Founders Pledge

A guide to the changing landscape of high-impact climate philanthropy

JOHANNES ACKVA
LUISA SANDKÜHLER
VIOLET BUXTON-WALSH

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Executive Summary

Climate action and philanthropy have changed rather dramatically over the last year.

2021 has seen an unprecedented effort to include climate spending in an ambitious infrastructure package in the United States, accompanied by a ~50% increase of private cleantech investment. Internationally, apart from traditional climate leaders such as the EU and the UK, other countries have raised their ambition, with China's (late 2020) commitment to reach net-zero in 2060, and India's commitment to achieve the same goal in 2070.

Climate philanthropy by foundations was at roughly **2B last year** and is poised to increase significantly, **probably almost doubling this year**, with large new pledges (such as Bezos' Earth Fund) coming into effect. Traditionally, individual giving has dominated climate philanthropy, putting the total closer to **5-10B last year**, with a significant increase expected for this year as well.

This guide is about what we believe the implications of this changing landscape to be for donors that are motivated by maximizing positive climate impact. It's intentionally a "guide", not a research report, utterly oriented towards action-relevance but deeply informed by data and relevant scientific facts.

We structure this guide in three parts.

Part I provides a mapping of the climate action and philanthropy space, introducing key facts as well as key uncertainties that characterize the terrain and that, taken together, hopefully provide some clarity in a confusing and dynamic space.

Part II is about strategy. Given the landscape and how it has changed, what should we do?

Part III explains our grantmaking through the Founders Pledge Climate Fund implementing this strategy. However, our intent is much broader than explaining our own grantmaking, and Part I and II do stand alone and hopefully provide useful observations and principles for other philanthropists, who might come to other conclusions regarding specific grants.

Part I: Key facts and uncertainties

Key facts

Climate change is an incredibly complex challenge and the climate space is dynamically evolving. **To see through the confusion and be able to act, it is important to distill the most important *action-relevant* facts**, key characteristics of the challenge that are deeply influential in shaping what the most effective philanthropic actions are. We focus here on what we believe to be the three most important stylized facts about climate:

1. Surge in climate philanthropy:

The first one, already alluded to, is that climate philanthropy has been growing rapidly over the last couple of years, and again this year. While the climate space is vast and will eventually be able to absorb this influx of money well -- there is enough to do -- this surge makes it likely that the average open funding margin is less impactful than a couple of years ago and, by implication, that **one has to be more strategic to find opportunities where funding makes a large counterfactual difference**, both in (i) being truly additional as funding (rather than, say, being filled a week later by someone else) and also (ii) in being truly additional as activity (rather than, say, fund the 1001st voice making the same argument in a particular policy debate).

2. Climate attention is not, by and large, where future emissions are:
While the surge in climate action and philanthropy is certainly good news and there are spaces where additional funding adds little in terms of additional impact, **there are still severely neglected spaces** where we should expect lots of future emissions but where right now there is fairly little attention.

To understand what is currently neglected, we built on the foundational work of ClimateWorks (2021), including commitments up to 2020, but added in major new commitments from this year using their categorization scheme. We particularly focus on including the two largest commitments we are aware of from this year, Bezos Earth Fund, pledging 10bn for climate action this decade, as well as the Global Energy Alliance for People and the Planet (of which the Bezos Earth Fund is a member) pledging 2.5 B over 5 years for renewables in developing countries. As many of the grants are quite new with little public information, we include significant uncertainty bars in our full estimates.

Figure 1 below shows very significant changes in annualized estimates of climate philanthropy through new major commitments in 2021 compared to 2020:

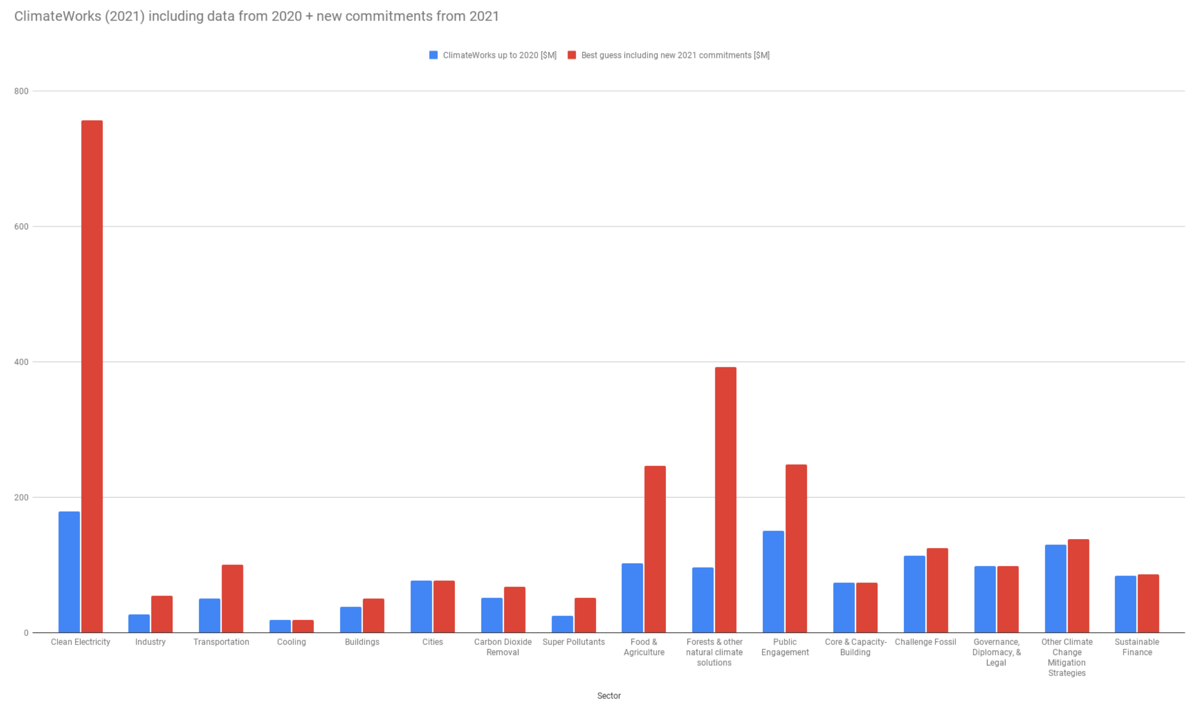


Figure 1: Distribution of climate philanthropy by sector, based on ClimateWorks (2021) and own analysis

Figure 2 provides the same by region though we are very uncertain about the distribution of funding of the Global Energy Alliance for People and the Planet amongst non-OECD economies, so we below focus on OECD/non-OECD comparisons:

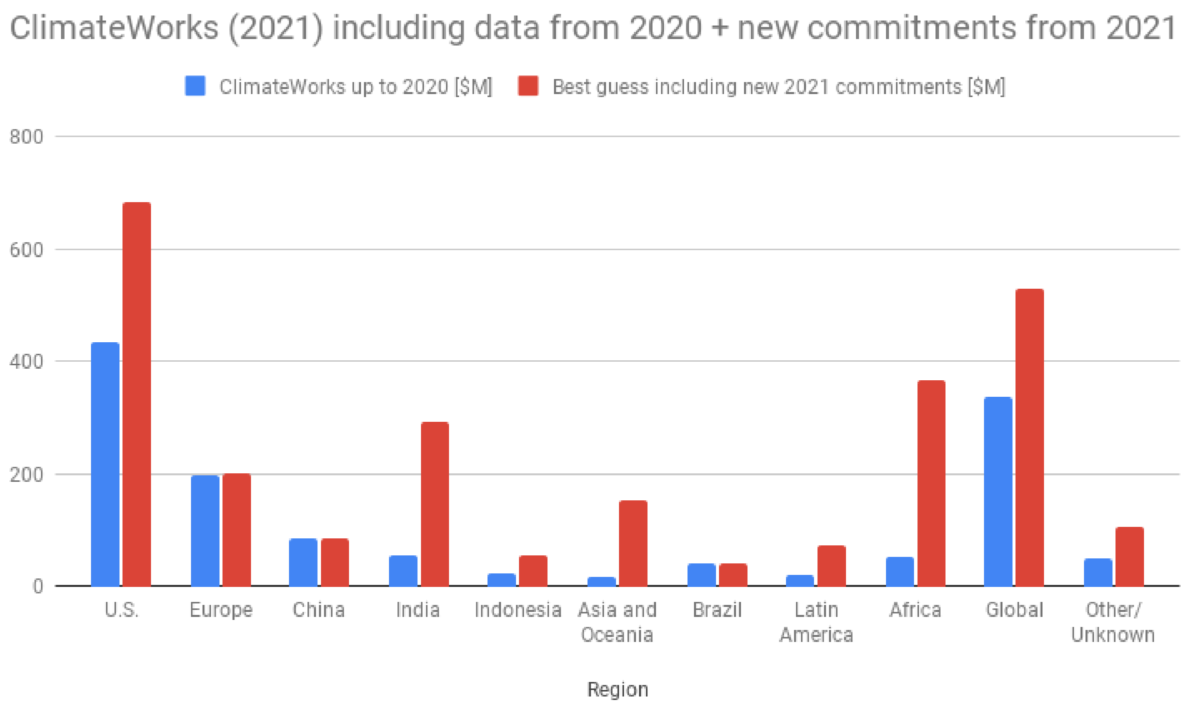


Figure 2: Distribution of climate philanthropy by country, based on ClimateWorks (2021) and own analysis

We believe the following key findings from the data warrant special highlighting for climate philanthropy prioritization:

- **(1) Philanthropic funding heavily skews towards clean electricity rather than other sectors, in particular it continues to pay little attention to hard-to-decarbonize sectors**. While there is some uptick in philanthropic funding for those sectors such as industry[1] and heavy-duty transport through Bezos Earth Fund commitments, these are dwarfed by other increases. Insofar as philanthropy sets the agenda for civil society and policy, this is worrisome as it continues the

[1] Our best guess including new 2021 commitments shows that 2x more money is going into industry compared to 2020 (making it around 2 percent of total philanthropic climate spending).

overemphasis on relatively mature and popular technologies rather than focusing on those parts of the economy that are not yet trending clean which need more attention the most.

- **(2) While there is a lot of philanthropic funding for clean electricity, this is almost entirely focused on renewables**, with minimal contributions for other clean electricity sources such as nuclear power. While the ClimateWorks numbers do not disaggregate between different clean electricity sources, it is clear that most of this is focused on renewables and the additions from the Bezos Earth Fund and Global Energy Alliance for People and the Planet are exclusively focused on renewables.[2]

- **(3) Topics that have been at the forefront of public attention in 2020 and 2021 or that are generally popular profit disproportionately from the funding surge** , this is true for **forestry and other natural climate solutions** that have received a large share of existing commitments from the Bezos Earth Fund[3] and it is **true for groups focused on building attention for climate action and/or for increasing priorly quite neglected environmental justice** which have been another major focus of initial Earth Fund grants. It is also **increasingly true for work on super pollutants such as methane,** given recent surging policy attention to the issue.

- **(4) There is somewhat more attention to innovation and innovation advocacy than before**, with some of the aforementioned grants particularly focused on innovation in hard-to-decarbonize sectors and commitments from Bezos Earth Fund to Breakthrough Energy and Breakthrough Energy Action. We take this into account in our grantmaking, assuming that innovation advocacy in the US is *less*

[2] Making the conservative assumption that 25% of clean electricity spending from the annualized climate philanthropy data up to 2020 was for nuclear power, the share of nuclear power including new major commitments in 2021 (with zero spending for nuclear power) reduces to around 6%..
[3] Around 15% from our best guess for annualized spending.

neglected.

- **(5) About 35% of climate philanthropy is targeted at the US, about 10% at Europe**, for a total of about **45%**, despite those regions representing **at most 15%** of future emissions. While this focus was more severe before, ClimateWorks (2021) numbers suggested that 66% of geographically assignable climate philanthropy were focused on the US, Canada and the EU, it still suggests that there is a heavy focus on those regions. **Given that those numbers reflect foundation data** and that individual donors dominating climate philanthropy are probably more focused on domestic initiatives than foundations, **this is almost certainly an underestimate of the true differential in geographic distribution of funding**.

The data therefore show that climate philanthropy is disproportionately directed at the US and EU, even though these regions are together responsible for at most 15% of 21st century emissions. Yet, this is not necessarily a misallocation given that those jurisdictions also command a much higher share of climate attention, societal resource mobilization, and innovation capacity than the rest of the world.

This means climate philanthropy in those regions should primarily be judged by the degree to which they improve the climate response in those jurisdictions to facilitate global decarbonization, as the indirect effects of climate action in the US and EU can easily be 10x larger than their domestic effects.[4]

[4] If this sounds hyperbolic, consider that small to medium-sized jurisdictions such as Denmark, Germany and California have been able to drive decarbonization outcomes -- via driving cost reductions and technology improvements in wind, solar, and electric cars -- that went far beyond their domestic emissions. E.g. Gerarden (2017) estimated that "32% of the global solar adoption due to increased technical efficiency would not have occurred in the absence of German subsidies", while the virtuous cycle set in motion by these policies will eventually save Gigatons of emissions globally every year, while -- at most -- saving hundreds of millions of tons in Germany (see here for a more detailed version of this argument).

The data also shows that some sectors and technologies, albeit less so, remain quite neglected -- this is true for sectors such as industry and heavy-duty transport and solutions such as advanced nuclear, carbon removal beyond nature-based solutions, and carbon capture and storage. By and large, climate philanthropy bets on the success of renewables and nature-based solutions and pays less attention to less-popular alternatives.

3. Non-linearity of climate damage:

The third crucial fact is that **climate change gets increasingly unmanageable as it gets more extreme.** While a world of 1.5 degrees would not be *that* different from today, a world of 3 degrees of warming would pose significantly more challenges, with risks being a lot more pronounced still at 6 degrees:

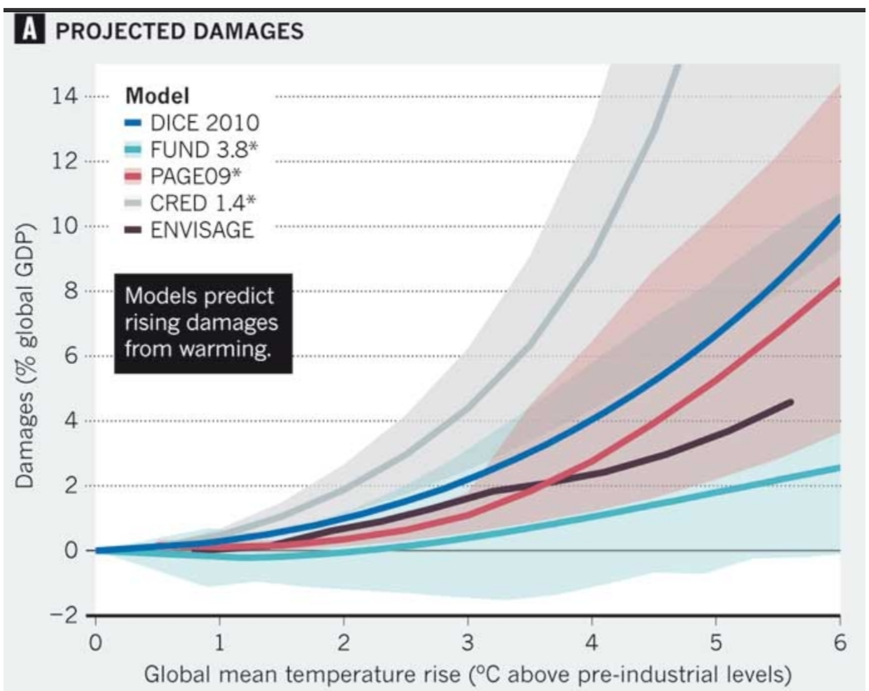


Figure 3: Some estimates on the non-linearity of climate damage (taken from Revesz et al. 2014)

While the displayed models have been heavily criticized for likely underestimating climate damage (and we agree), what matters here is **not the level of climate damage, but the shape of it.** On the median model of this set (PAGE09), climate damage increases about 5-fold from 1.5 to 3 degrees and about 8-fold from 3 to 6 degrees. Importantly, this basic pattern of increasing damage holds across all of those models and is also consistent with many other approaches to estimate climate damage, it's a robust stylized fact.

