interests. For example, initial discussions of governance at the UN Environment Assembly in 2019 met [immediate political roadblocks](#), with some countries, including the United States, preferring the Intergovernmental Panel on Climate Change (IPCC). Even the creation of the IPCC itself, *jointly* by the UNEP and World Meteorological Organization, was driven by the [United States wanting to limit UNEP's influence](#) over the emerging international climate policy landscape.

Similarly intense political drivers can be seen in specific policy design as well. For example, The Paris Agreement uses a “pledge and review” approach; its main commitment for countries is putting forward, not meeting, a new climate target every five years.

Paris' pledge and review wasn't chosen because countries thought it the most effective approach to reduce emissions. It was used because the lack of substance allowed [the United States to join the Paris Agreement via presidential executive order](#). This intentionally bypassed a heavily deadlocked United States Senate that would never ratify a climate treaty. The world essentially traded policy substance for American participation.

Good science and good policy proposals do not fix ugly politics. Rather, ugly politics taint what they touch.^[i]

Imperfect governance. Because of power and politics, most international treaties fail to meet their stated goals (as found by a [recent synthesis of more than 200 studies](#)). Flawed governance is an

acceptable and manageable risk for some policy areas. Carbon markets, for instance, tolerate a level of imperfection, with up to 20 percent of carbon credits usually accepted as flawed, in order to incentivize additional climate action overall. Furthermore, just having a treaty in place can affect social and political norms, even if its substantive core is lacking.

But soft governance is simply not acceptable for high-risk technologies like solar radiation management initiatives. Stratospheric aerosol injection with deeply flawed governance would be like hoisting a climatic Sword of Damocles—it may allay climate risks in the short term, but when disaster strikes (and over several decades, one will) it would be catastrophic.

As technological capabilities grow, the margin of error for effective governance shrinks. In the mid 1900s, humans were attempting to affect a specific weather event—cyclones. With stratospheric aerosol injection, they'd be affecting the entire world's climate. The risks are high, and that makes the required bar for effective governance even higher.

Given humanity's political track record, that bar seems out of reach.

This article is available online at: <https://thebulletin.org/2023/03/dead-before-arrival-the-governance-of-stratospheric-aerosol-injection/>

[i] As a counterpoint, some may point to our global aviation system as an indication that complex, international, and rather technocratic governance is possible. However, one must also consider the political context that enabled and drove such efforts. By the end of World War II, the United States had developed substantial advantages in its aviation infrastructure. They had an economic interest in aggressively expanding the global aviation market, against the wishes of less wealthy countries who wanted to protect their own aviation industries. It was also a time of historical power imbalances. America's only rival was the Soviet Union, and the United States still had an economic and technological advantage. The world today, while still deeply unequal, is far more multi-polar. At the time, a complex international governance system was only

possible because a powerful country was willing, and able, to force it into existence.

Appendix 4

Below is a creative writing exercise as part of an intensive visitor's programme at the Centre for the Study of Existential Risk at the University of Cambridge.

Even though he had weak child fingers which couldn't press the piano keys properly, 6 year old Aaron kept trying. His teacher always believed in him. Months of progress saw continuous improvements. Until one day Aaron vomited all over the piano and the teacher quit. It was at this moment Aaron understood the importance of extreme unlikely events.

