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Stratospheric aerosol injection tactics and costs in the first 15 years of deployment

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Abstract

We review the capabilities and costs of various lofting methods intended to deliver sulfates into the lower stratosphere. We lay out a future solar geoengineering deployment scenario of halving the increase in anthropogenic radiative forcing beginning 15 years hence, by deploying material to altitudes as high as ~20 km. After surveying an exhaustive list of potential deployment techniques, we settle upon an aircraft-based delivery system. Unlike the one prior comprehensive study on the topic (McClellan *et al* 2012 *Environ. Res. Lett.* **7** 034019), we conclude that no existing aircraft design—even with extensive modifications—can reasonably fulfill this mission. However, we also conclude that developing a new, purpose-built high-altitude tanker with substantial payload capabilities would neither be technologically difficult nor prohibitively expensive. We calculate early-year costs of ~\$1500 ton^{-1} of material deployed, resulting in average costs of ~\$2.25 billion yr^{-1} over the first 15 years of deployment. We further calculate the number of flights at ~4000 in year one, linearly increasing by ~4000 yr^{-1}. We conclude by arguing that, while cheap, such an aircraft-based program would unlikely be a secret, given the need for thousands of flights annually by airliner-sized aircraft operating from an international array of bases.

1. Introduction

Solar geoengineering is commonly seen to be subject to what some call its 'incredible economics' (Barrett 2008) and, more specifically, its 'free driver' effect: its direct costs are so cheap compared to its potential climate impacts so as to reverse many of the properties of the so-called 'free rider' problem governing carbon mitigation decisions and climate policy more broadly (Wagner and Weitzman 2012, 2015, Weitzman 2015). The governance problem becomes one of cooperation to restrain rather than increase action. Here we probe these economic assertions and review the capabilities and costs of various lofting methods intended to deploy sulfates into the lower stratosphere, the leading proposed method of solar geoengineering (Keith 2000, Crutzen 2006, National Research Council 2015). Stratospheric Aerosol Injection (SAI) would require lofting hundreds of thousands to millions of tons of material each year to altitudes up to ~20 km. Here we seek answers to three questions: if SAI deployment were to commence within the foreseeable future with the tools and technologies at our disposal, how would such deployment be physically achieved, how much would it cost, and could it be done in secret?

National Academies of Sciences (NAS), Engineering and Medicine (1992) provides an early review of SAI deployment options, deriving detailed pricing for naval rifles and two different balloon systems (appendix Q.11). McClellan *et al* (2012) attempt to provide the first comprehensive answer to this question, publishing results from an earlier Aurora Flight Science Corporation analysis (McClellan *et al* 2010). Like McClellan *et al* (2010, 2012), and later reviewed by Moriyama *et al* (2017), we explore an array of different SAI lofting technologies and given our more specific mission criteria, we conclude that aircraft are the only reasonable option. Unlike them, we conclude that modified existing business jets are incapable of flying above ~16 km, a conclusion confirmed directly by the manufacturers of the jets in question. This directly contradicts both McClellan *et al* (2010, 2012) and IPCC (2018). The latter demonstrates the large influence McClellan *et al*'s analysis has had on the broader conversation. IPCC (2018) states that 'there is *high agreement* that aircrafts after some modifications could inject millions of tons of SO_2 in the lower stratosphere (~20 km)' (chapter 4). IPCC cites three studies in support of that statement, including McClellan *et al* (2012). However, both of the other two studies, in turn, base their conclusions, in large part, on McClellan *et al*'s earlier analysis. Irvine *et al* (2016) also cites the other (Davidson *et al* 2012), which, in turn, cites McClellan *et al* (2010). Robock *et al* (2009) provides one further independent analysis, reviewing capabilities of military fighters and tankers. We agree with Robock *et al* (2009) that military fighters are capable of reaching ~20 km, but they are incapable of sustained flight at that altitude (see table 2 below).

We further conclude that no other existing aircraft have the combination of altitude and payload capabilities required for the mission, leading us instead to the design of a new plane.

We propose such a plane and call it SAI Lofter (SAIL), describing its basic specifications and providing detailed cost estimates for its design, manufacture, and operation under a hypothesized solar geoengineering scenario of halving the increase in radiative forcing from a date 15 years hence. We do not seek to foretell future technological breakthroughs, nor do we guess at costs in 50 or 100 years when next-generation deployment technologies would likely become available. Further, we do not consider solar geoengineering methodologies other than SAI or materials other than sulfate aerosols (Keith 2000, Keith *et al* 2016). We instead hope to illuminate discussions of direct SAI deployment costs based on existing technologies, thereby facilitating further benefit-cost comparisons and grounding 'free driver' discussions in concrete numbers supported by science-based SAI deployment scenarios and sound aerospace engineering.

