On thin ice: Solar geoengineering to manage tipping element risks in the cryosphere by 2040

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April 18, 2024

Abstract

Tipping elements are features of the climate system which can display self-reinforcing and non-linear responses if pushed beyond a certain threshold (the “tipping point”). Models suggest that we may surpass several of these tipping points in the next few decades, irrespective of which emissions pathway humanity follows. Some tipping elements reside in the Arctic and Antarctic and could potentially be avoided or arrested via a solar geoengineering program only at the poles. This paper considers the utility of proactively developing the capacity to respond to emergent tipping element threats at the poles as a matter of risk management. It then examines both the air and ground infrastructure that would be required to operationalize such capability by 2040 and finds that this would require a funded launch decision by a financially credible actor by roughly 2030.

1. Introduction

Despite the rapid growth of solar and wind power along with advancements in battery technology capabilities to store such energy, demand for fossil fuels remains undiminished¹ and greenhouse gas emissions continue to increase². Without rapid and dramatic changes in our emissions trajectory, humanity may surpass a 1.5°C global average surface temperature anomaly by 2030 and 2°C by mid-century³. Such a temperature trajectory would not only be damaging in its own right, but would risk triggering self-reinforcing transitions in various tipping elements of the climate system⁴–⁹, which in turn could dramatically accelerate climate damages.

The potential urgency of devising responses to tipping elements is illustrated in Table 1, which is derived from Armstrong McKay et al. (2022). The minimum estimates of the global mean surface temperatures that would trigger many of these tipping elements are in many cases well below 2°C, and expected tipping temperature values for key tipping elements such as abrupt permafrost thaw and subpolar gyre (SPG) collapse are also below 2°C⁵. These tipping temperature thresholds will be surpassed irrespective of which Representative Concentration Pathway (RCP) the world ultimately selects. Even diverting to a radical emissions reduction scenario such as RCP2.6 would not prevent us from crossing before 2050 the minimum temperature threshold that might trigger a tip of the Atlantic Meridional Overturning Circulation (AMOC)⁵. It is possible that we have already crossed the thresholds that might lead to an irrecoverable loss of the West Antarctic or Greenland ice sheets⁵. Or, those thresholds may lie just ahead.
In all events, these circumstances emphasize how imprudent it is to rely upon emissions cuts as our sole means of defense against tipping elements. Mitigation remains a plausible defense against triggering tipping elements whose thresholds lie in the latter part of this century or the next, but if thresholds lurk in the next several decades and before the world’s net zero emission point, mitigation is likely to be an ineffective means of avoiding them. We would remain on a collision course with climate disasters that could unfold in a non-linear fashion and evade any subsequent efforts to control them. That would portend runaway climate change.

2. SAI and tipping elements

A growing body of literature supports the assertion that a global stratospheric aerosol injection program could be effective in delaying or avoiding many tipping points. This would suggest a distinct rationale for the commencement of global SAI – not for the direct purpose of reducing global mean surface temperatures to avoid heat-related damages, but rather to avoid tipping thresholds until mitigation or carbon capture could obviate the need for further solar geoengineering. This is a twist on the common conception that SAI may be a means to “buy time” before other interventions can kick in.

However, this too may prove too late. While global SAI is often portrayed as a possible “emergency” climate intervention, humanity should anticipate a roughly two-decade interval between a funded launch decision and the achievement of substantial global cooling on the order of 0.5°C or 1°C. Such a program would require a fleet of several hundred large-payload, high-altitude jets of a type that does not yet exist. Developing such a prototype aircraft along with sufficiently powerful engines would consume roughly a decade, and then manufacturing the fleet would consume a second decade. Limited and ramping deployment could begin at the beginning of the second decade, but only when the fleet is complete would the target level of cooling be reached.

Moreover, this does not imply that we can expect global SAI capability ~20 years from today. What would commence this countdown to achieving a cooling target would be a “funded launch” – a firm contractual commitment by a financially capable actor to spend tens of billions of dollars to purchase a large fleet of new jets from one of the world’s major airframers. A roughly equivalent sum would also be required for ground
infrastructure – principally airports and aerosol management facilities. Such a spending spree can only be underwritten by major governments, whose approval or consent would also be required to authorize such a program. No government of any size is known to be even considering embarking on such a program, so a funded launch of a global SAI endeavor is not a near-term prospect.