This leads to what might be a surprising conclusion -- **the goal of high-impact climate philanthropy is *not* to maximize emissions reductions but to minimize climate damage.**

While this sounds technical, it has profound practical implications for high-impact climate prioritization: if we can, it is *much* more important to shift from 5 to 4.5 degrees if we are in a 5-degree scenario than it is to shift from 3 to 2.5 degrees in a 2-degree world.

Because we know that certain combinations of events are unlikely, for example ending up in a 5 degree world *and* current mainstream approaches succeeding, **this non-linearity also informs our actions in the face of uncertainty** -- putting more resources into solutions that could be vital if current mainstream solutions fail.

Key uncertainties

Every high-impact strategy to take action on climate, philanthropically or otherwise, **carries significant uncertainty** about outcomes ("*How many emissions are avoided?*"[5]). This makes it fundamental to think clearly about the role that uncertainties play in our decision-making.In this section, we make five important points about how to think about and deal with uncertainty:

[5] Or removed, in the case of carbon removal.

- **(1) Avoiding uncertainty carries a heavy cost** -- certainly having little impact is much worse than probably having much larger impact.

- **(2) Large uncertainty does not imply ignorance** -- even when we are quite uncertain this is not equivalent to having no action-relevant information.

- **(3) What matters in guiding action** on climate is *not* being precisely right about impact (cost-effectiveness) which is impossible, but rather **being roughly right about relative impact** (cost-effectiveness) which is decisive for prioritization and taking action.

- (4) When **strategies are characterized by many uncertainties**, it is important to **consider how they are related** -- whether they are independent of each other (multiplication works) or correlated (scenarios need to be considered jointly).

- (5) Given the non-linear increase of climate damage, **when being wrong it is much worse to be too optimistic than to be too pessimistic**.

This leads us to a principle we call **robust diversification** -- when diversifying (i) doing so that the uncertainties are **negatively correlated** and (ii) paying special attention to **robustness against the worst worlds**, where uncertainties resolve in ways that climate damage is maximal (and, hence, additional effort particularly valuable).

We apply this principle to four different uncertainties about **(a) the degree to which we are already set on a low-carbon trajectory**, **(b) the adequacy of existing solutions** and the related need for additional innovation, **(c) the degree to which carbon lock-in prevents the diffusion of new technologies** (i.e. limiting the potential of innovation), and

(d) whether or not the current favorable geopolitical situation and climate attention will persist.

This leads us to favor strategies that assume that a lot of progress is needed still **(a,b)**, i.e. accelerating innovation, because being too pessimistic is better than being too optimistic, It also leads us to complement innovation with efforts to avoid carbon-lock-in **(c)** as the uncertainties about both strategies are negatively correlated (innovation is the least-effective when carbon lock-in is severe, and vice versa) and to prioritize solutions **(d)** that do not assume lots of international coordination and/or willingness to pay for climate action, given that it is quite plausible that the situation will deteriorate and this is where most climate risk is concentrated (i.e. where additional mitigation is most valuable).

Part II: Strategy

We think of high-impact theories of change (strategies) as combining several impact multipliers, so after explaining what we see as the most important impact multipliers in climate we explain promising theories of changes each leveraging several of those multipliers.

Impact Multipliers

Given both the daunting nature of the challenge as well as the current surge of funding in the space, when seeking to maximize impact in the climate space, we believe it is fundamental to look for "***impact multipliers*", reasons to expect that a given funding allocation will have an above-average impact** by filling blindspots and leveraging effective mechanisms. In the main text, we explain those multipliers in detail, including the reasons why we think they provide above-average impact and what common objections and our responses are.

We think there are **three primary sources of outsized impact in the current climate funding space.**

1. The first one is about **complementing rather than copying the behavior of other donors** to maximize the probability that your efforts are additional and in spaces where there are low-hanging fruits.

This means focusing on approaches, technologies and regions that are **neglected**, underserved compared to their relative importance.

This means supporting technologies such as (a) carbon capture and storage (CCS), carbon removal and advanced nuclear that receive a small fraction of climate philanthropy and are often perceived negatively, (b) paying more attention to the hardest parts of the decarbonization challenge, such as industry, heavy-duty transport, and (c) expanding to regions where climate philanthropy, to date, is very low compared to future emissions, such as India, Southeast Asia, and Sub-Saharan Africa (but, crucially, not in ways restricting energy access).

There are also **three corollaries to neglectedness that are not about the substantive focus of funding, but about the kind of funding:** From many interactions with donors and charities and other data points, we find that too much funding is risk-averse, impatient (focused on short-term wins) and focused on specific projects.

This gives rise to the expectation that many projects that are risky but worthwhile and/or delayed in their effects are not funded, giving an impact multiplier to donors that are willing to be **patient** and **risk-neutral**. For similar reasons, we also think it is good to give unrestricted funding and to **conceive of "overhead" not as negative, but as a positive organizational multiplier** -- an investment that allows a charity doing great work to invest in doing this work more efficiently and attracting more funding.

2. The second one is not about donors and funding levels, but rather about **utilizing substantive facts and mechanisms of the climate space to maximize impact**.

The first one here is about engaging around **trajectory changes**, in situations that are pivotal because they set in motion dynamics that will self-reinforce (positive feedback loops).

Examples of this can be positive, such as **virtuous cycles** of increased investment enabling cost reductions leading to further investments, as seen with wind, solar and electric cars, where early investments were arguably trajectory-shaping. But they can also be negative, such as **carbon lock-in** where long-lived assets, infrastructure investments and the political economy they give rise to commit emissions streams decades into the future.

Another impact multiplier consists in **ensuring policy additionality**, that funded efforts do not simply make it easier to reach policy targets that would be reached anyway (in which case additionality would be zero).

3. The third one is about the **type of work** to fund.

While we discuss many objections in the main text, we firmly believe that -- on balance -- funding **advocacy**, efforts to induce policy change and affect how societal resources are spent, provides the most compelling proposition for impact-oriented philanthropists.

This is so for a number of reasons, such as the vast scale difference between philanthropy and public spending (leverage), the necessity of policy change and its ability in triggering private action (causal primacy), and the abstractness and intangibility of advocacy that make it likely to be relatively underfunded.

Theories of Change

A *lot* of theories of change make sense at face value, i.e. are internally consistent and seem convincing. Thus, when prioritizing between different theories of change and interventions that could receive support, **we need to prioritize based on observable and comparable characteristics to identify high-impact strategies**. That is why, apart from examining theories of change in detail on a mechanism level, we primarily rely on exploiting systematic features of the climate space and the interventions to make relative judgments in an uncertain world.

In other words, **we evaluate how different theories of change perform alongside the identified impact multipliers to give rise to expectations of particularly high impact.**

We currently have identified four theories of change that we expect to be particularly promising, as summarized in Table 1:

Theories of change >	**Policy Leadership and Paradigm Shaping**	**Accelerate innovation of neglected-yet -critical technologies**	**Advocacy to avoid carbon lock-in in emerging economies**	**Accelerate the growth of promising organizations**
Rationale	The diffusion of policy ideas is one of the few ways in which countries with a small share of emissions can have outsized	Between ⅓ to ⅔ of technologies for deep decarbonizatio n are not ready (or economical) for mass deployment	The majority of 21st century emissions will come from emerging economies and many of those emissions are	In a field with surging funding it is important to invest in the growth of promising organizations to reduce

	impact.	requiring additional innovation effort, yet attention is overwhelmingly focused on relatively mature and popular technologies.	“locked-in” for decades through investments in infrastructure and long-lived assets, yet they receive fairly little attention by climate philanthropy.	declining impact of additional donations, also the surge of funding makes supporting growth less risky.
Mechanisms of theory of change	1. Identify important policy ideas in need of amplification 2. Amplify those ideas 3. Higher likelihood of policy implementation and related emissions reductions	1. Fund advocates for innovation in neglected technologies 2. Policy change or improved allocation of budgets 3. Improved innovation outcomes (cost reductions, performance)	1. Fund advocates in jurisdictions with high leverage on future emissions 2. Avoid carbon lock-in and instead lock-in lower carbon trajectories 3. Reduced emissions	1. Accelerate and de-risk the growth of promising organizations. 2. Additional emissions reductions from the organization’s work & crowding in of other donations

		4. Global diffusion of new technology and emissions reductions		
Impact Multipliers leveraged				
Theories of change >	**Policy Leadership and Paradigm Shaping**	**Accelerate innovation of neglected-yet-critical technologies**	**Advocacy to avoid carbon lock-in in emerging economies**	**Accelerate the growth of promising organizations**
Neglectedness		X	X	NA
Trajectory changes (other than innovation)	X		X	X
Global diffusion of technological change (innovation)		X		NA
De-risking of investments				NA
Risk Neutrality	X	X	X	X

Patience	X	X	X	X
Advocacy	X	X	X	NA
Catalytic growth				X
Policy additionality	X	X	X	NA

Table 1: Theories of change and leveraged impact multipliers

Part III: Our Grantmaking

In line with this analysis of the situation and strategy, we have made the following grants late last and this year:

We deployed USD 850,000 to the Clean Air Task Force (CATF) and USD 400,000 to Carbon180 directly after the Biden victory to enable those organizations to optimally engage with the incoming administration and utilize the momentum to push for innovation in neglected technologies, based on our analysis of the special opportunity for climate impact under a Democratic President in a political environment with unusual willingness to spend boldly in the wake of COVID-19.

While the bipartisan infrastructure bill has become law, the "Build Back Better Plan", the Democrats-only climate and social spending bill, has not yet passed the Senate, not allowing for a final analysis of impact of those grants and our predictions. **However, from**

our intermediate understanding, we believe that the grants have been quite successful.

Several of Carbon180's policy suggestions have recently been taken up by US policymakers. C180 recommended that the Department of Energy launch an initiative to reduce the cost of carbon removal to $100 per ton and recommended that appropriations for carbon removal be significantly increased. Both ideas were implemented: the recently enacted infrastructure bill contains 3.5 billions in new funds for direct air capture (DAC) efforts.[6] Similarly, the infrastructure package reflects many of CATF's priorities, such as increased support for carbon capture and storage (CCS) and hydrogen infrastructure, industrial decarbonization, and advanced nuclear demonstration, with a total of USD 30bn for clean energy provisions championed by CATF. Because both Carbon180's and CATF's foci are overall fairly neglected on the political left, there are few fervent advocates for carbon removal, CCS, and advanced nuclear, it is plausible that these organizations had significant impact in the provisions of the infrastructure bill they worked on. We will provide a more detailed and rigorous retrospective grant analysis once the Build Back Better Plan has passed as well (in 2022).

While we examined other grant opportunities in the wake of Biden's win and the Democratic win in Georgia, it became our impression that this space was increasingly saturated and that additional money focused on short-term wins would not have large additional impact, that organizations doing incredibly important work had sufficient resources to do so.

We also observed a strong uptick in US-centric and a moderate uptick in innovation-focused philanthropy and advocacy (including Bill Gates's *How To Avoid A Climate Disaster* and commitments from Bezos's Earth Fund to innovation in hard-to-decarbonize sectors), which led us to broadening our scope exploring other theories of change, in particular around avoiding carbon lock-in in emerging economies.

[6] In 2015, C180 was among the first organizations to advocate for DAC to be eligible for 45Q, the federal tax credit for carbon sequestration, and has consistently worked to raise the value of DAC under that program.

This has led to a large **organizational investment and globalization grant** for the CATF which is focused on allowing CATF to become a truly global organization, with new presences in China, India, the Middle East and North Africa, Southeast Asia as well as a strengthened presence in Sub-Saharan Africa. While a long-time grantee of ours, **this grant evaluation was driven by a fundamentally different rationale than prior investigations** (and grants), not focusing on innovation advocacy in OECD economies, but rather in supporting trajectory changes and avoiding carbon lock-in in those regions of the world where energy demand growth is concentrated.

We are also investigating further grants under this theory of change, focused on **co-benefits of air pollution and climate advocacy in Southeast Asia** (Clean Air Asia) and **accelerating mature clean** technologies, such as solar PV, **through strengthening cleantech ecosystems** in emerging economies (New Energy Nexus). We currently believe that a fair amount of our future grantmaking will be concentrated in emerging economies, in particular if -- as is to be expected -- the US political opportunity somewhat dries up after the 2022 midterms.

While we believe that the US climate policy debate has become significantly more innovation-oriented, this is far less true in Europe. This is why we are **excited to scale a new organization, Future Cleantech Architects (FCA)**, to help positively shape German, European, and global debates on innovation priorities. Over the past year, after being approached as advisors, we have closely observed this organization have had impressive initial successes[7] and we are now ready to invest in its ambitious growth, **supporting the organizational development as well as key programs in hard-to-decarbonize sectors requiring more innovation, namely zero-carbon fuels, industry, long-duration storage, and carbon removal technologies**. We believe that if FCA is successful this

[7] Such as conducting and publishing a cleantech R&D priorities survey through the World Economic Forum and the TEC committee of UN Climate Change, hosting a cleantech innovation call with three UN organizations and presenting key neglected R&D needs in two events at COP 26 in Glasgow as well as taking the only European perspective in the release of ITIF's 2021 Energy Energy Innovation Index.

could significantly improve the German and European climate policy response, while -- at the same time -- this kind of organization is much rarer in Europe than in the United States.

Similarly to FCA in terms of scale, we believe that **TerraPraxis continues to do incredibly important work around shaping a conversation for advanced nuclear to address critical decarbonization challenges,** such as the decarbonization of hard-to-decarbonize sectors and the conundrum of how to deal with lots of very new coal plants that are unlikely to be prematurely retired.

Yet, as a very small organization and with a relatively small pro-nuclear funding landscape, TerraPraxis has not been able so far to scale to its full potential. For this reason, we not only invested in Terra Praxis's programmatic work, but also in its organizational capacity.

In the wake of COP26, we also made a time-sensitive grant to the EEIST project helping a critical argument about how traditional cost-benefit analysis underestimates the innovation returns of seemingly extremely expensive policies (such as early deployment subsidies) reach 4M people through a professional PR effort (more details here, #Grant 1).

As we head into 2022, we will continue to deepen our research and grantmaking, trying to find the best opportunities by analyzing the funding landscape, identifying and evaluating new theories of change and finding and funding opportunities we perceive as bottlenecks and blindspots of the current climate response.

Key facts and principles guiding our thinking

We will first lay out the key facts guiding our current thinking as well as our key uncertainties and how we think about being robust to them before we explain the grantmaking in the second part of this report.

The goal here is maximal reasoning transparency, making it clear why we believe what we do and how this informs our actions. We also try to give a balanced consideration of counter-arguments.

While we have discussed many of those facts and uncertainties in prior writing, the goal of this document is to be a complete description of our current thinking in one place. We hope it is easy for readers familiar with our prior work to find those sections that contain significant updates and extensions of our prior work,

Three crucial facts

Climate change is an incredibly complex challenge and the climate space is dynamically evolving. **To see through the confusion and be able to act, it is important to distill the most important *action-relevant* facts**, key characteristics of the challenge that are deeply influential in shaping what the most effective philanthropic actions are.

This is the goal of this section, describing key features of the challenge from the perspective of impact-oriented philanthropy, seeking to make the largest positive difference with additional dollars.

As with all attempts at mapping a space, this involves simplifications -- but the goal is very much to simplify in ways that make the problem tractable and that is sufficiently accurate to not lead decision-making astray.

We focus here on what we believe to be the three most important stylized facts about climate.

1. Attention to climate is increasing strongly

2021 has seen an unprecedented effort to include climate spending in an ambitious set of infrastructure packages in the United States, accompanied by a ~50% increase of private cleantech investment. Internationally, apart from traditional climate leaders such as the EU and the UK, other countries have raised their ambition, with China's (late 2020) commitment to reach net-zero in 2060, and India's commitment to achieve the same goal in 2070.

Climate philanthropy by foundations was at roughly **2B last year** and is poised to increase significantly, **probably almost doubling this year**, with large new pledges (such as Bezos' Earth Fund) coming into effect (also, see here). Traditionally, individual giving has dominated climate philanthropy, putting the total closer to **5-10B last year**, with a significant increase expected for this year as well.

Moreover, ultimately what matters is not primarily the amount of philanthropy, but the amount of overall societal resource allocation. While quite hard to specify exactly, global climate spending now is close to **1T/year**[8] and climate is a primary political concern, certainly in Europe, in key US states, and amongst Democrats federally.