2. Stratospheric aerosol deployment scenario

Following a research hypothesis proposed by Keith and Irvine (2016), we consider a limited SAI deployment scenario (Sugiyama *et al* 2018) intended to cut in half the rate of temperature change from the first year of the program onward. While such a scenario is less ambitious (and less environmentally risky) than those aimed at keeping temperatures constant from a certain date forward, it is more ambitious than SAI merely holding the rate of temperature change constant (MacMartin *et al* 2014).

We further assume anthropogenically driven radiative forcing of ~2.70 W m^{-2} by 2030, with an assumed decadal increase of ~0.5 W m^{-2} that is roughly consistent with the Representative Concentration Pathway (RCP) 6.0 scenario (Moss *et al* 2010, IPCC 2013). Assuming the desire to cut this rate of increase in half implies the need for SAI to reduce radiative forcing by ~0.25 W m^{-2} by the end of the first decade of deployment. The implied change in global average surface temperatures from SAI deployment is −0.2 K per decade, with an assumed global average temperature sensitivity of 0.8 K per W m^{-2}.

Table 1. Hypothesized base-case SAI scenario in the first 15 years of deployment commencing in 15 years. Tons of S carried are half of tons SO_2 dispersed.

Year	Unabated forcing (W m^{-2})	Target forcing (W m^{-2})	SO_2 dispersed (Mt)[a]	Temperature reduced (K)[b]
2033	2.850	2.825	0.2	−0.02
2034	2.900	2.850	0.4	−0.04
2035	2.950	2.875	0.6	−0.06
2036	3.000	2.900	0.8	−0.08
2037	3.050	2.925	1.0	−0.10
2038	3.100	2.950	1.2	−0.12
2039	3.150	2.975	1.4	−0.14
2040	3.200	3.000	1.6	−0.16
2041	3.250	3.025	1.8	−0.18
2042	3.300	3.050	2.0	−0.20
2043	3.350	3.075	2.2	−0.22
2044	3.400	3.100	2.4	−0.24
2045	3.450	3.125	2.6	−0.26
2046	3.500	3.150	2.8	−0.28
2047	3.550	3.175	3.0	−0.30

[a] Assumes −0.25 W m^{-2} per Tg S.
[b] Assumes 0.8 K per W m^{-2} average temperature sensitivity (see text).

We focus on SAI using sulfates, not because they are optimal—they may not be (Keith *et al* 2016)—but because the long record of prior analyses on both efficacy and risks of sulfate deployment (National Research Council 2015) renders them the best understood and therefore least uncertain material with which to commence in this hypothetical scenario of partial deployment. In the base case, we assume a top-of-atmosphere (TOA) sulfate forcing sensitivity of −0.25 W m^{-2} per Tg S yr^{-1}, a value toward the lower end of recent estimates. Pierce *et al* (2010) estimates −0.34 W m^{-2} and Dai *et al* (2018) derives a range from below −0.50 to over −2 W m^{-2} for injections between 30 °N and 30 °S. Other estimates for different injection scenarios, roughly converted to TOA values, range from −0.15 W m^{-2} (Kuebbeler *et al* 2012) to −0.33 W m^{-2} (Niemeier and Timmreck 2015), while Pitari *et al* (2014) shows results from the Geoengineering Model Intercomparison Project (GeoMIP), here roughly converted to TOA, for one point of injection at the equator ranging from −0.47 to −0.98 W m^{-2}.

Table 1 summarizes the base-case SAI deployment scenario for the first 15 years of a program commencing in 15 years. The year 2033 is entirely hypothetical. It is not the most likely start date, nor are we suggesting it is an optimal one, but any deployment much sooner seems highly unlikely based on scientific and political considerations. Later deployment may mean the approaches explored here can be revised in light of new scientific and technological developments.

The assumed linear ramp-up, and assumed lofting of sulfate for the purpose of dispersing SO_2 (Smith *et al* 2018), implies the need to loft ∼0.1 Mt of S in year one, increasing at a rate of ∼0.1 Mt yr^{-1} linearly thereafter. Note this is significantly less material than McClellan *et al* (2012) assumed mass fluxes of either 1 or 5 Mt yr^{-1} of S, presenting a more limited and phased deployment scenario (Sugiyama *et al* 2018).

Another important consideration is the location for SAI. Following Tilmes *et al* (2018a), we assume base-case injection sites at latitudes of 15° and 30° North and South of the equator. This is no verdict as to these four latitudes being optimal or definitive. It is a statement that, if forced to choose today, these four latitudes appear like a good starting point for discussions (Kravitz *et al* 2017, MacMartin *et al* 2017, Richter *et al* 2017, Tilmes *et al* 2017, Dai *et al* 2018). Note that while SAI latitudes matter, longitudes appear not to, as injections at any one longitude mix rapidly to all others. Latitudes, meanwhile, influence the height of injections. At 15 °N and S, injections may be required as high as ∼20 km (Pierce *et al* 2010). Some argue that even higher injection altitudes would provide greater radiative benefit (Tilmes *et al* 2018b). For the purpose of defining the deployment scenario, we define the service ceiling necessary for the lofting platform at ∼20 km.