Given the immensity of the technological, scientific and governance obstacles that would need to be overcome before undertaking such a launch decision, it is our subjective view that a funded launch decision seems all but impossible in the coming decade and improbable in the subsequent one. To draw an admittedly arbitrary but no longer flatly implausible line in the sand, let us imagine that it is the crossing of the 2ºC GMST threshold in ~2050 that finally galvanizes a funded launch decision. That would enable the commencement of a gradually ramping deployment program starting in ~2060 and the achievement of the cooling target by ~2070. So long as no thresholds had been transgressed before 2070, humanity would thereafter have the ability not only to cool the planet generally, but to ward off tipping elements thereafter. We would have successfully crossed the abyss.

But this would leave a task undone. If there are any tipping points lying in our path before 2070, we would be destined for a head-on collision. This begs the question as to whether there are any tools that could reduce our risk of triggering tips before 2070. Fortunately, the answer appears to be – perhaps.

An alternative to the global “peak-shaving” conception of SAI is a program that would target just the poles. Aerosols injected into the stratosphere equatorward of the Arctic and Antarctic circles would be carried poleward by the Brewer Dobson Circulation, fitting a parasol over the top and bottom of the earth. Such a sunshade appears likely to slow or prevent the deterioration of high-latitude tipping elements such as the AMOC, the SPG, and the ice sheets covering the Greenland and the West Antarctic. It could ward off abrupt permafrost melt and prevent Arctic winter sea ice loss. Though not tipping systems, gradual permafrost melt and September Arctic sea ice loss can also be ameliorated by a polar SAI program. This would provide humanity with a direct defense against colliding with some, though not all, of the tipping thresholds that may be encountered in the middle of this century.

We posit here another line in the sand, though we perceive this one to be less arbitrary. It would be prudent for humanity to arm itself to defend directly against tipping high latitude tipping elements by 2040. We choose this temporal target not because no protection is required sooner, but rather because we judge this timescale to be as soon as is reasonably possible. Later is certainly plausible (much more so, in fact), but sooner is not. Therefore, from here, we proceed from a simple question, which is “if humanity were to seek by 2040 the capability to respond within a year to a high-latitude tipping point emergency, what would we need to do between now and then?”

We will first describe in greater detail the deployment scenarios that might be responsive to the sorts of climate emergencies we can envision at the poles. We will next consider the air infrastructure that would be required to mount such responses, and subsequently, the required ground infrastructure.

### 3. Polar solar geoengineering deployment scenario

In describing a deployment scenario that might prevent or delay crossing tipping thresholds at the poles, we start with the sub-polar focused program described in Smith et al. (2022), which was characterized as follows:

a) Temperature anomaly target: Annual average surface temperature reduction of 2ºC for the area between 60ºN and the North Pole

b) North/south symmetry: Any deployment at one pole must be countervailed by a roughly equivalent deployment at the opposite pole

c) Injection seasonality: March through June in the north, and September through December in the south

d) Injection locations: At latitudes of roughly 60ºN and as close to 60ºS as logistics allow
Several elements of the above program merit further discussion or modification here.

a) While an average annual surface temperature reduction of 2°C was chosen for Smith et al. (2022), there was no suggestion that this level of cooling was in any way optimal or fit for purpose. Moreover, in the current paper, we note many different cryospheric hazards to which we might potentially seek to respond, and the level of cooling that may be optimal to suppress one hazard is unlikely to be the same as might be warranted in respect of another. Further modeling beyond our intent here will be required to calibrate cooling objectives to particular hazards. Nonetheless, a -2°C annual temperature change in the area north of 60°N would be a very substantial cooling, particularly since the seasonal imbalance would concentrate most of the cooling in the summer months.

b) As conceived here, the trigger for an SAI response would be convincing data suggesting that a particular high-latitude target tipping element was approaching its threshold, but any large-scale SAI response at one pole must be countervailed by a roughly equivalent one in the opposite hemisphere. This is because a program in just one hemisphere would substantially shift the location of the Intertropical Convergence Zone (ITCZ), which in turn materially impacts rainfall patterns in the tropics. So as not to disturb agriculture and ecosystems in the most densely populated parts of the world, any polar SAI program must be bi-polar and roughly symmetrical. Symmetry could be conceived as meaning the same deployed mass in each hemisphere, or the same cooling impact, or such ratio of hemispheric distribution as creates the least disturbance to the ITCZ. Noting these prospectively different definitions, we will assume herein that equivalent masses are deployed in each hemisphere.