[8] The number here is 630B but does not include recent uptick due to COVID-stimulus spending and also excludes other categories, e.g. as far as I can tell, spending on nuclear power is not counted.

This is good news, the world is paying more attention. Much overlooked, this is not only true for climate attention, but **also true for our climate outlook**, extreme climate change scenarios are now a lot less likely than they were thought to be, as emissions forecasts are corrected downwards and uncertainty around climate sensitivity is reducing at the tails as well.

It also means that **impact-oriented philanthropists need to be more strategic**, as more of the low-hanging fruits for impact are being picked and the influx of funding means that opportunities where additional money has a large positive impact are, in expectation, harder to find. Providing key facts and methods to find such opportunities is what the rest of this guide is about.

2. Future emissions are *not* where most of the climate attention is.

Given that **climate change mitigation is a vast cause area, covering the entirety of global economic activity**, it would be quite mistaken to just assume that because climate attention and funding is rising strongly that there are no neglected spaces anymore.

But, unlike in causes that are clearly neglected on the whole -- such as farmed animal welfare -- it requires us to dig deeper to examine whether attention and funding are addressing the biggest levers and where, given existing allocations, additional money is likely to have the highest impact (also see our discussion of Neglectedness as impact multiplier below).

For this purpose, we are analyzing and relating four sources of data:

- **(1) Climate philanthropy by foundations:** Building on the foundational work of ClimateWorks (2021) and extending it to cover major new commitments, we analyze how climate philanthropy is distributed across geographies and sectors.

- **(2) Emissions futures by geography:** Building on the Shared Socioeconomic Pathway (SSP) and Regional Concentration Pathways (RCPs) scenarios used by the IPCC and their national-level downscaling by Guetschow et al. 2021, we examine emissions across a set of world regions we specified based on primary theories of change.[9]

- **(3) Sectoral distributions of emissions:** Building on Davis et al. (2018), we examine the distribution across sectors and expected future sectoral distributions based on differential progress (less "baked-in" progress in hard-to-decarbonize sectors).

- **(4) Data on innovation capacity by geography:** We scale the recent analysis of energy innovation capacity by ITIF (2021) to examine how innovation capacity varies across jurisdictions.

We first review geographic and sectoral patterns individually before considering them jointly.

[9] We differentiate the following regions: EU27, UK, US, OECD Asia, as regions where future emissions are relatively low but innovation capacity / climate leadership potential is high; China and India are their own regions given their significance, Emerging Asia captures part of (Southeast) Asia where emissions are growing strongly and Middle East and North Africa (MENA), Sub-Saharan Africa, and Latin America are the respective geographic regions.

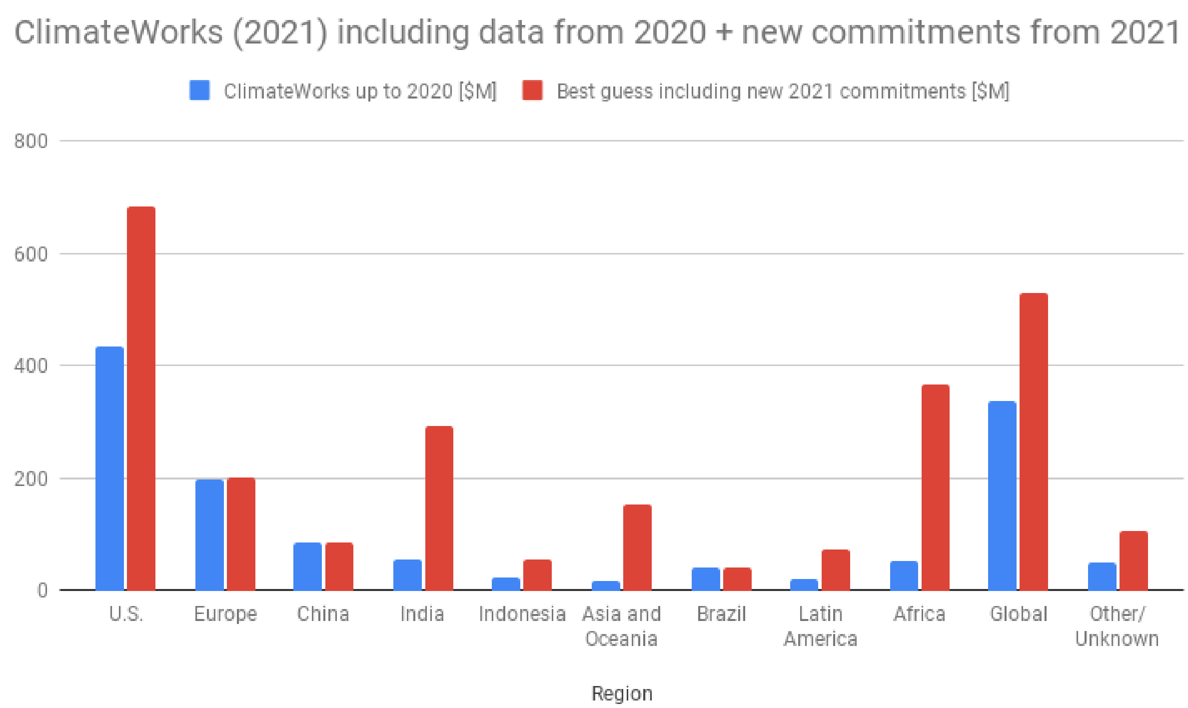


Figure 2: The distribution of climate philanthropy based on ClimateWorks (2021) until 2020 and including our own estimates of major new commitments, including from COP26.

The above figure displays the distribution of climate philanthropy across world regions, including major changes due t0 commitments from Bezos Earth Fund as well as more recently the Global Energy Alliance for People and Planet (including Bezos' Earth Fund) committing USD 2.5 B over five years for renewables in developing countries.

To make these numbers more meaningful, we will first consider how emissions are distributed over those regions this century.

"*Predictions are hard, especially about the future*", the famous saying goes.

Given the many possible emissions futures, we believe that the approach by the IPCC, examining almost thirty different emissions trajectories (combinations of Shared Socioeconomic Pathways expressing different possible political, economic and demographic futures and Regional Concentration Pathways identifying ambition and emissions outcomes, see here for a great explainer) is the best available basis for climate philanthropists to think through the consequences of our actions.

Crucially, this is quite different from many other frameworks, such as estimating effects in the context of a single emissions scenario (the approach traditionally chosen by Project Drawdown) or with reference to an ideal outcome, such as meeting the upper end of the Paris Agreement (1.5C).

We believe this is the right approach for two reasons, (1) the large uncertainty about the trajectory we are on (see "Key Uncertainties" below) and the concentration of climate damage in less likely but much worse worlds (see "Climate damage is non-linear" below).

For example, consider two possible scenarios from the IPCC, the most optimistic SSP1-RCP 1.9, Figure 4, and the most pessimistic SSP5-RCP 8.5, Figure 5:

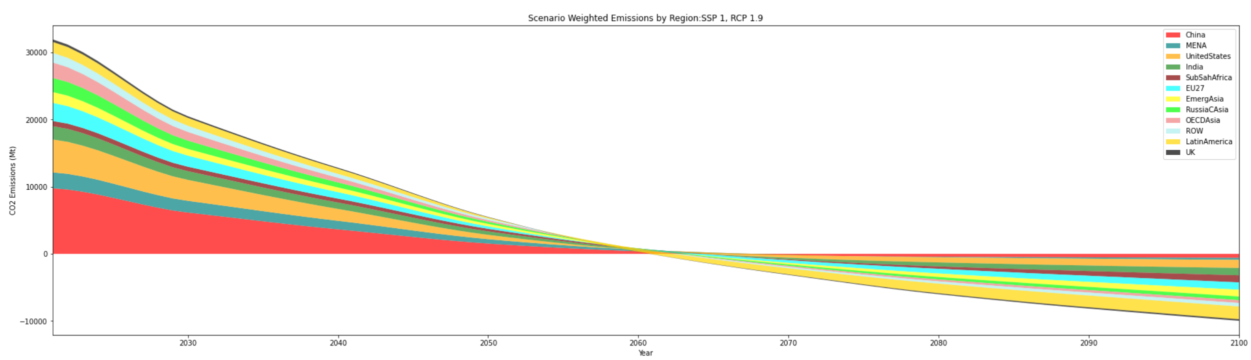


Figure 4: A benign climate future (SSP1-RCP 1.9)

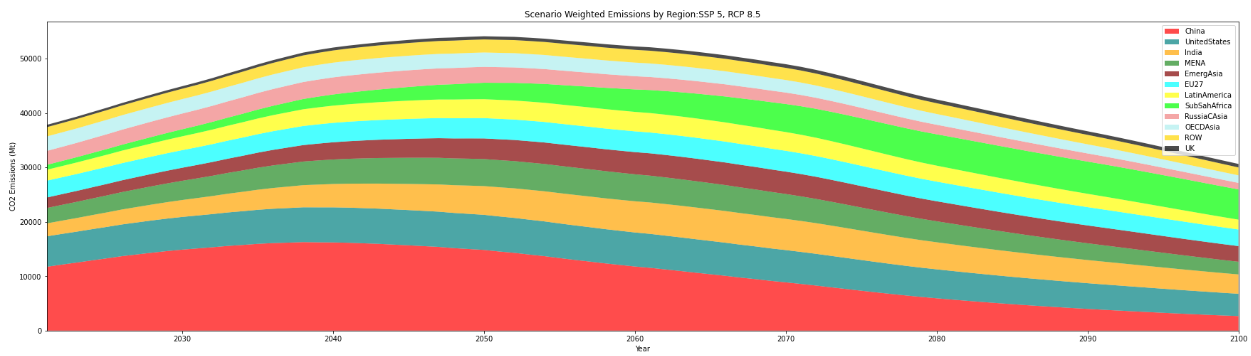


Figure 5: A catastrophic climate future (SSP5-RCP 8.5)

Both of these estimates are probably quite wrong, the first one looks quite a bit too optimistic -- with the entire world moving net-negative around 2050, the second one is hopefully unduly pessimistic with emissions increasing until 2050 and net-zero not being reached this century.

Ideally, when making decisions, we want to consider how they perform against our expectations about a range of possible futures.

Luckily, there is an emerging literature on probabilistic forecasts of future emissions (e.g. Liu & Raftery 2021) or scenarios (e.g. Pielke et al 2021) which we are using to assign probabilities over different scenarios resulting in the following projection (see Background below):

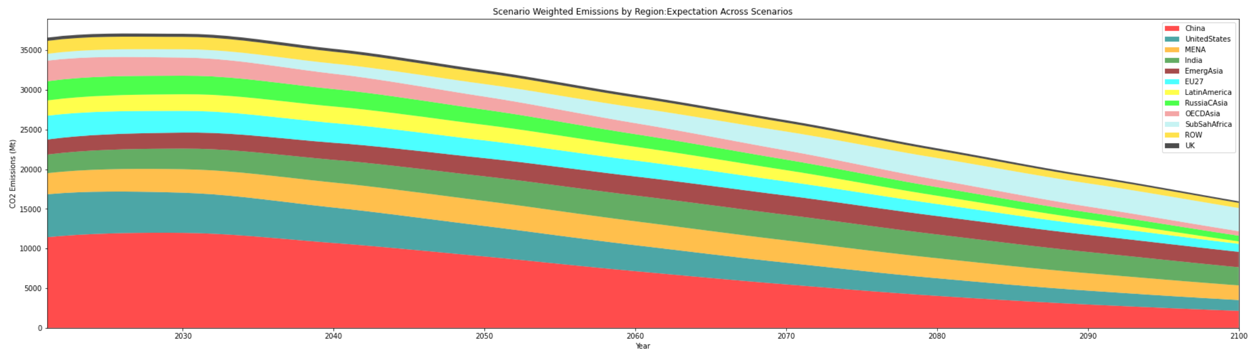


Figure 6: Expected emissions by region

When the global regions plotted above are described as a percentage of cumulative end of 21st century emissions, we see that most of today's highest emitting regions account only for a small fraction of total emissions. Importantly, these contributions also stay fairly constant across the worst case, best case, and expectation, with China accounting for the largest fraction of emissions in every case. In contrast, the emissions contributions of the United States, the United Kingdom, and the European Union are around 12, 1 and 7% of end of century emissions respectively, for a total of less than 20%. Crucially, this is not a point about shying from historical responsibility but about the ability to shape future outcomes given a much larger responsibility.

REGION	**China**	**EU27**	**Emerging Asia**	**India**	**Latin America**	**MENA**	**OECD Asia**	**Rest of World**	**Russia, CAsia**	**SubsahAfrica**	**UK**	**UnitedStates**
Expectation	25	7	8	10	5.3	9.7	5.1	4.3	5.6	7	1.1	11.7
Best case (SSP 1 RCP 1.9)	48	4.9	3	5	-8.7	14.9	7.1	3.6	6.1	1	0.3	15.5
Worst case (SSP 5 RCP	23	7.7	7	10	6.4	8.8	5.1	4.5	5.1	8.4	1.3	12.6

8.5)												
Increase in share in worst case	0.5	1.6	**2.3**	**2.0**	**-0.7**	0.6	0.7	1.3	0.8	**8.4**	4.3	0.8

Table 2: Regional Emissions as a Percentage of Cumulative Global End of Century Emissions under Different Scenarios

There is one other important feature of these statistics worth highlighting -- the strongest differentials between emission shares in best- and worst-case worlds occur in emerging economies with low current per-capita emissions, for example there is an 8-fold difference in the emissions share of Sub-Saharan Africa between SSP 1/RCP 1.9 and SSP5/RCP 8.5 and the differentials are also large for Emerging Asia & India.

This points to an important pattern -- the highest emissions futures and thereby highest climate risk are concentrated in worlds where regions with low current per-capita emissions follow the lead of Western Europe, North America and China in pursuing a fossil-driven growth trajectory. Engagement in those regions now is thus not motivated by reducing emissions in the short-term, but rather by avoiding lock-in into such high-emissions pathways that will result in high net emissions given population and growth trajectories.

Limitations and likely directional bias of these numbers

There are three important limitations of such data worth highlighting, as they bias the data in different ways:

- **Carbon removal (understating affectable emissions in OECD economies):** In most emissions scenarios, OECD economies will reach net-zero first and then go net-negative. This is not reflected in the numbers above and likely somewhat increases the share of affectable emissions in those regions by a bit (a factor of ~2).

- **Policy additionality (overstating affectable emissions in OECD economies):** These emissions scenarios do not reflect emissions commitments by economies. While most major economies now have net-zero targets, those in Western OECD economies, particularly the UK and the EU, are arguably most binding -- enshrined into law (UK, soon EU) and protected by strong environmental movements. Comparatively speaking, net-zero commitments by emerging economies such as China and India are -- understandably -- further in the future, looser, and less protected by forceful domestic constituencies. As such, we should expect that emissions reductions in OECD economies are far less additional (easily by a factor of ~5) because they will only be additional when they enable reaching a target that otherwise had not been met or driving stronger domestic target setting (also see "Policy additionality" below).

- **Trajectories (overstating affectable emissions in OECD economies):** For simplicity, we chose to assign the same scenario probability across regions, yet the literature suggests that OECD economies' emissions trends are more consistent with lower emissions trajectories (lower RCP ranges), whereas the opposite is true for emerging economies (Pedersen et al. 2020).

On balance this means that these emissions shares are very likely overstating the direct territorially affectable emissions in OECD economies, i.e. that the true affectable share of Western OECD economies is probably closer to 10% or less than to 20% of future emissions.

It is no coincidence that this statement comes with a lot of qualifying adjectives which is what we discuss next -- the capacity to indirectly affect emissions streams.

Innovation capacity

One of the most obvious and important lessons from climate progress we have observed so far is that the **global long-run effects of domestic action can outweigh the domestic short-term effects dramatically**.

For example, not being a particularly sunny country and investing early when module prices were extremely expensive, the German feed-in-tariffs for solar in the early 2000s and 2010s barely reduced German emissions, but they have been trajectory-shaping for solar likely, once solar reaches its full potential, shaving off Gigatons of emissions every year.

Similar trajectories have been observed with many other energy technologies, such as the US pioneering and then exporting nuclear power in the 1950s, Denmark promoting wind power and California promoting renewables and electric cars.

In most of those examples, much more cost-effective abatement options would have been available in the short-term -- such as fuel-switching from coal to gas or moderate carbon pricing -- to meet national emissions targets, **yet those investments proved transformative for global decarbonization trajectories** (see the EEIST project for a more thorough treatment of these arguments as well as case studies).