3. Review of possible lofting technologies

We have undertaken a review of all lofting technologies that seem plausible as methods to hoist 0.1 Mt S to an altitude of up to ∼20 km in 2033. Our main research involved engaging directly with commercial aerospace vendors to elicit what current and near-term technology platforms can achieve at what cost. We have met or corresponded directly with: Airbus, Boeing, Bombardier, Gulfstream, Lockheed Martin, Northrup Grumman; GE Engines, Rolls Royce Engines; Atlas Air, Near Space Corporation, Scaled Composites, The Spaceship Company, Virgin Orbit, and NASA, the latter in respect of its high-altitude research aircraft fleet.

Table 2 summaries our findings across lofting technologies. We eliminate technologies we deem insufficiently mature to be used for deployment 15 years hence and those incapable of reaching the required altitude. Existing commercial and military transport aircraft cannot achieve the required altitudes, even with extensive modifications. Modified business jets, noted prominently in McClellan *et al* (2010, 2012) study, are incapable of reaching altitudes above ∼16 km. High payload, high altitude aerostats have been hypothesized but not yet successfully tested, and in all events, are operationally fragile, unable to operate in adverse weather conditions. Tethered hoses are even less technologically mature and to-date untested. Military fighters such as the F-15 have reached altitudes of ∼18 km in the context of record-setting ballistic climbs in ideal conditions, but they are incapable of either sustained flight or regular operations at such altitudes.

Among technologies capable of achieving the mission, costs are often prohibitive. NASA's existing high-altitude aircraft that can reach appropriate altitudes do so with ∼1 t payloads, making them very costly. Rockets are intended to reach altitudes 15–25× higher than are required to reach the lower stratosphere, rendering them both ill-suited and extremely costly. Even if the unit-costs of the massive SpaceX Falcon Heavy were reduced by 95% to account for the ratio of its normal target altitude to the ∼20 km assumed here, it is still roughly 50× costlier than SAIL. Balloons and large naval-style guns are capable and plausible alternatives, but their per-ton costs are at least 10× as high as those we estimate for SAIL.

Table 2 also shows McClellan *et al* (2010, 2012) new high-altitude aircraft, which posits a cost-per-ton similar to that of SAIL. While we derive a similar unit-cost, SAIL's numbers apply to the initial years of deployment, while McClellan *et al* consider annual masses of both 1 and 5 Mt, the latter of which implies a larger and more mature program that may have achieved substantial economies of scale. For reference, our estimate is that a second-generation platform lofting the same 5 Mt yr^{-1} might have unit costs at least 20% lower than the $1400 calculated here for a first-generation SAIL technology.

4. SAI lofter (SAIL)

Given the apparent inadequacy of existing technologies, especially of previously assumed-to-be-adequate modifications to existing aircraft (McClellan *et al* 2010, 2012), we propose a novel aircraft with disproportionally large wings relative to its narrow fuselage. We also describe the aircraft fleet requirements, and we calculate development and deployment costs from conception through year 15 of the hypothetical program.

4.1. Design

The aircraft is designed to meet the assumed requirements outlined in section 3 above. In particular, it is capable of level flight at an altitude of ∼20 km while carrying a 25 ton payload—large enough to lower operational costs significantly relative to existing high-altitude aircraft, yet small enough to make the mission possible. We have developed the design with direct input from several of the aerospace and engine companies consulted. It assumes a novel aircraft design but utilizes modified pre-existing low-bypass engines, which, though disfavored in commercial

Table 2. Cost and capabilities comparison of lofting technologies.

Platform	Cost ('000 \$/t)	SAIL multiple	Source
Mission capable			
SAIL[a]	1.4	1×	
McClellan New High Altitude Aircraft	1.5[b]	~1×	McClellan *et al* (2010, 2012)
Delft SAGA[c]	4.0	~3×	Delft Report[c]
McClellan Modernized Gun	19	~14×	McClellan *et al* (2010, 2012)
Balloons	~40	~28×	Near Space[d]
NASA WB57	43	~30×	NASA[d]
NASA ER2	50	~35×	NASA[d]
NASA Global Hawk	70	~50×	NASA[d]
SpaceX Falcon Heavy Rocket	71[e]	~50×	Chang (2018)
Gun Mark 7 16'	137	~100×	McClellan *et al* (2010, 2012)
Vector Rocket	1180[e]	~850×	Chang (2018)
Virgin Orbit Rocket	2000[e]	~1400×	Virgin Orbit[d]
Mission incapable			
Existing Commercial Aircraft	Not capable of reaching ~20 km[f]		
Modified Commercial Aircraft	Not capable of reaching ~20 km[g]		
Existing Military Transporters[h]	Not capable of reaching ~20 km[g]		
Military Fighters	Not capable of sustained flight at ~20 km[g]		
Tethered Hose	Not sufficiently mature technology[g]		
Aerostats/Airships	Not sufficiently mature technology[g]		