c) Given the extraordinary seasonal imbalance of insolation at the poles, one would intervene only in the sunny months and not in the dark ones. Building on conclusions reached in Lee et al. (2021), one would inject only in the spring/early summer months, such that the aerosols are aloft when the days are long and the insolation is at its maximum. While aerosols vented above the tropics would have an atmospheric lifetime on the order of a year or more, aerosols injected at 60°N/S would endure for less than half that time, meaning the particles injected in the spring would have mostly fallen out of the air by autumn.

d) In respect of injection locations, latitude matters greatly but longitude seems not to substantially impact deployed aerosol efficacy, given the efficient east/west mixing provided by the earth’s rotation. Since the hazards we seek to manage reside in the high latitudes and since material vented into the lower stratosphere flows mostly poleward, high-latitude injection locations are sensible for this purpose. In the Arctic, a sunshade from 60°N northward would shield all of Greenland, the Arctic Ocean, and most of the permafrost regions from the most intense summer sunlight. Likewise, all of Antarctica lies south of 60°S. Bases near 60°N are plentiful, with each of Anchorage, Oslo, Stockholm, Helsinki, and St. Petersburg being proximate. On the other hand, the southernmost widebody-capable airports in the world are at the tip of Tierra del Fuego, lying only 54 degrees south of the equator. These are less ideal than their northern counterparts, but will have to do.

e) An injection height of 13 km is chosen based on the height of the tropopause at deployment latitudes, noting that aerosols must be injected above the tropopause to retain sufficiently high residence times in the stratosphere. Compared to our proposed program, the principal source of difficulty in respect of tropical deployment is that an altitude of roughly 20 km is required to access the lower stratosphere in these regions, requiring the development of novel aircraft. Latitude is the primary determinant of tropopause height, and fluctuations due to seasonality, longitudinal, and interannual variability are relatively minor. During our proposed injection periods, the tropopause sits at roughly 10 km altitude at 60°N and 9 km altitude at 60°S. Tropopause heights at a deployment latitude of 54° in the Southern Hemisphere rise by...
only about 1 km compared to $60^\circ S$, suggesting that deployment from $54^\circ S$ is an acceptable alternative. To all of these altitudes, we add a 3 km buffer to account for tropopause abnormalities and minor seasonal, longitudinal, and interannual variation, arriving at an injection height of 13 km. However, tropopause height variability may mean that on certain days in the deployment window, an abnormally high tropopause may render deployment ineffectual. We have reduced dispatch reliability by 5% herein to account for this, noting that is simply a plug number intended to take account of this issue, subject to further refinement.

f) While other forms of sulfur are possible and perhaps preferable in deployment schemes, and non-sulfur aerosols would avoid interfering with the recovery of the ozone layer particularly in the Antarctic, we have for simplicity retained the most common assumption, which is that our program’s aerosol is SO$_2$ vented as a gas. After a few weeks, SO$_2$ would oxidize to H$_2$SO$_4$, in which state it would be effective at deflecting the intense summer sunlight at the poles.

g) Sourcing from Lee et al. (2021) and assuming linear scaling, 6.7 Tg-SO$_2$/yr was determined to be required in the Arctic in order to reduce temperatures by 2°C. An equivalent deployed mass was assumed in the Antarctic. We reiterate that the resulting aggregate deployed mass of 13.3 Tg/yr should be perceived as an order-of-magnitude first guess, subject to substantial further refinement.

4. Air infrastructure

The lower altitude required for high-latitude aerosol injections is a game-changer in respect of aeronautical infrastructure. Unlike tropical injections that would require novel high-altitude aircraft with minimal payloads, sub-polar injections could be performed with existing aircraft designs, because the 13 km altitude requirement (roughly 43,000 feet) is at or near the service ceilings certified for many modern jetliners. Moreover, contrary to the conclusions reached in Smith et al. (2022), we conclude here that some existing aircraft would be well-suited to this mission and that purpose-built novel aircraft would not be required. This removes one substantial task from the infrastructural build-out necessary to undertake such an intervention.