This means that there is at least one other dimension beyond future domestic emissions that we should take into account when considering the importance of different regions for future climate change, their respective innovation capacity to affect global decarbonization.

Here is one metric to capture this dimension, though we seek to consider a broader set of indicators in future work (though the results will be broadly similar)[10]:

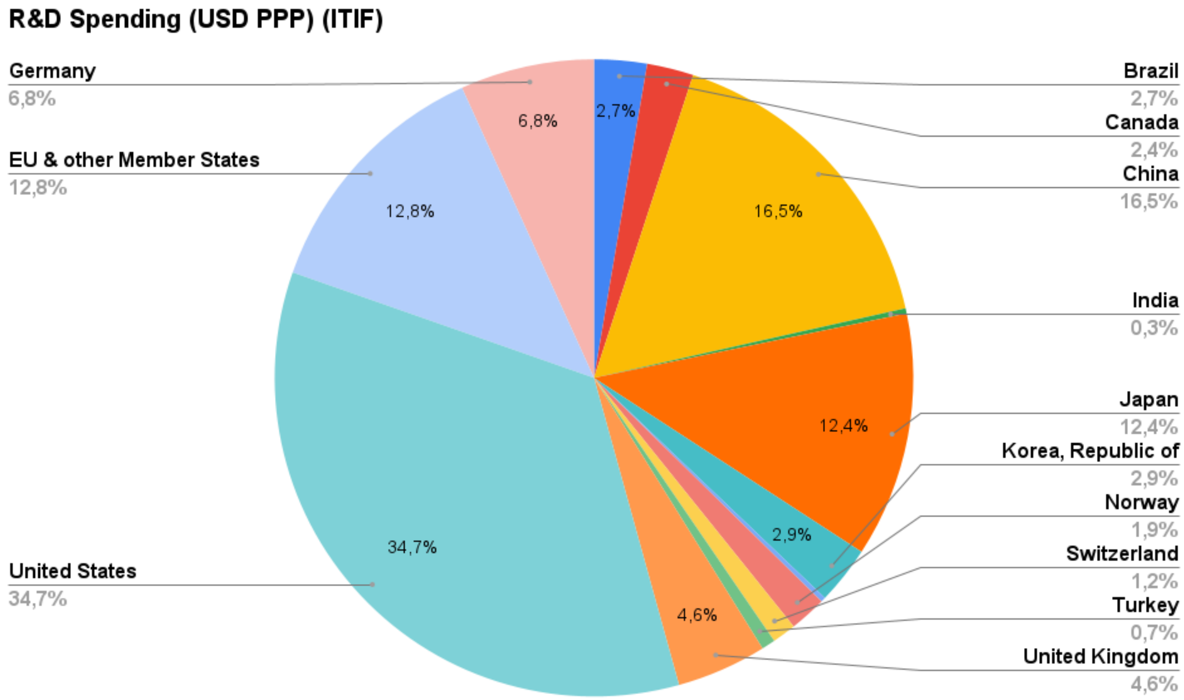


Figure 7: Clean Energy Research and Development Spending by Country (based on ITIF 2021)

As it turns out, this capacity is likely almost perfectly negatively correlated with future emissions -- because future emissions are concentrated in emerging economies, whereas innovation capacity and willingness to spend on clean energy RD&D (and early deployment policies) are concentrated in industrialized high-income countries:

[10] Indeed, ITIF (2021) considers a much broader set of indicators, yet these are mostly on the GDP/capita level and we have not yet scaled them all to the country level.

Variable	US	EU27	China	Rest of World
Innovation capacity (in %)	35	20	17	28
21st century Emissions (in %)	12	7	25	56
Innovation / Emissions Ratio	2.9	2.9	0.7	0.5

Table 3: Innovation capacity and future emissions in comparison

While one should not interpret these numbers as precise -- other indicators of innovation capacity would reach slightly different results, there is no adjustment for the importance of innovation priorities of jurisdictions and there are many limitations to future emissions estimates discussed above -- the basic result holds and is, we believe, critical for how to evaluate climate philanthropy in OECD economies, namely by its how it affects the response of those countries to be most productive in driving down global decarbonization trajectories.

Consider, for example, conservatively[11] that the total value of better innovation were an avoided extra 100 GT CO_2e of emissions, roughly two years of current global emissions. If the ability to affect these 100 GT of additional emissions savings were proportional to innovation capacity, this would give EU-27 innovation policy leverage over 20 GT of emissions, about 5 years of total EU-27 emissions on current trajectory.

Indeed, **these numbers likely strongly underestimate the leverage that countries have on global emissions through innovation** -- because *no* country is anywhere close

[11] See "Adequacy of existing solutions and the need for innovation" for a justification of why this is conservative.

to spending significant amounts of GDP on energy innovation. For example, **Americans famously spend more on potato chips than on energy innovation[12], vividly illustrating that a doubling or tripling of energy innovation capacity would easily be within the capabilities of any industrialized economy** (i.e. this measure is very far from a measure of total capability) whereas a doubling or tripling the speed of domestic decarbonization would be significantly more challenging.

Temporality and type of emissions streams

A common objection to the focus on 21st century emissions rather than, say, a focus on emissions reduction potentials in the next ten years is that emissions further out are harder to affect.[13]

We are not sure this is true.

If we look at the major sources both of emissions as well as avoided emissions they are often related to relatively small decisions -- in terms of resources utilized -- made many decades ago, such as investments into renewables in Europe and California in the 1990s and early 2000s, the discontinuation of the Integral Fast Reactor program in the US in the 1990s, etc. Indeed, the roots of cheap renewables go back to the 1970s and the birth of the modern environmental movement, a 40-year lag. Similarly, much of what is now discussed as “advanced nuclear” goes back to experimental designs from the 1960s-1980s.

At the same time, because the emissions intensity of most economic activity is related to the lifetime of physical assets, such as cars, coal plants, and gas boilers, it is usually much easier to shape emissions trajectories over the medium rather than the immediate term. Indeed, with infrastructure and long-lived capital assets emissions are often shaped for

[12] This quote is from 2010, but the situation has not improved much since in terms of purchasing power parity, see ITIF (2021).
[13] Another one would be to give special attention to near-term emissions for reasons of urgency, however as we discuss under "Patience" this has little justification in climate science.

many decades (which is why avoiding carbon lock-in, discussed below, is one of our primary theories of change). For example, the layout of cities has been found to strongly affect energy consumption (Seto et al. 2016) and much grid infrastructure in OECD countries is more than half a century old.

Both of these considerations push us to not hugely discount emissions later this century in terms of their affectability, though we are planning to come to more considered and quantified views in future research.

A related issue where we seek to achieve more precision through future research is the issue of emissions streams -- whether they are related to existing infrastructure ("committed" emissions), related to infrastructure currently planned or under consideration ("considered" emissions) or to "expectable" emissions, emissions one should expect based on demographic and economic trends, but that are not related to existing or planned infrastructure.

Given that it is considerably less plausible that new capital-intensive infrastructure is prematurely retired (e.g. shutting down new coal plants in China) than that new infrastructure is built more cleanly (e.g. building solar + gas in Sub-Saharan Africa instead of coal), this plausibly affects prioritization putting somewhat more attention to smaller but faster growing and more affectable emitters.

The distribution of funding from the perspective of future affectable emissions

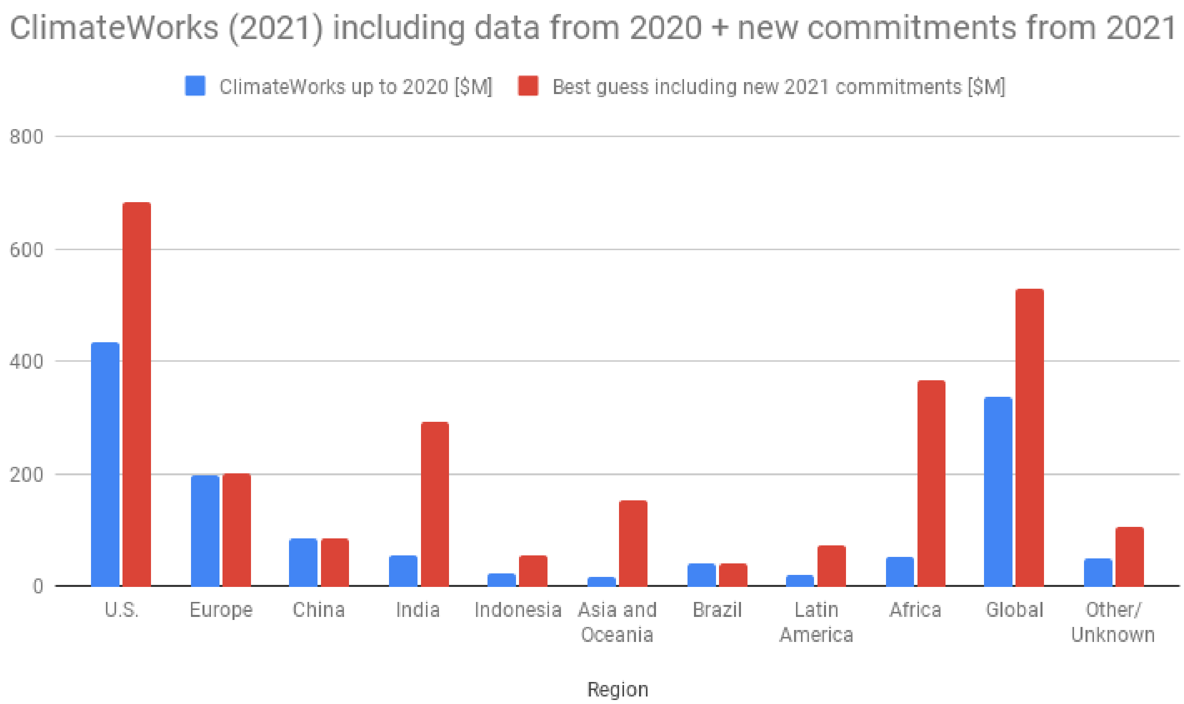


Figure 2: The distribution of climate philanthropy based on ClimateWorks (2021) until 2020 and including our own estimates of major new commitments, including from COP26.

The prior analysis focused on future emissions shares and ability to affect emissions indirectly via innovation capacity is helpful, but not definitive with regards to what an optimal geographic allocation of climate philanthropy would be.

Statements about optimal allocation and, resultantly, misallocation from the perspective of minimizing climate damage would require far more information and assumptions, such as about the relative importance of technological innovation vis-a-vis investment and policy trajectories in jurisdictions, to name a few. We see this as a research frontier, but rather than here making definitive statements about optimal allocation requiring much more detail (such as, "How much climate philanthropy should be targeted at Europe rather than US?"), **we here focus on top-level observations that seem sufficiently obvious and robust from the data we do have.**

We believe there are **four such observations** we can draw from the data:

- **(1) Climate philanthropy is changing very fast:** Recent commitments from this year are changing the picture significantly, given the magnitude of new engagements (Bezos Earth Fund) and significant increases and new programs (such as Global Energy Alliance for People and Planet) data as new as one year old can be quite outdated. For example, by our best guess, the share of climate philanthropy targeted at the US and EU reduced from 66% to 45% just in the last year.

- **(2) US and EU receive allocation far beyond their future emission shares:** Given low future emissions shares but high capacity to affect global emissions via innovation and other indirect effects, this philanthropy should primarily be judged by how it is used to optimize this jurisdictions' responses for global decarbonization given that not only philanthropic but also societal climate spending more widely is much larger in those regions. **It generally does not make sense to evaluate climate philanthropy targeted at those regions by their domestic short-term effects**, given that we know that (a) indirect effects are often much larger and (b) domestic short-term effects are not necessarily indicative of global long-term effects at all (i.e. domestic short-term effects are not a reliable proxy for global long-term impact).

- **(3) The Global Energy Alliance for People and Planet improves the regional balance of funding:** While we are very uncertain about the precise distribution of funding it is clear that this shifts the funding more direction into emerging economies, a welcome improvement from a prior funding allocation that was even more heavily focused on US and EU.

- **(4) The most significant changes outside the US come from just two sectors:** Renewables (part of clean electricity) and forests & other natural climate solutions -- thus, this chart hides a critical fact that despite climate philanthropy increasingly expanding to where future emissions are concentrated, there are still severely neglected spaces when considering sectors and region x sector interactions, which we will do next.

The sectoral distribution of emissions and climate philanthropy

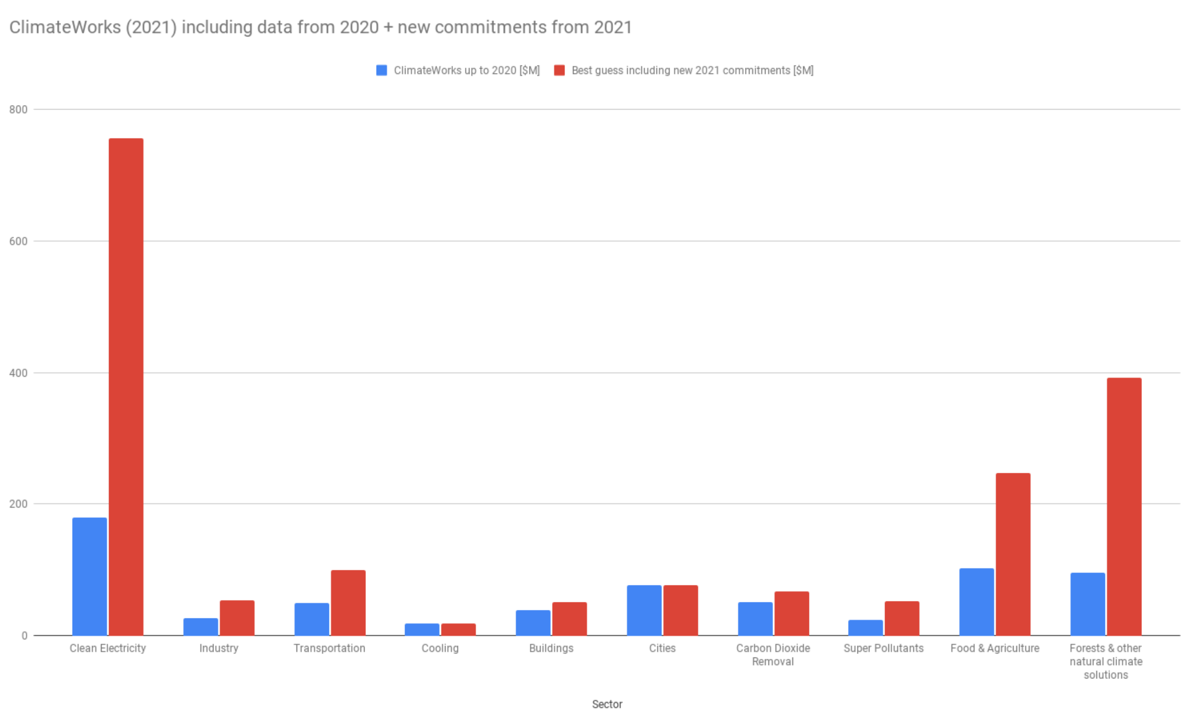


Figure 8: The sectoral distribution of climate philanthropy based on ClimateWorks (2021) until 2020 and including our own estimates of major new commitments, including from COP26.

Again, we first examine emissions sources first to contextualize the distribution of climate philanthropy.

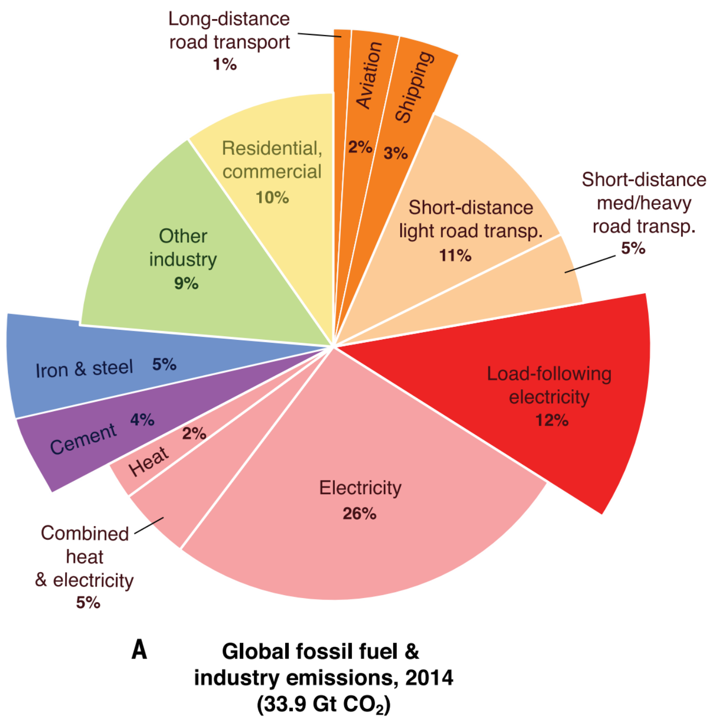


Figure 9: Sectoral distribution of emissions in fossil fuels and industry, highlighting hard-to-decarbonize sectors (from Davis et al. 2018).