[a] See section 4 for cost derivations.
[b] Assumes a program deploying ~1 Mt yr^{-1}.
[c] TU Delft student report developing SAGA, the Stratospheric Aerosol Geoengineering Aircraft (Design Synthesis Exercise Group 2 2016).
[d] Personal communications with individuals at respective entities.
[e] Reduced by 95% to account for 20 km target altitude relative to 200 km for Earth orbit; Chang (2018)'s estimates for Vector Rocket confirmed by Vector Launch.
[f] McClellan *et al* (2010, 2012) and authors' analysis (see text).
[g] Authors' analysis (see text), including, for military fighters, personal communication with Boeing, Lockheed Martin, and Northrup Grumman.
[h] Including existing military tankers.

service due to their reduced fuel efficiency, will perform better at high altitudes.

Broadly, SAIL is equivalent in weight to a large narrow body passenger aircraft such as the A321, or in Boeing terms, sized between the 737–800 and the 757–200. In order to sustain level flight in the thin air encountered at altitudes approaching ~20 kms, SAIL requires roughly double the wing area of an equivalently sized airliner, and double the thrust, with four engines instead of two. (While maximum thrust requirements of most aircraft are defined by takeoff, SAIL's engines are configured to perform at high altitudes.) At the same time, its fuselage would seem stubby and narrow, sized to accommodate a heavy but dense mass of molten sulfur rather than the large volume of space and air required for passenger comfort. SAIL would therefore have considerably wider wingspan than length. Its compact fuselage, however, would sit behind a conventional manned cockpit. While it is easy to imagine SAIL migrating to unmanned cockpits over time, under current certification rules, it would be substantially faster and therefore cheaper to certify the aircraft with onboard pilots.

More specifically, the preliminary design for SAIL calls for a length of ~46 m, a wingspan of ~55 m, and a wing area of ~250 m^2, with an aspect ratio of ~12:1. The maximum structural payload would be ~25 t, with maximum takeoff weight (MTOW) of ~100 t, operating empty weight (OEW) of ~50 t, and maximum fuel load of ~32 t. The aircraft would have 4 wing-mounted low-bypass engines, modified for high-altitude operations with an aggregate take-off thrust of ~25–30 t and a thrust-to-weight ratio of ~30%. (GE Engines considers its F118 engine adequate, noting that it powers the NASA Global Hawk aircraft to similar altitudes; its Passport 20 engine may similarly be capable. Rolls Royce suggests its BR710 or BR725 engines.) The design will require a smaller fifth centerline auxiliary power unit for bleed air and onboard combustion of the molten sulfur payload.

This highlights another advantage of aircraft as a lofting platform, since they can take advantage of the onboard combustion system from S to SO_2 explored by Smith *et al* (2018). Lofting S would cut in half the payload required compared with lofting SO_2. Moreover, S is a less dangerous substance than SO_2 to handle on the ground or contend with in the event of an accident. Other possible lofting methods such a balloons and guns could not accommodate this *in situ* conversion with existing technologies and would, therefore, need to loft SO_2 with twice the mass of SAIL's payload.

SAIL is designed for a service ceiling of ~20 km, with a maximum altitude of up to ~19.8 km in a

Table 3. Total fleet and flight activity by hypothesized deployment year.

Year	New aircraft[a]	Total aircraft[a]	Total payload (Mt S)[b]	Flights/year	Bases	Monthly flight hours/ aircraft[c]	Flights/ base/day
2033	8	8	0.1	4007	2	278	5
2034	6	14	0.2	8015	2	278	11
2035	8	22	0.3	12 022	4	278	8
2036	6	28	0.4	16 029	4	278	11
2037	6	34	0.5	20 036	4	278	14
2038	6	40	0.6	24 044	4	278	16
2039	7	47	0.7	28 051	4	272	19
2040	6	53	0.8	32 058	4	273	22
2041	6	59	0.9	36 065	4	273	25
2042	6	65	1.0	40 073	4	274	27
2043	6	71	1.1	44 080	4	274	30
2044	6	77	1.2	48 087	4	274	33
2045	6	83	1.3	52 095	4	275	36
2046	6	89	1.4	56 102	4	275	38
2047	6	95	1.5	60 109	4	275	41

[a] Includes one spare aircraft per base.
[b] S burned *in situ* to disperse 2× SO_2 (see table 1).
[c] Excludes spare aircraft.

typical mission. Each mission would last ~5 h, with ~2 h of ascent and descent time each, plus ~1 h on station. The ~2 h for ascent and descent time situates SAIL reasonably between the performance rates of the Global Hawk and U2/ER2. That assumes a ~25 t payload and a conversion of S to SO_2 at ~0.5 t S per minute. Operational flights are flown out and back to the same base, with a range of ~4500 km for each plane at maximum payload. While Tilmes *et al* (2018b) have noted that injections at altitudes 5 km higher would add perhaps 50% to the radiative benefit derived from deployed aerosols, SAIL and similar aircraft deploying conventional engine technology to haul large payloads are unable to substantially exceed ~20 km.