Smith et al. (2022) examined the capabilities of existing air-to-air refueling tankers for polar SAI deployment given the mission similarities, since in both cases the planes are tasked with lofting dense loads of fluids into the air. However, the aging aircraft designs used for air-to-air refueling caused these aircraft to perform poorly at 13 km, suggesting that purpose-built SAI aircraft would be substantially more economical despite the required upfront investment in a new design. In this study, we have analyzed the capabilities of the most modern freighters either in production or on the drawing boards at Boeing and Airbus and find those to be much more capable than the tanker platforms and well-suited to a 13 km deployment mission. In particular, our analysis suggests that a modified variant of the 777F (a “777 Special Tanker”) could haul a whopping 110.6 metric tonnes (243,750 lbs) to the target altitude, which is roughly 3 times the payload of the tankers Boeing and Airbus are currently delivering to air forces around the world and over 1/3 more than the payload of the “SAIL-43k” bespoke deployment aircraft conceived in Smith et al. (2022). For the task of developing an emergency response capability to tipping point phenomena at the poles, the 777F appears to be an excellent starting point.
Figure 2: Altitude and payload capabilities for candidate aircraft for a polar solar geoengineering program at an altitude of 43,000 feet (~13 km).

<table>
<thead>
<tr>
<th>Service Ceiling (ft)</th>
<th>Maximum Take-off Weight (lbs)</th>
<th>Take-off Weight for 43k ft (lbs)</th>
<th>Net Payload @ 43k ft (lbs)</th>
<th>Net Payload as % of TOW for 43k ft</th>
<th>Derivation</th>
</tr>
</thead>
<tbody>
<tr>
<td>A330 MRTT</td>
<td>42,700</td>
<td>514,000</td>
<td>357,105</td>
<td>88,300</td>
<td>25% Converted A330</td>
</tr>
<tr>
<td>KC-46 Pegasus</td>
<td>40,100</td>
<td>415,000</td>
<td>256,500</td>
<td>74,870</td>
<td>29% Converted 767</td>
</tr>
<tr>
<td>777F</td>
<td>43,100</td>
<td>766,800</td>
<td>604,015</td>
<td>243,750</td>
<td>40% Current Production Freighter</td>
</tr>
<tr>
<td>777-8F</td>
<td>41,800</td>
<td>805,000</td>
<td>610,000</td>
<td>220,794</td>
<td>36% Basis of Future Freighter</td>
</tr>
<tr>
<td>747-8F</td>
<td>43,100</td>
<td>987,000</td>
<td>800,000</td>
<td>313,798</td>
<td>39% Out of Production Freighter</td>
</tr>
<tr>
<td>A350-1000</td>
<td>41,459</td>
<td>696,661</td>
<td>482,000</td>
<td>102,500</td>
<td>21% Basis of Future Freighter</td>
</tr>
<tr>
<td>787-8</td>
<td>43,100</td>
<td>502,500</td>
<td>400,000</td>
<td>152,700</td>
<td>38% Basis of Future Freighter</td>
</tr>
</tbody>
</table>

Even more capable is the final variant of the venerable 747 program, the 747-8F, which could carry a substantially larger payload of 314,500 pounds to the target altitude. The problem with planning the fleet based on this super-jumbo is that the last 747-8 was delivered in January 2023 and the production line has closed. Consequently, obtaining new aircraft from Boeing would require a line restart roughly a decade from now, and the chances of that are remote. Just 107 747-8Fs were delivered, along with 48 in passenger configuration. The polar SAI mission would require more than 70 such aircraft, which is to say most of the production freighter fleet and roughly half the total fleet. The likelihood that so large a proportion of the delivered fleet could be acquired on the secondhand market is low, and passenger aircraft converted to the tanker mission would have marginally reduced payloads relative to the production freighters. Nonetheless, some substantial proportion of a polar SAI fleet perhaps could be assembled via used 747-8s. This raises the prospect of a mixed SAI fleet, consisting of some repurposed 747-8s and perhaps some similarly repurposed mid-life 777s, operating alongside a core of factory-delivered 777 Special Tankers. Freight operators are opportunistic in their fleet acquisitions and therefore customarily have a mix of production freighters and repurposed second-hand aircraft in their fleets. An SAI fleet might be similarly composed. Nonetheless, for the purpose of the discussion hereafter, we will assume an all 777 Special Tanker fleet is newly acquired from Boeing, noting that in fact some portion of it might be otherwise acquired.
Figure 3: Activity and fleet requirements for different candidate aircraft.