While there are many ways to classify emissions sources into different sectors, one we have found particular helpful is the **identification of hard-to-decarbonize sectors, those sectors that seem currently furthest from technologically and economically feasible decarbonization solutions** (as with anything in climate, there is controversy how hard it is to decarbonize those sectors, but it is generally accepted that these provide the hardest challenge).

These are, in particular, sectors that are difficult to electrify due to energy density requirements (long-distance heavy duty transport, aviation, and shipping), because of industrial heat and process emissions (iron & steel and cement), as well as the challenge of load-following, dispatchable zero-carbon electricity. Outside the scope of this pie chart focused on the energy and industrial system, we would add carbon removal and agricultural emissions to the list.

Many of those sectors are also sectors where the emissions intensity is related to long-lived capital-intensive assets (iron & steel, cement, load-following electricity), i.e. prime sectors of carbon lock-in risk.

The fact that those sectors are harder-to-decarbonize means that, on default trajectory, we expect less progress in those sectors than in the overall economy, consequently their share in overall emissions rising.

For example, light-duty transport is on a trajectory towards electrification over the next 10-15 years, likely reaching cost parity in the next five years, meaning that emissions from this sector will, at least relatively, decline, whereas no such trend is locked in yet for aviation.

For impact-oriented philanthropists this also means that the additionality of effort in hard-to-decarbonize sectors is likely higher, as they are not yet trending clean and trajectory changes have not yet been reached.

With this background in mind, here is what we see as the major patterns in the sectoral distribution, again focusing on those findings that seem robust:

- **(1) Philanthropic funding heavily skews towards clean electricity rather than other sectors, in particular it continues to pay little attention to hard-to-decarbonize sectors**. While there is some uptick in philanthropic funding for those sectors such as industry[14] and heavy-duty transport through Bezos Earth Fund commitments, these are dwarfed by other increases. Insofar as philanthropy sets the agenda for civil society and policy, this is worrisome as it continues the overemphasis on relatively mature and popular technologies rather than focusing on those parts of the economy that are not yet trending clean which need more attention the most.

- **(2) While there is a lot of philanthropic funding for clean electricity, this is almost entirely focused on renewables**, with minimal contributions for other clean electricity sources such as nuclear power. While the ClimateWorks numbers do not disaggregate between different clean electricity sources, it is clear that most of this is focused on renewables and the additions from the Bezos Earth Fund and Global Energy Alliance for People and the Planet are exclusively focused on renewables.[15]

[14] Our best guess including new 2021 commitments shows that 2x more money is going into industry compared to 2020 (making it around 2 percent of total philanthropic climate spending).
[15] Making the conservative assumption that 25% of clean electricity spending from the annualized climate philanthropy data up to 2020 was for nuclear power (while this number is not known, the description of this category in the report heavily focuses on renewables and we know from many other sources that pro-nuclear funding is low), the share of nuclear power including new major commitments in 2021 (with zero spending for nuclear power) reduces to around 6%..

- **(3) Topics that have been at the forefront of public attention in 2020 and 2021 or that are generally popular profit disproportionately from the funding surge** , this is true for **forestry and other natural climate solutions** that have received a large share of existing commitments from the Bezos Earth Fund[16] and it is **true for groups focused on building attention for climate action and/or for increasing priorly quite neglected environmental justice** which have been another major focus of initial Earth Fund grants. It is also **increasingly true for work on super pollutants such as methane,** given recent surging policy attention to the issue.

- **(4) There is somewhat more attention to innovation and innovation advocacy than before**, with some of the aforementioned grants particularly focused on innovation in hard-to-decarbonize sectors and commitments from Bezos Earth Fund to Breakthrough Energy and Breakthrough Energy Action. We take this into account in our grantmaking, assuming that innovation advocacy in the US is *less* neglected.

Likely directional biases of foundation data rather than total climate philanthropy

As discussed above, the foundation data analyzed here has, historically, only reflected between 20-40% of total climate philanthropy (with large uncertainty about individual philanthropy leading to the fairly uncertain estimate). While it remains to be seen how the shares will develop with the strong uptick from foundation philanthropy it is important to understand that this data is quite partial while data on individual philanthropy is not available at the same level of disaggregation.

[16] Around 15% from our best guess for annualized spending.

While we have not yet formalized this in adjusted estimates, here are the directional changes we expect if full climate philanthropy data were available:

- **(1) Stronger geographical focus on US and Europe:** We expect the true share of climate philanthropy focused on the US and, to a lesser extent, Europe, to be higher than the foundation numbers suggest given this is where most donors are and that less strategic donors are likely to prioritize local giving more.

- **(2) Stronger focus on "public engagement":** Given the national name recognition of large Big Green groups as well as large social movement organizations such as the Sunrise Movement, we expect the true share of US-focused public engagement-focused climate philanthropy to be higher.

- **(3) Stronger focus on "forests & natural climate solutions":** We expect the true share of philanthropy targeted at forestry and other natural climate solutions to be higher given their extreme popularity.

Integration and intermediate conclusions

Finally, it is useful to examine region x sector combinations of climate philanthropy given that most philanthropic opportunities are focused on particular jurisdictions.

We visualize climate philanthropy by sector x region combination based on ClimateWorks (2021) data and our own additions based on 2021 commitments below, once including all combinations (including "levers", Figure 10) and once focused on geographically identifiable regions and economic sectors, Figure 11):

Figure 10: Climate philanthropy by region and sectors based on ClimateWorks (2021) and our best guess of new commitments

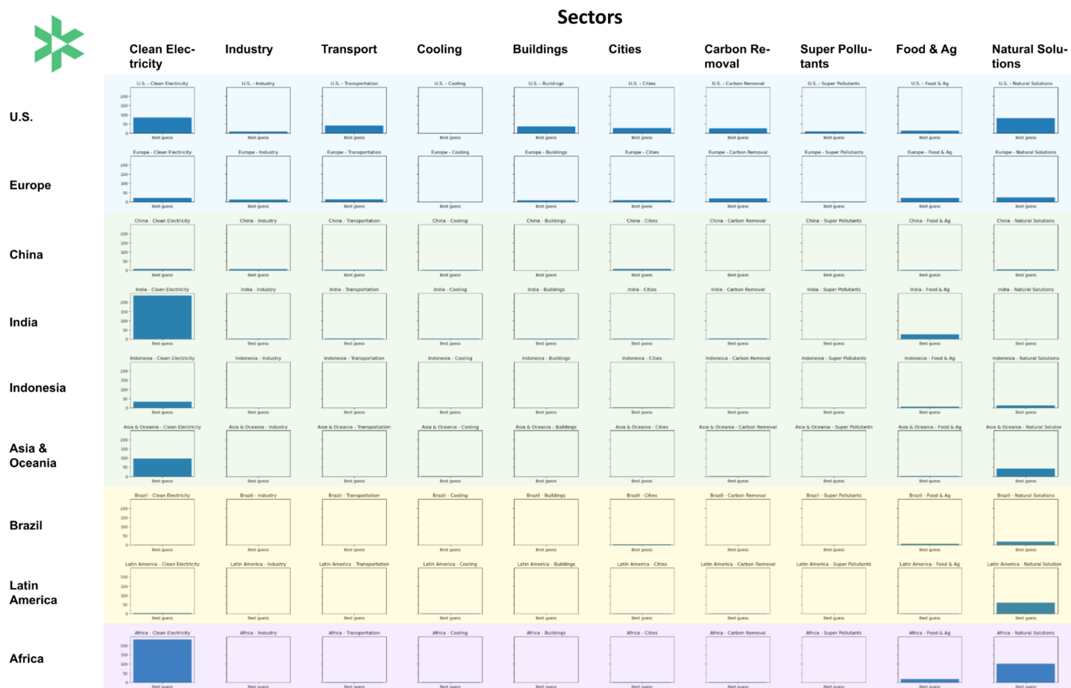


Figure 11: Climate philanthropy by region and sectors based on ClimateWorks (2021) and our best guess of new commitments (reduced to sectors and identifiable geographies)

Beyond the patterns already identified in the regional and sectoral analyses, t**here is one combinatorial pattern that is worth highlighting -- namely that climate philanthropy outside the EU and US is almost entirely focused on two sectors, clean electricity (overwhelmingly renewables) and forests and other natural climate solutions.**

While these are important sectors and an optimal allocation would not necessarily be an equal distribution across all region x sector combinatorials, it appears very likely that this is primarily a perceptual bias, not anything close to an ideal allocation to reduce climate damage.

3. Climate damage is increasing non-linearly

The third crucial fact is that **climate change gets increasingly unmanageable as it gets more extreme.** While a world of 1.5 degrees would not be *that* different from today, a world of 3 degrees of warming would pose significantly more challenges, with risks being a lot more pronounced still at 6 degrees:

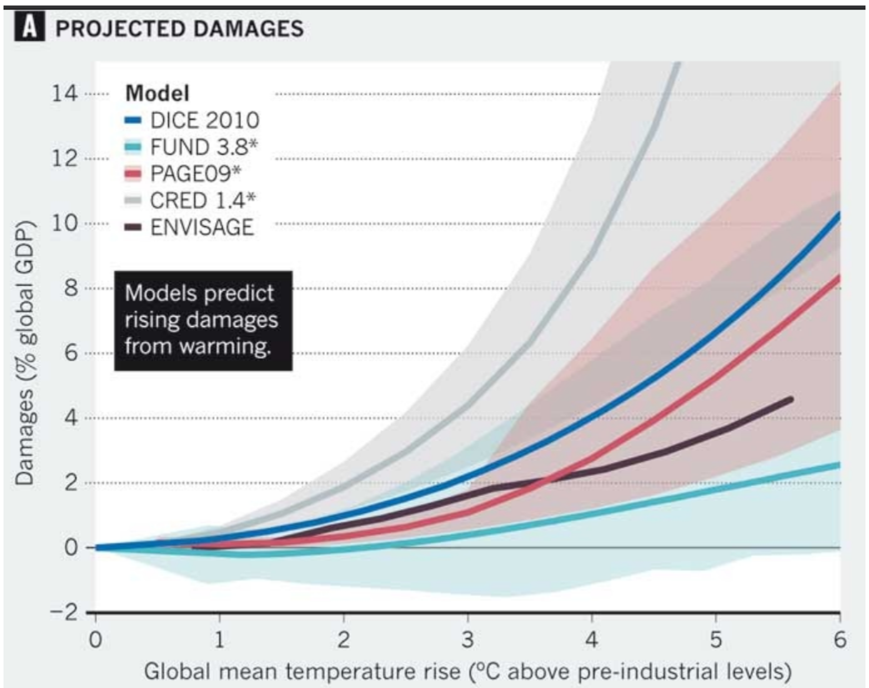


Figure 3: Some estimates on the non-linearity of climate damage (taken from Revesz et al. 2014)

While the displayed models have been heavily criticized for likely underestimating climate damage (and we agree), what matters here is **not the *level* of climate damage, but the *shape* of it.** On the median model of this set (PAGE09), climate damage increases about 5-fold from 1.5 to 3 degrees and about 8-fold from 3 to 6 degrees. Importantly, this basic pattern of increasing damage holds across all of those models and is also consistent with many other approaches to estimate climate damage, it's a robust stylized fact.

This leads to what might be a surprising conclusion -- **the goal of high-impact climate philanthropy is *not* to maximize emissions reductions but to minimize climate damage.**

While this sounds technical, it has profound practical implications for high-impact climate prioritization: if we can, it is *much* more important to shift from 5 to 4.5 degrees if we are in a 5-degree scenario than it is to shift from 3 to 2.5 degrees in a 2-degree world.

Because we know that certain combinations of events are unlikely, for example ending up in a 5 degree world *and* current mainstream approaches succeeding, **this non-linearity also informs our actions in the face of uncertainty** -- putting more resources into solutions that could be vital if current mainstream solutions fail.

In particular, we know a fair deal about what is likely true about worlds where we are strongly miss current climate policy goals, i.e. experiencing warming of 4 degrees[17] or more:

1. **Mainstream climate diplomacy has likely failed:** It's very unlikely that we are on a 4C-degree trajectory if the architecture of the Paris Climate Agreement is still intact. Even in the current situation, where national commitments do not match the global goal of the Paris Agreement (2C degree of warming, ideally 1.5C degrees), a 4C-degree trajectory is quite unlikely. Rather, on current trajectories we should expect around 3C degrees as the most likely outcome (see here, here, and here). So, it is likely that in the high risk worlds, cooperation around climate has broken down to a degree far more severe than what we currently imagine as "*business as usual*".

[17] Adapted from here, which focuses on a 6 degree case we now think is quite unlikely, but the point already holds for scenarios above 3 degrees which are quite unlikely if the goals of the Paris Agreement are met.

2. **Mainstream technological solutions have likely failed:** In a world where wind and solar continue to exceed expectations *and* the direct and indirect (vs. zero-carbon fuels, such as hydrogen) electrification of other sectors of the economy is easy, it is very difficult to end up on a 6C degree trajectory. This means that most of the risk lies in worlds where these solutions have -- relatively speaking -- failed compared to the most optimistic expectations. Maybe the build-out of large-scale transmission infrastructure does not become politically feasible in enough jurisdictions, maybe the growth of renewables plateaus, or maybe the production of green hydrogen just doesn't get cheap or doesn't scale. In any case, we will not be in a world where everything that looks promising and feasible right now will have exceeded expectations.

3. **There's probably a lot of energy-intensive growth in emerging economies:** In energy scenario models that "succeed" at producing very high levels of warming (in particular, the "Shared Socioeconomic Pathways" scenarios 3 and 5), one typical feature is that there has been a lot of growth, particularly in emerging economies. This makes sense: because on typical trajectories emissions in OECD countries are already trending downwards as economic and emissions growth are decoupling, most climate risk comes from emerging economies becoming rich the same way OECD countries did -- burning lots of fossil fuels in energy-intensive industry and an expanding infrastructure.

4. **There might have been a growth explosion:** One plausible way to get to lots of warming is an explosion of economic growth not accompanied by a corresponding investment into decarbonization. One can, for example, think about AI-fueled growth explosions where -- for some reason, such as an arms race or another reason for breakdown of climate concern -- little attention is paid to making this growth low-carbon.

Implications for high-impact climate philanthropy

The combination of these three crucial facts -- (1) strongly increasing attention and funding for climate, (2) an uneven allocation of this attention and funding leaving some approaches, technologies and geographies severely and predictably neglected and (3) the increasing damage the more we fail -- **fundamentally shapes what we believe to be the "plausibility space" for high-impact climate philanthropy.**

The combination of facts)(1) and (2), for example, makes it extremely unlikely that funding a popular and well-resourced strategy, such as forestry and other natural climate solutions or supporting climate movements with national name recognition is anywhere close to the most effective thing one can fund at the margin.

This does not make it *impossible* that the best funding margin is with an organization doing very popular work in the same way that it is not impossible that a cat shelter in a wealthy neighborhood provides the best opportunity to help animals with additional money, but it strongly ups the burden of proof required for such claims.

That also does not mean those strategies are not vital, but rather that -- compared to their potential -- there are vastly more underfunded spaces.

This argument becomes even more pronounced through what we know about the risk and cost of failing on climate action (fact (3), non-linearity of climate damage). Because we know that certain combinations of events are unlikely, for example ending up in a 5 degree world *and* current mainstream approaches succeeding, this non-linearity also informs our actions in the face of uncertainty -- putting more resources into solutions that could be vital if current mainstream solutions fail. Given all the attention mainstream approaches get, it is hard to imagine that marginal philanthropy, additional attention beyond existing attention, can make a large difference to the success probability of those approaches.