The design assumes 2 pilots plus 1 payload operator, and accommodates 1 supernumerary, possibly a scientific observer. Crucially, there are no passengers, which simplifies regulatory certification for the newly designed plane. SAIL would only have one mission and at most a handful of operators. Ferry and positioning flights aside, SAIL can be expected to fly only in a few remote air corridors, likely enabling it to operate as an experimental aircraft in a restricted category without full commercial certification. This in turn would substantially reduce developmental costs.

4.2. Fleet

We calculate that in year 1 of the deployment program (assumed to be 2033), the SAIL fleet would require 8 new aircraft including one flight-ready spare plane at each of the two initial bases. This assumes that one spare does not substantially influence our cost estimates. Table 3 summarizes SAIL fleet and activity in the first 15 years of deployment.

Such a scenario also assumes that by year 16, the 'first-generation' SAIL technology is supplanted by a second-generation lofting solution for which much higher development sums would be expended to achieve substantially lower subsequent operating costs. No new SAILs would be manufactured thereafter, though the existing SAIL fleet would serve out its remaining economically useful lifespan. We therefore consider development costs of this first-generation SAIL technology, commencing 7 years before year 1 of the program, but do not include any additional development costs to further refine or supplant the technology.

4.3. Development Costs

We estimate total development costs of $~2 billion for the airframe, and a further $350 million for modifying existing low-bypass engines. These numbers are toward the lower end of McClellan *et al* (2010, 2012) range of $2.1 to $5.6 billion and significantly below the TU Delft students' estimates of $14 billion for its purpose-built Stratospheric Aerosol Geoengineering Aircraft, or SAGA (Design Synthesis Exercise Group 2 2016). The former base their estimates largely on RAND Corporation's Development and Procurement Costs of Aircraft (DAPCA) model first developed in the 1960s and 1970s (Boren 1976, Raymer 1999). The latter use McClellan *et al* (2010, 2012), and thus DAPCA indirectly, as one data point, but also consider a more granular build-up of development costs by category, and finally compare those numbers to the developmental budget for the A380. We arrive at our numbers by developing the preliminary aircraft design described in section 4.1 and then budgeting the elements of that design in a series of personal conversations with relevant commercial vendors. Among the important findings derived from that approach was that while both McClellan *et al* and TU Delft devoted roughly half their developmental budget to the development of new engines, we found several pre-existing engines that can power SAIL, though with substantial modifications to account for the high-altitude operations.

We de-emphasize commercial aircraft development programs as relevant data points, since it is very different and significantly costlier to design a flexible aircraft for a range of commercial operations than to design a small batch of specialty aircraft like SAIL that is intended for a novel but very specific mission. SAIL must demonstrate that it can fulfill its mission, but its testing and certification process does not need to explore the entire flight envelope to determine the range of operations for which a variety of operators might purpose the aircraft. Moreover, SAIL does not need to compete against other aircraft based on operating costs. In these senses it is more like a military design exercise—what matters is that the aircraft can achieve the specified mission, but the optimization of operating costs is a substantially lesser consideration. Much of the design, certification, and testing costs for commercial manufacturers like Boeing, Airbus, and Bombardier lies in optimizing the aircraft for operational cost by reducing drag, fuel consumption, and maintenance cost, while increasing operational reliability. These same considerations would be applicable to a second-generation SAI lofting solution, when (and if) the desirability of this intervention has been proven and the lofted masses need to be substantially greater. This may be a more advanced and potentially unmanned aircraft, or a non-aircraft lofting technology. The first-generation solution on the other hand would favor 'quick and cheap' experimental aircraft for an experimental mission.

Moreover, the small production run of SAIL is unlikely to attract the world's biggest airframe developers and is more likely the province of experimental aircraft designers. Two such companies have reviewed detailed SAIL specifications and contributed to the conclusion that development costs for SAIL would be less than the reported $300 million budget for Stratolaunch (Foust 2011), the massive catamaran aircraft currently being built with funding from the late Paul Allen. Given that the 650t MTOW of Stratolaunch is more than 6 times that of SAIL, a $250 million budget for the demonstrator aircraft seems generous. Testing costs for a restricted category certification, meanwhile, would run two to three times that, placing the total airframe budget at $0.75 to $1 billion. We arrive at our $2 billion airframe development figure by taking the high end of the range, and arbitrarily doubling it to account for the well-established history of cost overruns in aircraft developmental programs.

To this airframe budget we have added $350 million for engine modifications and testing, which personal communications with Rolls Royce indicates would be sufficient to purpose one of its existing engines to this program.

Both Scaled Composites and The Spaceship Company estimate five years to be the best-case development timeframe, and would suggest allocating 7 years from the commencement of a fully funded program through certification in the relevant jurisdictions and entry into service of the first production aircraft.