Even in respect of aircraft newly delivered from the factory, a design modification program would be needed to adapt the 777F to this unique mission. This could be done in the 777 production facility, but is more likely to be done in a separate modification line elsewhere. Insulated double-walled pressurized tanks would need to be installed through the existing freighter side door. These tanks should provide safe carriage of the aerosols and maintain it at the desired temperature in flight. The payload weight concentration near the aircraft’s center of gravity will require local strengthening of the main deck and surrounding airframe structure. An internal plumbing system would need to be developed and installed to enable aerosol loading on the ground and venting at altitude. Nozzles to vent the aerosols would need to be designed and installed. Local modifications to the aircraft electrical system would be required to provide power to pumps, control valves, and monitoring systems. If required by the FAA, the cargo bay may need to be isolated from the cockpit via a physical pressure bulkhead and positive pressure in the crewstation. This would ensure that an inadvertent escape of the highly toxic aerosols on the ground or in flight would not endanger the flight crew. Modifications to the cockpit instrument panel would allow the crew to manage and control the payload. All of these modifications would need to be engineered, tested, and certified before they could actually be installed in an operational aircraft.

As illustrated in Addendum 1, a reasonable time frame in which to engineer and certify the modification program that would transform the 777F into the 777 Special Tanker might be three years. Thereafter, a fleet of more than 90 777s must be manufactured, and then modified to the Special Tanker configuration. This is far too large a fleet to be sourced second-hand on the open market, so some or all of these would be new deliveries from the factory. Boeing currently produces 36 777s per year. If the SAI program were to...
elbow aside the world’s airlines and claim half the production line, it would take another 5 years to build
the fleet. 15 aircraft a year is also the rate at which Boeing is delivering new KC-46 tankers to the USAF, so
this is a market-reasonable production rate. Nonetheless, even with an existing aircraft program as our
starting point, it would require roughly a decade to stand up the aeronautical infrastructure required to be
capable of responding to tipping points in the cryosphere.

5. Ground infrastructure

A decade may prove short in comparison with the span required for the corresponding ground infrastructure.
We envision a minimum of four air bases as being required for the polar SAI program – two in the north
and two in the south. From a robustness/resilience/reliability standpoint, operating from a single base in
each hemisphere would be unreasonably risky. As previously noted, there are several options from which to
choose for bases near 60ºN, but for the purpose of this analysis, we focus on the two busiest among them,
Anchorage (ANC) and Stockholm (ARN). Each hosts roughly 100,000 aircraft landings per year. While
the SAI program would be merely seasonal, during the four month deployment window, daily operations
would roughly double at both airports. This would require a rough doubling of capacity of nearly everything
at the airport – hotels, fuel storage, maintenance hangars, kitchens, roadways, automobile parking garages,
medical facilities, warehouses, etc.

The most acute shortage noted in dialogue and correspondence with the operators of both airports is hard
stand aircraft parking spots, so a substantial apron expansion would be required. This would be relatively
straightforward for ARN, which owns ample adjacent land (F. Jaresved, personal communication, February
9, 2024), but not so for ANC, which is surrounded by urban development and the Cook Inlet. ANC provided a
budget estimate for 50 widebody hard stands with hydrant fueling and taxiway infrastructure of $500 million
(J. E. Johansen, personal communication, February 14, 2024). The estimated cost of a 777 maintenance
hangar would be nearly $100 million. Storage and servicing facilities for additional airfield maintenance and
ground service equipment would require $50 million, while the added equipment itself would add $20 million.
Additional de-icing capacity and crew quarters add yet another $20 million. A further $100 million would
be sufficient to fund the installation of four additional four million gallon storage tanks – three for expanded
fuel storage and one for sulfur (J. E. Johansen, personal communication, April 3, 2024). Nonetheless, for
$800 million dollars, all of the capacity expansion noted above could be designed, permitted, and built in
a span of perhaps fewer than ten years (J. E. Johansen, personal communication, April 3, 2024).