For example, in a situation where climate philanthropy improves a lot by putting 500m/year into renewables in developing countries (the recent pledge by Rockefeller, IKEA Foundation and Bezos Earth Fund), it is still possible that allocating 10m more to a similar intervention is the best use of additional climate money (indeed, we are evaluating one organization in this space). However, on the face of it, this seems very unlikely, whereas it appears a lot more plausible than investing 10m into philanthropy targeted at, say, increasing the prospects for advanced nuclear adoption in emerging economies, an area receiving close to zero philanthropic funding, does much more in de-risking the future, as it provides an intervention that will be helpful if renewables, relatively speaking, fail.

This means that we strongly believe that **the plausibility space for high-impact climate philanthropy primarily contains solutions and approaches that are considered controversial, speculative, or remote by some.**

In the late 1990s, this was maybe advocacy for ambitious renewable policies, as renewables were considered by many to be inherently expensive and unable to contribute meaningfully to decarbonization (they were wrong). In the early 2000s, this might have been advocacy for methane regulation which was outside the public view then. In the late 2010s it was possibly carbon dioxide removal, a crucial path of any decarbonization scenario by then, but not reflected in public attention at all (luckily, this is improving now, thanks to, i.a. Carbon180).

Thus, in a situation where *current* mainstream approaches profit from a strong surge of attention and funding, we should continue to look out for those opportunities that are, as of now, overlooked or considered weird.

Key uncertainties

Every strategy has significant uncertainties

Every cost-effective strategy to spend money on reducing the climate problem will carry some uncertainty -- the desired outcome of a particular emissions reduction cannot be guaranteed.[18]

Even something as seemingly certain as funding the planting of a tree is usually uncertain in its emissions outcomes, as it is not guaranteed that the tree will persist on climate-relevant timelines and, more subtly, it is usually questionable whether the planting of that tree is truly additional given existing policies and regulations.

This is not about singling out tree planting offsets, the same is true for other offsets as well.

It is even true for *seemingly* clearly additional actions such as using efficient light bulbs, because -- if you live in a high-income country -- it is possible that overall emissions from electricity are capped making individual actions less than 100% additional.[19]

Of course, there is also uncertainty in philanthropy, but -- crucially, as explained above -- this is *not fundamentally*[20] *different* from other ways of taking action.

This uncertainty often makes donors uneasy, but it is **important to recognize that *certainly having little impact* is *much* worse than probably having *much* larger**

[18] This would only be false if there was nothing uncertain you could do, leveraging advocacy, technological change or other ways to increase impact, that would be more cost-effective than buying direct emissions reductions, as we will explain in more detail later (in the Strategy section), this is extremely implausible.
[19] The argument is not that they have zero effect, most carbon markets are now avoiding the full "waterbed effect" that plagued early carbon markets, but rather that even something as direct and physical as installing better light bulbs is uncertain in its emissions consequences and, consequently, its cost-effectiveness.
[20] Maybe one could say "but the uncertainty is much larger in philanthropy". While this might be true, there isn't a clear reason to care about the degree of uncertainty beyond non-linear damage (explained below)

impact in expectation, as long as your expectations are unbiased.[21] Indeed, as long as we collectively make enough unbiased bets to have a positive impact on the climate, through philanthropy and otherwise, it doesn't matter whether the ones we made as individuals succeeded.

So, to be high-impact climate philanthropists we think **it is critical to be comfortable with uncertainty**, obviously trying to reduce it and updating when the reduction of uncertainty leads us to new conclusions, **but not avoiding uncertainty at the cost of giving up on the chance of high impact.**

With that said, let's explore a bit what uncertainty does and doesn't imply and how the presence of large uncertainty should affect our thinking about high-impact strategy.

Uncertainty does not imply ignorance

Uncertainty does not imply ignorance.

Nowhere is this more obvious than in climate where we *know* that climate change is occurring even though we are *very uncertain* about the precise level of warming given a level of emissions as well as the associated damages of warming.

Even when we deal with large uncertainties, this does not mean we cannot make statements about the relative promise of different strategies and the likelihood that they will be highly impactful.

Consider the following example from everyday life: we know that riding a motorcycle is more dangerous to us than driving a car. In both cases, the risk of a fatal accident is very

[21] Our preferred method to hedge against the risk of biased expectations is to make extremely conservative assumptions which we know to be too pessimistic. We only describe something as extremely cost-effective when it looks extremely cost-effective even on very conservative assumptions.

low and the precise risk is unknown to us because it doesn't only depend on population averages (which we can look up), but also our driving style, local conditions, etc. Yet, despite this large uncertainty *and* the low probability of the event (here: a fatal car accident), we can robustly say that -- if our goal is to minimize loss of our own life -- we should prefer driving a car over a motorcycle.

Just like in this example, where probabilities are low and uncertainties large, we can make relative statements about safety (and do so all the time in all walks of life), we can also make statements about the relative promise of different approaches we can fund in the climate space.

From uncertainty about precise cost-effectiveness to confidence in impact differentials

A typical objection to pursuing highly uncertain strategies, such as funding advocates for better energy innovation policy, is that it is very uncertain how cost effective those interventions are.

This is true.

For any given project we fund in this space we aren't certain whether it will save a ton of CO2 for, say, 0.001 USD to or 10 USD/tCO2e, implying -- in this case -- a uncertainty of a factor of 10,000.

But it doesn't *really* matter.

What matters for action is not precision about *absolute* cost-effectiveness, but a robust understanding of *relative* cost-effectiveness.

Rather than asking "*Does it cost 0.001 USD or 10 USD to reduce a ton of carbon dioxide via funding advocacy?*" we should ask *"How confident are we that this is much more cost-effective than another intervention, say, directly planting trees?"*

This is why in the remainder of this guide we will talk about " *impact differentials*" and "*impact multipliers*", systematic features of the world or strategies that should let us expect that some actions and strategies are much more cost-effective than others.

Uncorrelated uncertainty: Multiply

If uncertainty is uncorrelated, if knowing how one uncertainty resolves does not tell us anything about another uncertainty, then we can simply multiply uncertain ranges with each other. In this case, a tool like Guesstimate -- allowing us to multiply distributions rather than simple point estimates -- gives reliable results.

Correlated uncertainty: Consider jointly

Sometimes, important action-relevant uncertainties are deeply intertwined, not independent from each other. In this case, multiplying them would be completely misleading.

Consider the following toy-example which will re-occur throughout (note that probabilities here are chosen for ease of explanation, these are not our real estimates for those variables):

For both events, (a) mainstream technologies failing, and (b) being on a 6 degree trajectory, the overall probability is a fair coin toss (50/50). Yet, of course they are not independent from each other[22] because it is, luckily, really hard to end up in a 6 degree world when mainstream low-carbon technologies succeed.

Conversely, we can reason backwards -- when we know that we are in a 6C world, then we also can be quite confident that mainstream low-carbon technologies have, relatively speaking, failed.

<table>
<tr><th colspan="2" rowspan="2">Two correlated uncertainties</th><th colspan="2">Are we on a 3 degree or 6 degree trajectory?</th><th rowspan="2">Total probability</th></tr>
<tr><th>3 degrees</th><th>6 degrees</th></tr>
<tr><td rowspan="2">Have mainstream low-carbon technologies succeeded or failed?</td><td>Succeeded</td><td>0.4</td><td>0.1</td><td>0.5</td></tr>
<tr><td>Failed</td><td>0.1</td><td>0.4</td><td>0.5</td></tr>
<tr><td colspan="2">Total probability</td><td>0.5</td><td>0.5</td><td>1</td></tr>
</table>

Table 4: Correlated uncertainties

As we will see throughout, **this is not a technical issue at all**, but **has rather fundamental practical implications** for high-impact climate prioritization.

[22] In which case each of the cases below had a 25% chance.

Ability to further accelerate decarbonization

We actually do know fairly little about how much of climate change is *currently* solved if we think about it from the perspective of making an additional impact.

Given current trends of uptick in policy ambition, venture capital investments, and broader societal attention, it is entirely conceivable that the virtuous cycle of cost reductions, increased policy ambition and related further investments and cost reductions will drive us towards net-zero emissions faster than mainstream models anticipate. Indeed, energy models have -- so far -- had a structural bias to underestimate cost reductions and overestimate emissions.

It is thus *possible* that we are already on a trajectory where the success story in parts of the power sector (intermittent renewables) and transport (electrified light-duty transport) **will be repeated across a wider range of technologies allowing the decarbonization of the entire energy economy and carbon removal at scale.**

We are currently quite unsure what probability to put on this outcome, but think anything between 10-50% seems broadly defensible. Conversely, we think there is a 50-90% chance that we are not currently on a trajectory where we quickly decarbonize the entire world economy.

Of course, just because there is a probability that we are already on a relatively benign trajectory this should not be an argument for complacency as long as we cannot rule out much worse outcomes. This is analogous to preparing against existential catastrophe from artificial general intelligence (AGI) despite a probability that such AGI will be inherently safe,

Adequacy of existing solutions and the need for innovation

A related uncertainty to whether or not we are already on a swift decarbonization trajectory is the question to which degree existing technologies are sufficient (and what is lacking, if anything, is policy or other societal changes) or, conversely, the degree to which technological innovation would unlock additional emissions reductions.

This is of course a very complicated question and, unfortunately, it has also been caught up in the "culture wars" of climate change, with vocal supporters of innovation and voices focused on deploying existing preferred solutions often juxtaposed.

Crucially, while it appears like a question merely about technology, it is not -- the answer is deeply intertwined societal and political factors, such as the willingness to pay carbon taxes, to live near large-scale transmission or electricity generation infrastructure, etc. For example, if a global uniform carbon tax starting at USD 100/tCO2e and escalating over time were politically feasible, we would probably not need a lot of additional innovation and -- in addition -- this kind of credible and high price signal would likely induce the innovation still required.

So we think the most action-relevant way to pose this question and related uncertainty is: "Given political, economic and technological constraints *and* our ability to change them, would additional innovation deliver significant decarbonization and climate risk mitigation benefits?"

While there clearly is significant uncertainty on the precise need for innovation -- there are many ways to get to net-zero -- phrasing it in terms of usefulness at the margin strongly reduces the uncertainty, while also being more action-relevant.

Put this way, we put about an 80% probability that additional innovation could deliver *at least* a 100 GT of additional avoided (or removed) emissions, which would equate to about two years of current global emissions, or about 25% of the difference the IEA believes technologies currently in prototype or demonstration stage would make between now and 2070 when moving from a"stated policy" to a "sustainable development" scenario.[23] We think this is fairly conservative and quite different from a maximal estimate on the benefits of innovation, while it appears justified as a lower bar based on the evidence on induced innovation, as well as expert assessments on additional innovation needs and potentials (such as Energizing America or the IEA), both of which we somewhat discount in this conservative estimate.

There is one more important aspect to this uncertainty which is about the cost of being wrong: being too pessimistic leads to a possible overspend on innovation for resources that could have been better spent on other efforts. Being too optimistic, however, appears like a far more severe risk, given that a lack of sufficiently cheap decarbonization technologies across all sectors is a primary way to end up in a high-emissions high climate risk future (also see below).

There is one **important caveat**, namely that **simplistic versions of innovation arguments can and sometimes are weaponized against action in the present**, with lukewarm commitments to innovation being used to not act boldly in the present. Obviously **this is nothing we support** and our grantmaking only supports organizations that focus on a bolder innovation effort *alongside* ambitious emissions-mitigation policies in the present.

Severity of carbon lock-in

Carbon lock-in describes a set of phenomena that make current decisions stick for decades, via long-lived carbon and capital intensive assets (e.g. new coal and steel plants), infrastructure choices, and other forms of lock-in (e.g. regulation, special interest

[23] I.e. we assume that about 75% of that innovation will be delivered without additional effort.

coalitions) that limit the speed of (future) low-carbon transitions (see [Seto et al. 2016] for an academic review). For example, while renewables have become much cheaper recently affecting the new-build of coal, the world has a lot of very new coal assets that are, in the best case, challenging to prematurely retire (and in the worst case won't be retired and commit the world to a > 1.5C warming trajectory).

Consider this graph from [Tong et al 2019] which suggests that we have already committed more than 500 GT of emissions, more than 10 years worth of current global emissions, into the future:

:

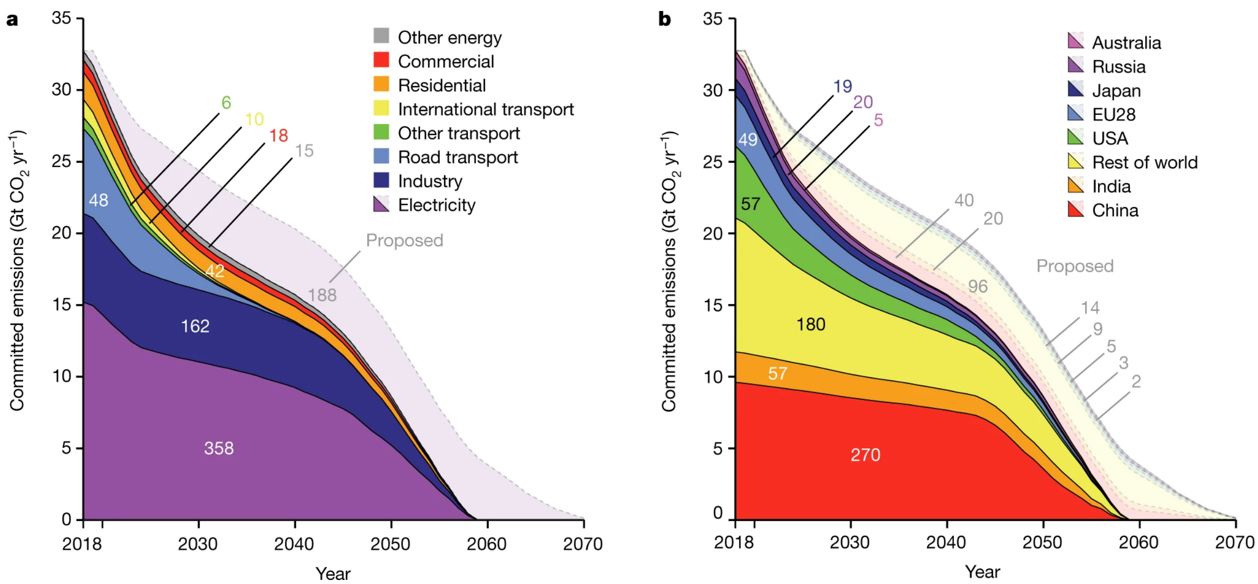


Figure 12: Committed emissions by sector and region (taken from Tong et al. 2019)

If this calculation is roughly accurate, then we have already blown our chance at hitting the 1.5 C temperature limit.

Of course, such a calculation about **committed emissions** -- emissions locked-in from existing infrastructure, a part of carbon lock-in[24] -- always come with lots of assumptions and it could be the case that early retirement, lower utilization or retrofitting of assets are much easier (or: harder) than assumed, for example it could be that advanced nuclear becomes a viable re-powering option for coal plants.

This is the uncertainty we describe here, how severe carbon-lock in is and how much future emissions have already been decided (committed) or are currently being decided (proposed, or considered emissions).

Crucially, as Table 5 illustrates, the uncertainties that constitute our uncertainty about the severity of carbon lock-in are negatively correlated with uncertainties about the potential of accelerating innovation:

Variable	**Context**	**Description**	**Effect of variable increase on importance of avoiding carbon lock-in**	**Effect of variable increase on importance of accelerating innovation**
Lifetime of assets	Long-lived asset lock-in		Increases, as it shapes future more	Decreases, as it makes it harder for innovation to make a difference
Ability to retrofit assets	Long-lived asset lock-in		Decreases, as future becomes more affectable	Increases, as it will make more of a difference
Operational cost	Long-lived asset lock-in		Decreases, as it makes decommissioning	Increases, as it makes it easier for innovation to make a difference

[24] Note that this is only one part of carbon lock-in and there are other aspects such as infrastructure lock-in.

			and reduced utilization rates more feasible	
Extent of infrastructure lock-in	Infrastructure lock-in	How many choices are being made that lock-in infrastructure in the future?	Increases, as it shapes future more	Decreases, as it makes it harder for innovation to make a difference
Severity of infrastructure lock-in	Infrastructure lock-in	How severe those choices are, how much less likely do they make adoption of low-carbon tech in the future?	Increases, as it shapes future more	Decreases, as it makes it harder for innovation to make a difference
Ease of adopting innovation in existing infrastructure	Infrastructure lock-in		Decreases, as future becomes more affectable	Increases, as it will make more of a difference

Table 5: Negatively correlated uncertainties affecting the impact of innovation and carbon lock-in avoidance strategies

Background conditions: Geopolitics and climate attention

When thinking about climate change over the course of this century, it is natural to extrapolate from existing geopolitical conditions and climate attention and this is, indeed, what most forecasting of future emissions assumes, extrapolating based either from existing trends of development and/or existing policy commitments.