All this assumes a deliberate but standard development program rather than a crash effort intended to deploy SAI as soon as possible in response to a (perceived) crisis. Such a military-style deployment effort could cut several years off the assume 7-year development phase and bypass the required civil certification process, while substantially increasing costs.

4.4. Operating costs

We build a SAIL operating budget using modeling conventions and cost factors common to the air freight industry, including aircraft financing assumptions. Table 4 details SAIL operating cost assumptions based upon the relevant cost drivers. We assume $2.00 per gallon for fuel, which comprises one of the largest elements of operating cost, while the cost of sulfur comprises a mere 3% of the budget. We assume a cost of $80/t for molten S (US Geological Survey 2018, pp 160–161) with an assumed additional $20/t for transport.

We assume the average marginal cost (i.e. excluding amortization of development costs) for each additional aircraft to be $100 million, roughly equal to the actual purchase price (as opposed to list price) of B767-300 and A330 freighters, both of which have about twice the OEW of SAIL. This assumes that SAIL aircraft will be priced at a substantial premium relative to OEW peers such as the A321 because of the much lower projected production volume.

Given that the low annual aircraft production rates will not facilitate optimization of the production line, we assume conservatively that the build time for one SAIL aircraft is two years. That implies that prior to the commencement of operation, the program will have funded not only the $800 million required for the initial complement of 8 aircraft, but an additional $300 million in progress payments towards the additional six aircraft required in year 2.

In addition to pre-start capital costs, we assume the need for an aggregate of $~40 million to fund an administrative entity that will manage the development program for the aircraft over its seven-year gestation cycle as well as to plan for the commencement of operations. During the two years immediately preceding deployment, a yet larger sum will be required for start-up costs such as hiring and training staff, setting up bases, procuring inventory, and certifying the airline(s) that will actually operate the flights. We estimate the capital required for this purpose to be 50% of the first year operating budget, excluding aircraft capital costs—a sum equal to roughly $100 million.

Table 5 summarizes total SAIL capital requirements during the assumed seven-year development

Table 4. SAIL operating cost assumptions.

Category	Input[a]	Units	Notes
Aircraft capital cost	\$1.035 m	/aircraft/ month	Total aircraft cost of \$130 m; 0.8% monthly lease factor
Crew	\$1.4k	/block hr[b]	9 crew/aircraft; narrowbody pay scale; crew utilization of 275 block hrs/ month; includes (remote) travel and accommodation, and accounts for payload operator
Airframe maintenance	\$~43k	/aircraft/ month	3, 8, 10 and 12 years heavy maintenance checks
Engine maintenance	\$~2.9k	/aircraft cycle	Average of industry CFM56 or Rolls Royce RR211 full restoration cost/cycle plus \$280/cycle for life-limited parts; 4 engines; full restoration costs = \$3.5 m
Landing gear maintenance	\$~4.2k	/aircraft/ month	Typical narrowbody industry cost; 10-year overhaul interval
APU[c] maintenance	\$20	/block hr[b]	Typical industry cost
Line maintenance	\$800	/block hr[b]	Average industry cost for narrowbody plus a premium for unique parts; includes both cyclic & hourly drivers, approximated by block hr
Specialized equipment	\$250	/block hr[b]	Maintenance of specialized aerosol storage, combustion, and dispersal equipment
Insurance	\$~54k	/aircraft/ month	0.5% of acquisition cost (typically 0.25%, added 0.25% for non-typical operation)
Fuel usage	1.85k	gallons/ block hr[b]	Average for Boing 737 and 757, doubled given 4 engines
Fuel price	\$2	/gallon	Projected long-term cost/gallon
Navigation charges	\$400	/block hr[b]	Typical industry cost
Landing fees	\$~1.8k	/cycle	\$0.009 kg^{-1} of MTOW (typical industry cost) × 2 for specialized facilities
Ground handling	\$2k	/cycle	Tow, pushback, etc; premium atop typical narrowbody costs to reflect low utilization airports
Cargo handling	\$0.05	/kg	Typical industry loading cost, applied to departures only (no offload on landing)
Overhead	\$250k	/aircraft/ month	Dispatch, crew scheduling, flight planning, flight and tech ops administration, other general and administrative costs
Initial spares purchase	\$2 m	/aircraft	Start-up cost; premium over typical narrowbody cost given unusual airframe; balance sheet item only, not income statement
Spares carrying cost %	20%	/yr	Typical industry cost; both depreciation and replacement costs
Inventory carrying cost	\$400k	/aircraft/yr	Annual income statement impact for spares
Initial crew training	\$800k	/aircraft	Typical industry cost; upfront cost only per aircraft; recurring training cost in crew cost numbers
Payload	\$0.10	/kg	\$80/t for molten S (US Geological Survey 2018, pp 160–161), plus \$20/t for transport

[a] All \$ figures in 2018 US \$.
[b] Block hour assumed to be 95% of flight hour.
[c] APU = Auxiliary Power Unit.

phase and the first 15 years of operation. Total pre-deployment capital requirements are ~\$3.6 billion. All costs (pre-start and operational) through Year 5 are ~\$10 billion. Total costs through Year 15 are ~\$36 billion.