The above assumes that existing runway capacity would be sufficient to handle the additional traffic, but
that would only be true if the SAI operation could schedule around the existing daily and seasonal flight
peaks, running limited operations during the mid-day but most at night. From an efficacy standpoint, this
is entirely acceptable, though it would place constraints on the number of sorties each aircraft could run per
day. If on the other hand continued growth in passenger and freight operations at ANC were to combine with
the SAI demand to require a new runway (as the operators of both ANC and ARN believe is likely), then
both the cost and time estimate would grow. A new 10,000 foot north/south runway and adjacent taxiways
at ANC would require a substantial landfill protrusion into Cook Inlet, where a threatened local population
of Beluga whales are the object of great local affection. Citizen outcry and lawsuits should therefore be
expected. A new runway requirement would add roughly $1.5 billion to the budget and stretch the project
span to 12 – 15 years, with the majority of the latter pertaining to planning, permitting, and approvals (J.
E. Johansen, personal communication, April 3, 2024). The construction itself could be done in perhaps five
years.

Alternatively, building a new airport on a green field site near but not protruding into Cook Inlet would
appear to add minimally to the budget and not at all to the span. Choosing a site that is not hemmed in
by city and sea would eliminate the landfill requirement and substantially reduce the likely environmental
and citizen outcry. The operators at ANC estimate that a brand new airport dedicated to the SAI operation
could be built across the inlet on Point Mackenzie for $3 billion (J. E. Johansen, personal communication,
February 14, 2024).
Given their greater control of adjacent property, ARN might confront fewer land constraints in building out airport capacity. Nonetheless, their estimate for the span to add a runway is 15 – 20 years (F. Jaresved, personal communication, February 9, 2024). Construction itself would require no more than three years, so the vast majority of that span is consumed by planning and approvals. Nonetheless, were a new runway needed by 2040, the airport would need already to be in motion, which it is not.

A novel trans-national financing arrangement may be required to fund any airport build-out for emergency response SAI capability since the timing and likelihood of its use would be highly uncertain. One can imagine the voters of both Alaska and Sweden declining to fund such a project with immense global but limited local benefit. Nonetheless, with sufficient global will, there would undoubtedly be a way to overcome the local politics – there is ample precedent for doubling capacities at major airports.

The same must be true for the southern airports, but problems there are far more acute. Although we might seek a latitudinally symmetrical southern base at or near 60ºS, there is no proximate land mass to accommodate one. The southernmost tips of Africa, Tasmania, and the main islands of New Zealand lie at southern latitudes of 34.5, 43.4, and 47.2 degrees respectively. Stanley, the capital of the Falkland Islands, sits at 51.4ºS. South Georgia is farther south at 54.2ºS, but hosts only a tiny non-permanent population and no airport. No other islands deeper into the Southern Ocean offer sensible options. The closest landmass to our target latitude lies across the Southern Ocean in the South Shetland Islands at the tip of the Antarctic Peninsula, but this is utterly inhospitable territory lying too far south at 62.1ºS. Hosting the northernmost airport in Antarctica, it has merely a gravel runway a bit longer than 4200 feet.

Re-crossing the Southern Ocean, the tip of Cape Horn is at 56.0ºS on an uninhabited island whose notoriously foul weather at the juncture of the world’s two largest oceans has bedeviled sea operations for centuries and would create challenging conditions for air operations as well. The most southerly permanently settled place in the world is Puerto Williams, Chile at 54.9ºS on the southern side of the Beagle Channel in Tierra del Fuego. With a population of merely ~2500, its small airport has a single 4700 foot runway that handles roughly 1000 landings per year. In contrast, each airport in the SAI program would call for more than 250 per day on runways twice as long. Moving northward in search of more viable options, next up are Ushuaia, Argentina (54.5ºS) on the opposite (northern) side of the Beagle Channel, and Punta Arenas, Chile (53.0ºS) on the Strait of Magellan. Each has a runway longer than 9000 feet that could accommodate a 777. However, the SAI program would increase daily operations by roughly 20X at Punta Arenas and 30X at Ushuaia.