But taking into account that the age of global cooperation on environmental agreements is only about 30 years old, with the 1992 Rio Earth Summit establishing the UN Framework Convention on Climate Change (UNFCCC) which underlies global climate cooperation, it is important to be aware that future geopolitical conditions could be quite different and, indeed, less favorable to global climate cooperation.

We are currently, by historical standards, in a situation of very high attention to climate throughout the Western world and even globally (notably: China). Despite all its shortcomings the Paris Agreement has been fairly successful in overcoming the prior stalemate and in focusing the conversation on escalating ambition.

While there are reasons to expect continuity, attention to climate has been strongly increasing and it is difficult to imagine it radically diminishing, there are also reasons to expect attention will decline and conditions will deteriorate.

The one we are worried about the most is intense geopolitical competition between great powers or even a great power war, an event for which we have seen expert forecasts in the 10-30% range for this century (see our upcoming report on Great Power War).

This is our main argument against being relatively optimistic on decarbonization progress, the main way in which we could end up in 3+ degree worlds are those where current momentum whittles down. We incorporate this uncertainty by assigning an extra 20% of probability on those SSP/RCP combinations that are meant to model worlds where climate attention is waning and where geopolitical conditions are relatively conflictual, in particular the higher three RCP scenarios of SSPs 3-5 (also see Background).

Implication: Robust diversification

The goal to (a) maximize avoided climate damage, (b) combined with the non-linearly increasing damage function and (c) the structure of uncertainties suggests a principle we call “robust diversification”, which has two components:

Portfolio diversification with negative correlations

When deeply uncertain about the precise returns of different strategies, we combine strategies where the uncertainties are negatively correlated, so that when one uncertainty is resolved “pessimistically” chances are the other uncertainties are resolved positively.

For example, we believe that strategies to avoid carbon lock-in provide a natural complement to innovation advocacy, as the primary uncertainties of both strategies -- the severity of carbon lock-in and the ability of innovation to upend it -- are negatively correlated (innovation is the least-effective when carbon lock-in is severe, and vice versa).

To a lesser degree, we are also applying this principle by combining advocacy focused on accelerating decarbonization (such as via CATF and TerraPraxis) and advocacy to accelerate carbon removal (Carbon180), as accelerated carbon removal will be particularly valuable if decarbonization is, relatively speaking, hard.

Robustness to the worst worlds

The second component is not related to the structure of uncertainties amongst each other, but rather to the resolution of uncertainties and the shape of climate damage.

Due to the shape of climate damage, much higher damage to be expected with each additional degree of warming, we weigh less likely but worse scenarios much higher than their probability (rather, we weigh them by expected damage).

Table X illustrates this with our best guess for the most optimistic (RCP 1.9) and most pessimistic (RCP 8.5) scenarios considered by the IPCC. While we currently believe that RCP 1.9 is more than 3x more likely than the RCP 8.5 (also see “Background” below), because RCP 8.5 is estimated to be 28x[25] worse (see “Climate damage is non-linear” above), the climate damage from this scenario dominates our expectation by a factor of almost 10.

RCP	**Probability (%)**	**Marginal damage normalized (in % of GDP)**	**Expected damage normalized**
RCP 1.9 (1-1.5C)	9.5	1(0.1)	0.095
RCP 8.5 (5C)	3	28(2.8)	0.84

Table 6: Expected climate damage

While one should not treat such estimates as overly precise, the basic pattern leads us to **prioritize strategies that are effective under pessimistic assumptions**, for example when it turns out that a lot of progress is needed still and international coordination and willingness to pay for climate action are low, given that it is quite plausible that the situation will deteriorate and this is where most climate risk is concentrated (i.e. where additional mitigation is most valuable).

[25] We estimate the slope, i.e. marginal damage, at 1.25C and 5C taking the median model (Page09).

Strategy

When seeking to maximize impact in the climate space, we believe it is fundamental to look for “impact multipliers”, reasons to expect that a given funding allocation will have an above-average impact.

These multipliers exploit systematic features of the climate (funding) space.

We think of high-impact theories of change as combining several impact multipliers, so after explaining what we see as the most important impact multipliers in climate we explain promising theories of changes each leveraging several of those multipliers.

Impact Multipliers

In the current moment, where attention to climate and philanthropic funding are surging, we should expect the average additional dollar going to a well-known organization with national name recognition or to an intervention that is universally popular to have low additional impact.

At the same time, this does not mean the climate space cannot and shouldn’t absorb more philanthropic funding, but rather that -- to maximize impact -- this funding should complement rather than copy existing funding streams and, in a world where the overall challenge is not solved, be used as strategically as possible to reduce climate damage as much as possible.

We now discuss those impact multipliers, reasons to expect above-average impact, in turn.

Three points are worth making before:

- **(1) Not all multipliers are independent:** As will become apparent in the discussion, not all of those multipliers are fully independent, so one should not just multiply them (though for those that are independent, multiplication is the right approach).

- **(2) Not fully quantified here:** While we do have rough quantitative estimates of the importance of different multipliers and mention some of them in the discussion, we do not focus on them here -- to avoid the illusion of false precision. Rather, we encourage thinking of them as robust considerations pushing our expectation of impact upwards.

- **(3) Expect vast impact differentials:** That said, given that we often find that similarly promising approaches are funded at vastly different levels due to reasons that seem primarily rooted in impact-agnostic considerations (such as ideological preferences or public attention) and given that fundamental features of the climate challenge, such as emissions math and non-linear trajectory changes in policy, society, and technology, we should not be surprised to expect vast impact differentials between different options we should fund. This does not mean we have found all multipliers or that we are right about all, but rather that the general picture we seek to draw, that there are vast differences and it matters to get better at prioritization, should not be as implausible as it might seem at first glance.

Ultimately, reasoning about impact multipliers does not replace deeply engaging with different theories of change and funding opportunities. Rather, they serve as an orientation for where to search and as a prior of how surprised we should be to find high-impact opportunities in different parts of the climate funding space.

Lastly, as illustrated by our analysis of funding levels above, we do not assume these multipliers to remain static over time, indeed the dynamically changing climate action and philanthropy landscape necessitates ongoing research and adjustment to find the highest-impact opportunities as the field changes.

Neglectedness

In the above section "Future emissions are not where most of the climate attention is" we analyzed the distribution of philanthropic funding across geographies and sectors.

This section is about why we should care about this and why this kind of analysis is actually tremendously helpful when prioritizing and when forming beliefs about where we should expect additional funding to have large counterfactual impact.

In other words, it is about why we expect a focus on neglected solutions -- solutions that compared to the part of the problem they could solve (here, climate damage avoided) receive few resources -- to provide a strong impact multiplier.

In principle, there are two powerful reasons why we believe this to be true:

- **(1) Higher chance of additionality:** In a field that is growing quickly and that generally receives significant societal attention, such as climate in the current moment, many actions in the most crowded sub-spaces will have low additionality, they will happen / be funded anyway. We believe that focusing on a blindspot rather than a sector receiving lots of attention can easily increase the probability of additionality by a factor of 10 or more.

- **(2) Picking low-hanging fruits:** In most cases, "early" efforts are more impactful, e.g. the first organization advocating for a particular solution will have a higher impact than the second one.

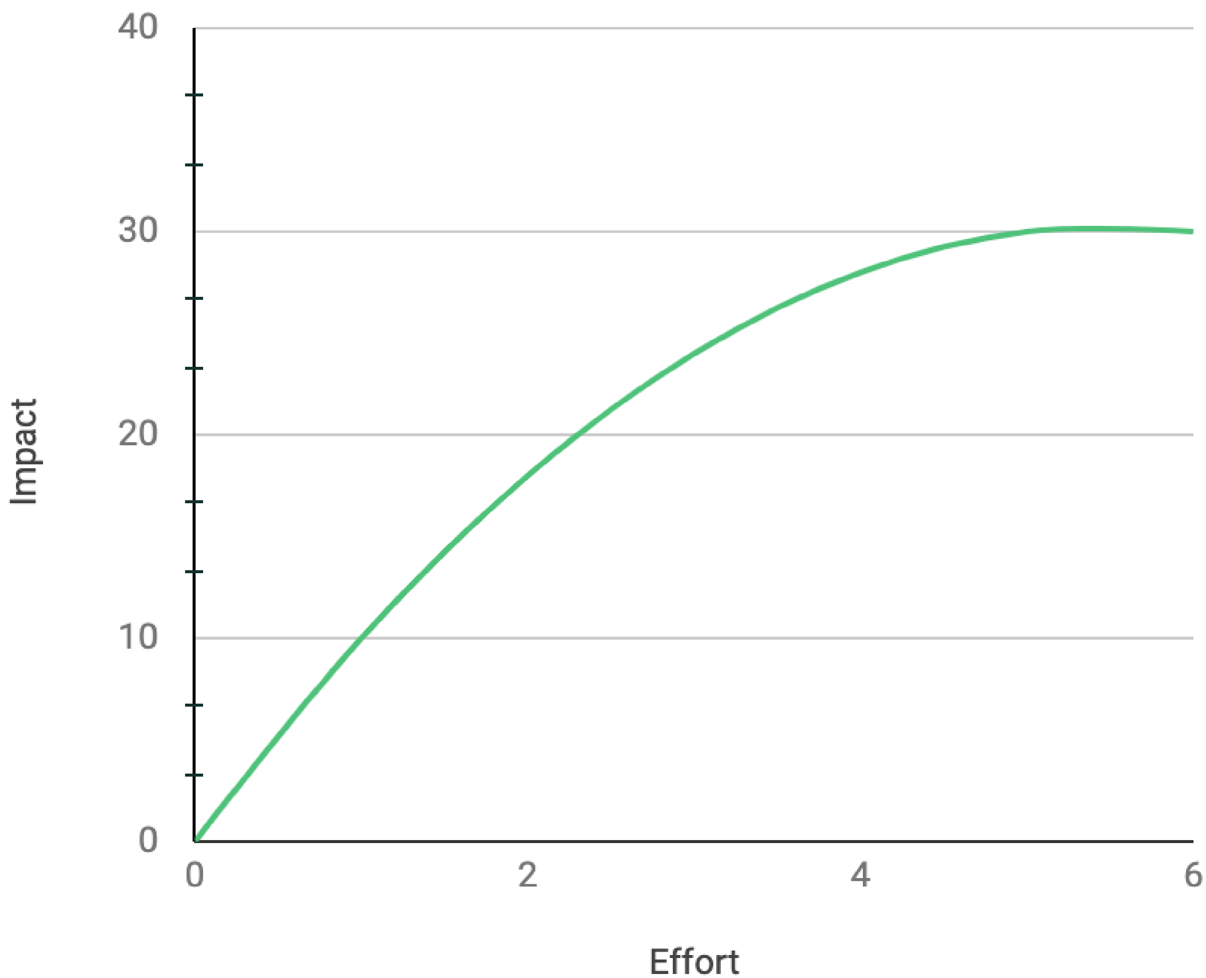


Figure 13: Expectation of impact as a function of effort

While it seems uncontroversial to us that neglectedness is a strong impact multiplier, one particular problem that arises is that "neglectedness" is nothing we can observe in the world and, indeed, should not be conflated with "lack of resourcing".

Obviously, not everything that is not well-resourced is "neglected", as not every solution is promising, making the observation of low funding a mixed signal with regards to whether something is (a) neglected or (b) just rightfully considered not promising.

We choose two strategies to differentiate the two and gain larger confidence in neglectedness:

- **(1) Identification of biases that should let us expect neglectedness:** Arguments for neglectedness are more convincing when there are underlying mechanisms, such as ideological or behavioral biases, that explain lack of resource allocation. For example, when observing a relatively lack of pro-nuclear climate philanthropy, the well-documented anti-nuclear bias of the environmental movement for reasons other than climate impact strongly pushes out towards neglectedness rather than rightfully unfunded. Of course, many other such biases exist across the entire political spectrum, e.g. we have often come across anti-government biases that downplays the vital role of government in innovation and, thereby, underestimates the potential of policy advocacy.

- **(2) Rooting of "neglected compared to importance" in expert assessments:** Wherever possible, we root judgements about relative neglectedness in expert assessments, such as on the importance of different technologies, and of sectoral and regional contributions to emissions (see "Future emissions are not where most of the climate attention is").

Given that there is often a fair amount of variance in assessments, we try to invoke "neglectedness" arguments only for those fields where there is a lot of converging evidence for neglectedness, i.e. funding differentials that seems clearly disproportionate and where there are clear systematic reasons that make us expect these are indeed instances of neglectedness.

Risk neutrality to avoided damage

A typical behavioral bias in climate giving is "risk aversion", preferring a (seemingly) certain emissions reductions -- say, from a carbon offset -- to a much higher expected reduction through a more uncertain means, say, advocacy-focused philanthropy.

While there is some argument for risk aversion due to non-linear climate damage (see below), we believe that the average climate donor is much too risk-averse. This means that, everything else being equal, we should expect opportunities like Option B below to be relatively under-funded compared to their goodness, giving the impact-oriented donor an impact multiplier to exploit.

> **Option A:** Reduce 10 units of CO_2 for certain.
>
> **Option B:** Toss a fair coin and reduce emissions by 30 units if it turns up heads, and by 0 if it turns up tails (expected emissions reduction of 15 units of CO_2).

This argument is exacerbated by the fact that seemingly certain options are usually not certain at all, e.g. almost all carbon offsets struggle with additionality and other integrity concerns undermining the seeming certainty.

Of course, this argument is too simplistic because of non-linear climate damage (see discussion above), tonnes of carbon dioxide causing different amounts of harm in different possible futures.

For example, in the case described by Table 7 we should not be neutral between avoiding 1 tonne of carbon in the 3 and 6 degree world (given the expected damage in the 6 degree world is 3x larger), but rather we should be neutral between avoiding 3 tonnes in the 3 degree world and 1 in the 6 degree world.

	Probability	Damage per marginal ton	Expected avoided damage
3 degrees	0.8	1	0.8
6 degrees	0.2	12	2.4

Table 7: Expected avoided climate damage in different futures

Patience

A lot of climate philanthropy is driven by a desire to quickly reduce emissions, e.g. by marginally accelerating transitions already in progress, such as coal phase-outs in Europe or a quicker adoption of electric vehicles (when this transition is already poised to happen).

While this would be impact-maximizing if there was something special about emissions in the near term, this does not seem to be the case -- if given the choice between reducing 2 units of emissions in 2030 or 1 in 2021, you should choose the former.

This is so because [cumulative emissions are the primary determinant of warming] and because the additional climate damage from a marginal additional ton of CO_2 in the 2020s is likely much lower than that of two additional tons from 2030 onwards.

Advocacy

We think that funding organizations that are trying to influence how societal resources (incl. attention) are allocated provides a strong impact multiplier compared to funding more direct interventions.

There are principally five reasons why we believe this to be true:

- **(1) Leverage:** Societal resources spent on climate are 100-200x compared to philanthropy, e.g. global climate philanthropy is at about USD 10b/year whereas public spending on climate this year will probably around USD 1t[26], i.e. a difference of 100x.

- **(2) Necessity:** Many required changes for global decarbonization require policy change and public investment, such as -- at the very minimum -- large-scale transmission infrastructure and regulatory changes to allow new technologies (e.g. reformed licensing around advanced nuclear). Of course, there is a much larger role for policy possibly through binding climate targets and other policies, though they are not feasible everywhere.

[26] This estimate is based on the Climate Policy Initative's estimate which puts climate spending at > 600 B/year, but excludes many sources of spending and is lagged, i.e. does not include uptick in spending in 2021 which is likely significant.