Total ops costs in table 5 presents the resulting annual operating costs, including capital costs for fleet procurement as well as the amortization of total development costs. All told, year 1 operating costs are ~\$310 m, increasing annually in rough proportion to the growing deployment masses. Unit costs per deployed t SO_2 decrease slightly in each year due to accumulating but limited economies of scale. Both simple and weighted average operating costs are ~\$1400/t SO_2 deployed, in 2018 US \$. That places total costs well below any other alternative currently available technology and roughly equivalent to McClellan *et al* (2010, 2012) \$1500/t unit cost estimate for 1 Mt deployed via its proposed new aircraft program. For the reasons outlined above, we have significantly more confidence in our estimate. While \$1400/t may convey a false sense of precision, we are confident to conclude that average operating costs are <~\$1500/t of SO_2 deployed throughout the first 15 years of a deployment that is aimed at offsetting half of the increase in radiative forcing, beginning 15 years hence.

4.5. Sensitivity analysis

Table 6 shows sensitivities of the key financial metrics to changes in various assumed inputs. In general, costs per deployed ton prove to be highly consistent across scenarios. Deployed tons vary substantially, leading to large variations in total annual operating costs.

Table 5. Hypothesized SAIL capital requirements and annual operating costs ($m) relative to deployment year, including cost per deployed t SO_2 ($k).

Year[a]	Admin costs	Aircraft NRE[b]	Aircraft build	Ops start-up costs	Non-fleet ops costs	Total ops costs	Cost per tSO_2 ($k)
−7	2.6	335.7					
−6	3.3	335.7					
−5	4.1	335.7					
−4	5.8	335.7					
−3	7.1	335.7					
−2	8.1	335.7	400	35			
−1	9.4	335.7	700	71			
1			700		212	311	1.56
2			700		406	580	1.45
3			600		613	886	1.48
4			600		807	1155	1.44
5			650		1003	1426	1.43
6			650		1199	1696	1.41
7			600		1401	1985	1.42
8			600		1596	2254	1.41
9			600		1792	2525	1.40
10			600		1988	2795	1.40
11			600		2184	3066	1.39
12			600		2380	3337	1.39
13			600		2576	3607	1.39
14			300		2772	3878	1.38
15					2968	4148	1.38
Total	*40*	*2350*	*9500*	*106*	*23 899*	*33 649*	*1.4*[c]

[a] Deployment year relative to hypothesized first year of deployment, assumed here to be 2033.
[b] NRE = Non-Recurring Engineering.
[c] Weighted average = $1402/$tSO_2$, unweighted average = $1422/$tSO_2$.

Table 6. Sensitivity of average cost per deployed t SO_2($), total cost for 15 years (billion $), and total development costs (billion $) to various input assumptions.

	Cost/tSO_2 ($k)	%Δ	Total 15 years costs ($b)	%Δ	Total pre-start costs ($b)	%Δ
Base case (RCP 6.0)	1.40		35.8		3.6	
RCP 4.5	1.43	2%	22.7	−36%	3.2	−11%
RCP 8.5	1.39	−1%	55.5	55%	4.2	17%
Forcing target of 0% change	1.38	−1%	68.5	91%	4.6	28%
Sulfate sensitivity * 150%	1.42	1%	24.9	−30%	3.3	−9%
Sulfate sensitivity * 50%	1.38	−1%	68.5	91%	4.6	28%
Aircraft NRE * 200%	1.50	7%	38.3	7%	5.9	65%
Aircraft manufacturing * 150%	1.57	12%	40.7	14%	4.1	15%
Aircraft manufacturing * 50%	1.24	−12%	30.8	−14%	3.0	−15%
Fuel cost * 200%	1.77	26%	44.7	25%	3.6	1%
Fuel cost * 50%	1.22	−13%	31.3	−12%	3.6	−1%
All other ops cost * 150%	1.80	28%	45.3	27%	3.6	1%
All other ops cost * 50%	1.00	−28%	26.2	−27%	3.6	−1%
Sulfur cost * 200%	1.45	4%	37.0	3%	3.6	0%
Sulfur cost * 50%	1.38	−2%	35.2	−2%	3.6	0%

Pre-start costs depend primarily on the aircraft development budget.

5. Conclusion

Solar geoengineering is often described as 'fast, cheap, and imperfect' (Keith *et al* 2010, Mahajan *et al* 2018). The deployment scenario laid out here assumes the first, though it clarifies that 'fast' in this context refers to the immediacy of the impact that would derive from deployment, not necessarily the ramp up to deployment, which, short of a military-style crash deployment scenario outside this present study, would require several years. This paper further confirms 'cheap,' but says nothing about 'imperfect.' We here make no judgment about the desirability of SAI. We simply show that a hypothetical deployment program commencing 15 years hence, while both highly uncertain and ambitious, would indeed be technically possible from an engineering perspective. It would also be remarkably inexpensive.