<table>
<thead>
<tr>
<th>City</th>
<th>Country</th>
<th>IATA Code</th>
<th>Latitude</th>
<th>Longest Runway (ft)</th>
<th>Landings per Year</th>
<th>Average Landings per Day</th>
<th>SAI Landings per Day</th>
<th>% Increase</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anchorage</td>
<td>USA</td>
<td>ANC</td>
<td>61.2N</td>
<td>12,400</td>
<td>114,975</td>
<td>315</td>
<td>251</td>
<td>80%</td>
</tr>
<tr>
<td>Stockholm</td>
<td>Sweden</td>
<td>ARN</td>
<td>59.7N</td>
<td>10,830</td>
<td>85,000</td>
<td>233</td>
<td>251</td>
<td>108%</td>
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<table>
<thead>
<tr>
<th>South</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Punta Arenas</td>
<td>Chile</td>
<td>PUQ</td>
<td>53.0S</td>
<td>9,154</td>
<td>5,000</td>
<td>14</td>
<td>251</td>
<td>1834%</td>
</tr>
<tr>
<td>Ushuaia</td>
<td>Argentina</td>
<td>USH</td>
<td>54.5S</td>
<td>10,827</td>
<td>3,200</td>
<td>9</td>
<td>251</td>
<td>2865%</td>
</tr>
</tbody>
</table>

Figure 4: Prospective bases for a polar solar geoengineering program.

Given such dramatic traffic increases, one could consider simply building a brand-new airport in one or both countries, but it is not merely tarmac and hangars that are required, but a local economy to employ, house,
and feed the many thousands of people required to staff this operation. To enable airport construction, the adjacent seaport would need to be expanded to bring in all the construction materials, fuel, sulfur, and other supplies. There is no large economic engine such as Anchorage or Stockholm in this region on which to piggyback, and there is no reasonable geographic alternative to Patagonia. One could perhaps reconceive these southern bases as isolated military installations, but that wouldn’t much change the infrastructure build-out required. Neither Chile nor Argentina would have any incentive to commence such a project until some external funding source stood ready to foot the bill. Nonetheless, given the low ebb at which these projects would start, it may be that the ground infrastructure build-out at the tip of Patagonia would define the critical path for global readiness to arrest the emergence of tipping points at the poles.

A review of public information about newly finished or ongoing airport construction projects around the world provides context regarding the granular cost and span information received from ANC and ARN. A list of substantially all new large commercial airports opened in the last 30 years for which publicly reported cost data is available suggests an average initial development cost of 5.8 billion in 2023 dollars. However, as this includes both very large and mid-sized airports, it is a very crude metric. Dividing these costs by the number of aircraft movements in a recent year yields a cost-per-100,000-movements of $2.9 billion. Alternatively, simply dividing development cost by the number of runways (another crude metric of airport capacity) yields a cost per runway of $2.6 billion. As we envision airports with 2 runways, this would suggest a cost of $5.2 billion for new SAI airports. This array of figures confirms that ANC’s estimate of $3 billion for a brand new airport is on the low end of a very reasonable range. We will therefore assume that a new airport of the size envisioned here might cost $3 – 5 billion. They also confirm that ANC’s estimate $2.3 billion to double the capacity of an airport currently handling roughly 100,000 aircraft movements per year is also reasonable, leading to a planning figure for such an expansion of $2 – 3 billion.

<table>
<thead>
<tr>
<th>Airport Name</th>
<th>Location</th>
<th>Cost (2023 SB)</th>
<th>Runways at Open</th>
<th>Aircraft Movements</th>
<th>Cost per 100k Movements</th>
<th>Cost per Runway</th>
</tr>
</thead>
<tbody>
<tr>
<td>Denver</td>
<td>Denver, Colorado</td>
<td>8.8</td>
<td>5</td>
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**Figure 5:** *Cost metrics for recent and ongoing new airport construction projects.*

The average span from initial start date to opening for the airports listed is nearly 19 years. This exceeds the
15 years estimated by ANC and suggests that 15 – 20 years for a new airport would be a prudent assumption.

<table>
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<tr>
<th>Airport Name</th>
<th>Location</th>
<th>Initial Start Date</th>
<th>Approval Date</th>
<th>Construction Start Date</th>
<th>Opening Date</th>
<th>Total Span</th>
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<td>2018</td>
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<td>Central Communicator</td>
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<td>2004</td>
<td>2017</td>
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<td><strong>Average</strong></td>
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<td><strong>18.9</strong></td>
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</table>

Figure 6: *Time span metrics for recent and ongoing new airport construction projects.*

6. Discussion

To summarize, the large infrastructural tasks required to put a tipping element response capability in place by 2040 are to:

- design and certify a modification program for Special Tankers
- manufacture or otherwise acquire a fleet of over 90 777s and run them through the modification line
- roughly double the capacity of two large northern airports
- build nearly from scratch two large airports and related support capacity in Patagonia