Total pre-start costs to launch a hypothetical SAI effort 15 years from now are ~$3.5 billion in 2018 US $. A program that would deploy 0.2 Mt of SO_2 in year 1 and ramp up linearly thereafter at 0.2 Mt SO_2/yr would require average annual operating costs of ~$2.25 billion/yr over 15 years. While these figures include all development and direct operating costs, they do not include any indirect costs such as for monitoring and measuring the impacts of SAI deployment, leading Reynolds *et al* (2016) to call SAI's low costs a solar geoengineering 'trope' that has 'overstayed its welcome'. Estimating such numbers is highly speculative. Keith *et al* (2017), among others, simply takes the entire US Global Change Research Program budget of $3 billion/yr as a rough proxy (Our Changing Planet 2016), more than doubling our average annual deployment estimates.

Whether the annual number is $2.25 or $5.25 billion to cut average projected increases in radiative forcing in half from a particular date onward, these numbers confirm prior low estimates that invoke the 'incredible economics' of solar geoengineering (Barrett 2008) and descriptions of its 'free driver' properties (Wagner and Weitzman 2012, 2015, Weitzman 2015). Dozens of countries would have both the expertise and the money to launch such a program. Around 50 countries have military budgets greater than $3 billion, with 30 greater than $6 billion (Stockholm International Peace Research Institute 2017).

6. Further discussion

SAI's low cost and its resulting 'free driver' properties often invoke considerable discomfort (Burns *et al* 2016, Lawrence and Crutzen 2017). While largely unstated, one possible concern that may underlie this discomfort is what one might call a deployment program's presumed 'secret driver' properties: the prospect that deploying SAI even at scale could remain undetected (Dalby 2014, Hamilton 2014, Stilgoe 2015).

Focusing on a mature 5 Mt SO_2 yr^{-1} deployment program, Lo *et al* (2016) surveys methods by which to detect the deployed aerosol particles. Instead, we here focus on the detectability of the deployment program. We would argue that dozens of large aircraft flying many thousands of flights annually from multiple bases in several countries would make such a program easily detectable. This may apply even in year one. A hypothesized deployment of 0.2 Mt SO_2 yr^{-1} involves an initial fleet of 8 purpose-built aircraft flying ~4000 flights, far too much flight activity to remain undetected.

Moreover, while the longitude of injection matters little, recent studies show the vastly improved efficacy of SAI at diverse latitudes. Our hypothetical scenario assumes bases at 30 °N, 15 °N, 15 °S, and 30 °S. No country comes close to spanning such territory. In the Americas, representative base locations would include Houston at 29.8 °N and the northernmost tip of Uruguay at 30.2 °S. African bases would span nearly the entire continent, with Cairo, Egypt, at 30.0 °N and Durban, South Africa, at 29.9 °S. Any such operation would require coordination among several countries in both northern and southern hemispheres, further defeating the prospect of it remaining a secret.

All of the above however assumes a rational actor seeking to implement a scientifically sensible SAI program in a reasonably cost-efficient fashion. Might a less cautious or transparent actor still deploy SAI from a single equatorial island in the middle of the Pacific and evade detection? Such an actor would either deploy directly overhead, or fly to the previously proposed latitudes and deploy there. Either scenario creates a serious tradeoff between operational efficiency and costs on the one hand, and purported 'secrecy' on the other. Less efficiency for direct SAI deployment above the equator (Dai *et al* 2018) implies substantially more deployed payload for the same climate impact. More payload requires more or larger aircraft and more flights, making the program more easily detectable. Meanwhile, launching operations from one base but injecting at or near 15 and 30.0 °N and °S requires flight legs of as much as 2000 miles north and south over international waters, if not foreign airspace. This, in turn, will lengthen flights and roughly double both the number of aircraft required and the overall cost of the deployment regime, making the likelihood of a secret program more remote.

Moving from means to motive, while we hope to demonstrate here that no global SAI program could reasonably expect to maintain secrecy, we also consider who if anyone might wish to implement such a covert program. For one, SAI is global in effect, if not implementation (Keith 2000). SAI, thus, is not a precision weapon. Moreover, judged purely by commercial motives, it is difficult to see how one might utilize SAI solely for local gain without triggering substantial global spillovers, both negative and positive. While there might be a long list of contractors who would eagerly bid to vend hardware, supplies, and services to an SAI endeavor, and there might even be a role for patents along that supply chain (Reynolds *et al* 2017, 2018), we believe strongly that commercial profits must not be a motivating factor in any decisions about whether, when, where, and how to implement SAI. Any entity that intends to engineer the climate of the entire globe must act—and be seen to act—purely out of humanitarian and environmental considerations unclouded by aspirations of direct financial gain.

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Competing interests

WS began work on this analysis independently. He subsequently became a donor of Harvard's Solar Geoengineering Research Project, co-directed by GW.

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