As previously noted, a roughly decadal time span should be sufficient to procure the fleet. That would be more than enough time to build out capacity at ANC and ARN if no new runways are required, but likely insufficient time to build no runways. It appears unlikely that the southern bases would be ready on this schedule, irrespective of whether they are built upon the existing infrastructure at USH and PUQ or simply erected on green fields elsewhere in Tierra del Fuego. However, it may be that a smaller operation could commence from the existing southern airports while the new ones are completed, implying that the attainment of the full Antarctic annual deployment would lag its Arctic counterpart by several years. Even with the benefit of this optimistic assumption, in order to have our tool ready by January 1, 2040, we would need to start on our longest span task by roughly January 1, 2030.

As earlier noted, “start” in this case would not mean “commence the governance discussions” – it would mean one or more large governments actually dispensing funds to contractors on three continents to begin
engineering on aircraft and airports. Within a few years, dirt would need to be moving at various airports and Boeing or Airbus would need to start delivering purpose-built aircraft. But if the funds started to flow in 2030, with some fudge factor for the southern airfields, we could conceivably have our response capability in place by our target date. All that seems feasible.

What does not seem feasible is that we would be ready to pull that trigger by 2030. This article is being written in 2024 and with luck will be published in the same year. Insofar as we are aware, no government is yet in motion to prepare a tipping point response capability at all, let alone by 2040. There is no awareness among policy makers of such a prospect, and no public support for it. And yet, in five years and change, some consortium of governments must be ready to sign a ~$20 billion purchase contract with a major airframer and another roughly $15 billion in construction contracts with Argentina, Chile, Sweden and the State of Alaska. Failing that, the 2040 date will recede into the future, and the span during which the world will be vulnerable to and unprepared for tipping points in the cryosphere will be elongated.

This flips the standard conception of the governance problem related to SAI on its head. Governance is customarily conceived in negative terms – i.e., the task is to prevent actors from deploying prematurely, irresponsibly, and/or without widespread consensus and legitimacy. It is a matter of preventing parties from doing things we don’t want them to do. However, the governance task here is the opposite – how to get someone to step up to the plate by 2030 with a roughly $35 billion checkbook to put in place a response capability that would protect all of humanity but that few people yet understand to be required. This protection would be a non-excludable good, creating incentives for all actors to free-ride. In light of that, how do we motivate a first mover, and how do we distribute the financial burden of implementing the program?

Addressing that is beyond our scope here, but we hope that a first step in deriving an answer is to pose the question.

7. Conclusion

In the climate arena as elsewhere, when one finds oneself in a hole, the first step is to stop digging. Emissions reductions are the indispensable response to the climate problem, and neither SAI nor any other palliative can replace them. However, it is increasingly clear that those may not arrive in time to ward off substantial climate damages in the coming decades, particularly from prospective tipping elements with low temperature thresholds. Mitigation alone is no longer sufficient to secure the future climate that we and our successors on this planet will desire. A global peak-shaving SAI program could also prove effective in delaying or preventing tips in both high and low-latitude regions, but for both infrastructural and sociopolitical reasons, it seems a distant prospect. The infrastructural hurdle related to an SAI program targeting just the poles appears to be substantially smaller, as would be the resulting deployment program. It is unclear on the other hand whether the remoteness of the deployment program and the prospective urgency of the tipping element threat may render this intervention more acceptable to policy makers and the general public than the global sort, but that is the question that needs to be promptly presented. Policy makers are only dimly aware that tipping element threats may be looming in our near future, and we know of no prior literature suggesting that the intervention discussed here may be a practicable remedy. A prudent response to these circumstances would be to dramatically accelerate both research and communications in respect of a polar solar geoengineering program such that by 2030, policymakers might be capable of making an informed decision as to whether to proceed.

On the one hand, this seems like an impossibly tall order given the low awareness of and regard for SAI that obtains currently. And yet, the response to the 1985 identification of the “ozone hole” demonstrates that concrete pending threats have a galvanizing effect that slowly rising temperatures do not. The analogy is not precisely apt, but the tipping element threats appear to be real if somewhat indeterminate. We are steaming heedlessly towards shoals that we are warned may lie directly ahead. As we seem unable to rapidly stop the ship, it would seem unjustifiably cavalier not to explore other response options that appear promising.
References