- **(3) Causal primacy:** While climate-relevant actions will often not be performed by the government, e.g. private companies are playing an important role in the later stages of innovation, both the policy environment (e.g. R&D tax credits) and early public investment will usually be critical (see, e.g. Block and Keller 2011, Mazzucato 2011). This is of course even more true for more inherently regulatory approaches, such as climate targets and other policies.

- **(4) Additionality:** As discussed under "Policy Additionality", in countries with ambitious climate policies, many private actions are not fully additional anymore whereas advocating for policy change, on the other hand, is inherently additional.

- **(5) Intangibility and likely neglect:** The fact that advocacy is an abstract thing to fund, with uncertain outcomes and lack of clear attributability -- unattractive psychological attributes from the perspective of donor psychology -- make it likely it is relatively underfunded in a philanthropic market dominated by individual givers such as climate philanthropy.

We think there are primarily two objections to advocacy-oriented philanthropy:

- **(6) Impact can always be zero:** While this is true, as we discuss under "Risk Neutrality" we think this should not be a reason to not pursue a strategy as long as the strategy looks highly cost-effective under unbiased expectations. In our assessment, funding organizations that focus on high-impact neglected solutions looks extremely cost-effective even under quite pessimistic assumptions about contribution of impact (or, conversely, success probability; also see Background).

- **(7) Pork barrel politics and crowdedness:** When the advocacy space is crowded and policy attention is high, it is quite plausible that some advocacy will have zero impact or that there will even be counter-mobilization dynamics.

We think that (7) is a valid concern and that when evaluating advocacy opportunities it is important to examine the plausibility of outsized impact via (a) advocates focusing on neglected solutions and (b) critically examining the plausibility of having a large impact in a crowded space.

We will dive into those issues more deeply in our upcoming retrospective grant evaluations (also see "Capitalizing on the Biden Moment" below).

On balance, we currently believe that advocacy -- when focused on neglected approaches and from organizations with a convincing past track record on influencing policy -- provides a strong impact multiplier.

Trajectory Changes

We think it is likely that **engaging around trajectory changes** -- in situations where decisions will have consequences for long time-spans due to self-reinforcing dynamics and/or stickiness provide another impact multiplier, **although we think this requires pairing with assessments of neglectedness**.

In principle, the climate space is full of dynamics which have this trajectory-shaping character, for example:

- **(1) Virtuous cycles around technological change:** Cost reductions, increased demand, further cost reductions, as seen with solar, wind, electric cars, and batteries.

- **(2) Vicious cycles around carbon lock-in:** Infrastructure and regulatory choices that favor particular technologies, capital-intensive investments with long lifetimes, etc.

- **(3) Virtuous cycles around social movements and shifting social norms:** E.g. the emergence of a national-level climate movement, once that movement reaches national name recognition it can profit from lots of attention and funding.

- **(4) Spreading of policy ideas:** The adoption of policy ideas across the world based on one or a few leading examples, as observed with carbon taxes, emissions trading systems, binding climate laws, net-zero commitments etc.

What unites those phenomena is that a decision is amplified, that it makes developments more path-dependent giving outsized importance to initial conditions and influences.

While this gives the basic rationale of why we should expect outsized impact, increased "hinginess", there are also considerations that push against this phenomenon as an impact multiplier in this context:

- **(1) Obviousness and resultant crowdedness:** The primary objection we see is that this trajectory-shaping nature is, overall, a well-known phenomenon so that in situations of competing interests, i.e. most situations relevant for climate progress, different players take this fully into account and thus crowd the space according to its special importance making additional engagement as or even less valuable than at other times.

- **(2) Path dependency is weaker than anticipated:** It could be that path dependency stated actually turns out much weaker, for example that seemingly locked-in infrastructure choices become upended by new inventions.

- **(3) Trajectory-shaping character is hard to predict:** It could be that most trajectory-shaping moments are not recognized or recognizable as such.

We think that (1) is the most serious objection here and warrants scrutiny in particular contexts. For example, we are unsure about whether engaging around COP-26 on promoting a report (the grant described in "Promoting a report on transformational benefits of technology-specific innovation policies") profited from this special moment or was rather, compared to attention at another time, crowded out by a very busy media environment.

But it is our impression that there are a lot of trajectory changes that do not receive sufficient attention, e.g. around carbon lock-in in emerging economies, i.e. that the combination with analysis of neglectedness can help identify opportunities for large positive impact. We also think that while (3) presents a challenge for many trajectory-shaping moments of a societal nature, there are many techno-economic dynamics in the climate and energy space that are sufficiently regular to detect, such as the described investment cycles and virtuous cycles around early technology investments and triggered dynamics.

Thus, on balance we think that "trajectory changes" are often an impact multiplier to consider. We next discuss two impact multipliers that are particular trajectory changes of particular kinds.

Catalytic growth

We believe that "overhead", unrestricted funding or funding directly targeted at improving the workings of an organization -- such as operations, fundraising, but also communications and strategy -- is often underprovided for small-to-medium-sized organizations and, as such, donors can maximize their impact by giving unrestrictedly or intentionally funding such positions to allow promising organizations to grow faster, become more resilient, and become more impactful. **Rather than the negatively connotated "organizational overhead" we think it would be better to understand it as an "organizational multiplier".**

There are four important reasons why we have come to believe this:

- **(1) In the current attention and funding surge large, mature climate organizations have a relatively easy time fundraising.** The most pronounced example of this is that traditional "Big Green" groups, such as NRDC, World Resources Institute and others have received hundreds of millions from Bezos' Earth Fund while already well-funded before. Similarly, large progressive grassroots organizations such as the Sunrise Movement have profited from the attention increase to climate and received very large donations. This makes it even less plausible than in normal times that marginal donations to these organizations can have large impacts.

- **(2) At the same time, small organizations are often not only funding-constrained but also bandwidth-constrained leading to a vicious cycle.** We have experienced many times that small organizations, led by incredibly talented advocates and subject-matter experts, are unable to scale as fast as the moment demands and the general climate funding landscape would allow. A world-class advocate is not necessarily a world-class fundraiser or organization builder. Crucially, we believe that these issues can be fixed with targeted investment in organizational infrastructure.

- **(3) "Overhead" has a bad name and is harder to get by than programmatic restricted funding.** This is well-documented and makes it likely that organizations underinvest in organizational capacity relative to returns.

- **(4) Funding growth in a growing field is lower-risk than at other times:** As long as one does not expect an imminent collapse of climate philanthropy, which seems unlikely given the rising attention and large investments in building problem awareness, it is likely that organizations with accelerated growth are able to fundraise from a variety of sources.

We believe there are also two important reasons that should be part of a balanced consideration of this kind of funding:

- **(5) Risk of organizational failure:** Of course, smaller organizations are more likely to fail entirely and this needs to be accounted for.

- **(6) Carbon lock-in and delayed impact:** As more and more climate decisions get locked-in, investments in future organizational capacity warrant more skepticism than in other causes.

We are fairly confident that, on balance, reasons (5) and (6) do not outweigh the significant benefits provided by (1)-(4).

To put numbers on it, we think it is not unreasonable to expect a 5-10x multiplier from organizational investments targeted at faster and more resilient growth in terms of crowded- in money and increased impact, whereas (5) and (6) would lead us to discount this estimate by a factor of 2-4x.

Of course, this is a very rough estimate and we have put metrics in place by which we will evaluate our grants based on those arguments to get better calibrated.

Global diffusion of technological change

There are at least two mechanisms through which technological change contributing to outsized changes in the global energy system with emissions consequences far beyond can be induced (and often, those mechanisms will act at different stages of the same innovation process):

(1) Research & Development & Demonstration (RD&D), i.e. early-stage innovation support ("technology push").
(2) Demand-policies for early deployment, such as deployment subsidies, public procurement, standards requiring new technologies ("demand-pull").

We consciously include both mechanisms, as both have proved vital for global decarbonization successes to date (see e.g. Kavlak et al. 2018. Nemet 2019, Grubb et al. 2021).

Because each country is small compared to global emissions, for countries with significant innovation capacity the leverage on emissions will probably be higher than through domestic reductions (also see "Future emissions are not where most climate attention is").

Both mechanisms share the following four characteristics:

(a) **A fairly localized action not requiring global coordination has a long-run global impact.** This deals with the central challenge of climate policy, that in the context of multi-decadal global decarbonization efforts, any domestic emissions reduction is ultimately fairly insignificant.

(b) **While that impact might be predictable, it is not observable and not attributable in the short-term domestically making it likely severely underprovided.** For example, governments under pressure to show progress on climate and meet domestic targets cannot claim global innovation benefits of championed policies against their legal commitments or with their constituencies (and companies always have an incentive to under-provide innovation if they, what is the rule for most early-stage efforts, cannot fully capture the benefits). **Crucially, we also think innovation advocacy remains underprovided philanthropically**[27], as discussed under "Future emissions are not where most climate attention is" a large majority of climate philanthropy focuses on relatively mature technologies, though the situation has probably improved somewhat.

(c) **Choices are often less politicized:** Unlike top-level climate policy choices, such as about carbon pricing levels or national emissions targets, innovation policy levers

[27] This is crucial because it could be the case that government by itself under-provides innovation but philanthropy engages to the maximum degree, making additional innovation a high-impact proposition but additional philanthropically funded innovation advocacy a low-impact one.

are often less politicized along national-level political divides[28] and, thus, more likely to be influenceable by advocates. This is somewhat less true for deployment-oriented policies once the technologies reach higher market penetration and the involved subsidies become more economically significant.

(d) **High effectiveness in the worst worlds:** In the worst climate futures with lots of energy-intensive growth and little appetite for global or national climate policy, where most climate risk is concentrated, innovation will still make a meaningful difference to emissions outcomes (and adaptation).

We think the primary objection against strongly leveraging this impact multiplier -- by supporting philanthropy targeted at improving innovation outcomes via policy or otherwise -- is that not all championed technologies will succeed and thus some such efforts will not reap any mitigation benefits. As discussed in our section on "Risk neutrality" we believe this is a weak criticism, it is much more important to pursue strategies that have high impact in expectation and it is not important which particular of a given set of effort succeeds.[29]

There is one important caveat to this impact multiplier, namely that **its effects will be the strongest for early-stage technologies** and that, once technologies reach maturity, become competitive in the marketplace and the political marketplace (building strong special interest coalitions), additional support will become less valuable. For example, while solar PV was a prime candidate for this advocacy 15 years ago, with much larger technological maturity (and much less technological learning and cost reduction per unit), this multiplier does probably not apply for solar PV advocacy in OECD economies right now, but it could -- for example -- apply to innovation-support advocacy for next-generation

[28] That does not mean they are not political, but they might be political in different ways, for example in more distributional "pork-barrel politics" with political coalitions around constituency interests rather than partisan lines.
[29] For example, in our grantmaking focused on dealing with committed emissions from coal plants we are supporting at least three different approaches -- (i) carbon capture and storage, (ii) re-powering with advanced nuclear and (iii) re-powering with enhanced geothermal. To a degree, these approaches do compete and probably not all of them will succeed, but we think this is a weak downward consideration on what is overall still a very strong impact multiplier.

solar technologies, such as perovskites.

Policy additionality

Being additional is not only about being additional to other funders but also about being additional to existing policy targets.

In jurisdictions with stringent and credible emissions targets, such as the EU and the UK and some US states, philanthropic efforts to reduce domestic emissions, e.g. via encouraging more energy-efficient housing insulation, might not create additional emissions reductions at all if sector or economy-wide targets are set and enforced. Rather, in this case the additional philanthropic action just makes a set policy target easier to reach.

While one can always argue that maybe targets would not be reached or that additional progress motivates additional ambition in the form of stricter policy targets, this is never certain (and often implausible) so in those settings **there should at least be *some* discounting of expected effects for the case that the same reductions would have been achieved otherwise.**

In that sense, "Policy Additionality" is not really an impact multiplier, but rather an "impact differentiator" as some actions will be unaffected and others' effects should be more strongly discounted.

We think that a philanthropic action will be more additional, i.e. its effects should be less discounted in the following circumstances:

(1) **Effects are outside a jurisdiction with strict and credible targets** (e.g. not domestic reductions in the UK where there is a binding climate law)

(2) **Effects are further in the future**, where (i) targets are less credible and (ii) where additional progress makes it more plausible that targets are indeed strengthened.

This "multiplier" is a fairly strong argument in favor of innovation advocacy given that the benefits of innovation are (a) temporarily delayed, (b) often significant enough to lead to stricter targets and (c) distributed globally, i.e. also leading to emissions reductions in jurisdictions where climate policy is weak.

On the other hand, this mechanism makes us considerably less optimistic about the impact of accelerating domestic short-term targets, i.e. accelerating coal-phaseout in the EU when there is already a strong policy commitment to reduce emissions making the full additionality of such actions less plausible.

Theories of change

We now discuss the different theories of change we *currently* believe to have the highest impact, relating them back to the impact multipliers identified.

Accelerating innovation in neglected technologies

Related concepts: Global diffusion of technological changes / Risk Neutrality / Patience / Trajectory changes / Neglectedness (technology) / Advocacy

This theory of change focuses on funding organizations advocating for more attention to, and improved, policies to drive innovation in neglected-yet-critical technologies such as carbon removal, carbon capture, industrial decarbonization, super-hot rock geothermal or advanced nuclear. As discussed under “Global diffusion of technological changes”, the understanding of innovation policies here is broad and covers both basic RD&D policies as well as “later-stage” innovation policies such as early procurement policies and market creation (“demand pull”).

This theory of change can be schematically summarized as follows and we include a worked back-of-the envelope cost effectiveness analysis in the Background section:

Figure 14: Schematic representation of advocacy to drive acceleration of neglected technologies.

There are three primary reasons why we believe this to be an extremely promising theory of change, corresponding roughly to the impact multipliers leveraged:

- **(1) Advocacy:** Affecting innovation budgets that are 100-1000x larger than budgets of advocacy organizations and contributing to the conditions in terms of policy response and innovation ecosystem that enable transformative change.

- **(2) Global diffusion of technological change:** Leveraging this impact multiplier to deal with the central conundrum of the climate challenge -- that future emissions are, by and large, not where the willingness and ability to pay for climate action right now are concentrated.

- **(3) Focused on neglected technologies:** Focusing those efforts on technologies that receive less attention than their import for global decarbonization would suggest optimal is the third lever for high-impact here, as neglected technologies will be those where returns to additional advocacy (and, consequently, policy support) should be the largest, everything else being equal (also see discussion of "Neglectedness" above). It is important to emphasize in this context that neglectedness varies in different political environments and political spheres, e.g. while the fossil fuel industry is one of the most powerful industries in the world, public support for carbon capture can still be underprovided if there are very few actors with credibility in the climate community that advocate on its behalf.

We think there are four dominant objections to this being a particularly impactful theory of change:

- **(4) Time lag:** The chain from advocacy to policy change to technological change will often take some time, thus most of the emissions savings will be at least five years, quite possibly, fifteen years in the future. As we discuss under "Patience" we think this delay is a likely reason this kind of philanthropy is underprovided, while the dependence of peak warming on cumulative emissions makes this a relatively weak counter argument, emission savings fifteen years in the future should not be discounted much compared to emissions savings now.

- **(5) Any particular technology might fail to reach maturity:** This is true, but as discussed under "Risk Neutrality" this should not discourage us from pursuing bets with high expected values, i.e. into neglected technologies (high additionality) that have a plausible shot at making a large difference to decarbonization outcomes if they succeed.

- **(6) Even if innovation succeeds, it might be locked out and cannot make a significant difference:** It is a common phenomenon in the history of technology that superior technologies, even if technologically ready, fail to replace incumbent technologies and, in the context of the global energy system, where many choices are related to decadal timelines involving regulatory, infrastructure and investment decisions, this "carbon lock-in" is probably the most plausible trajectory that combines technological success with climate failure. We see this as by far the strongest counter-argument limiting the impact of this theory of change, which is why we have included the avoidance of carbon lock-in as a complementary theory of change (also see the ToC-discussion below, as well as the "Robust diversification" section on how those theories of change complement each other under deep uncertainty and a situation of non-linear climate damage).

- **(7) Innovation advocacy is becoming less neglected:** In a world where Bill Gates publishes an entire book about the merits of energy innovation (*How to Avoid a Climate Disaster*) is it plausible that innovation advocacy is still highly impactful at the margin? As we discuss under "Future emissions are not where most climate attention is" we do think that innovation advocacy has indeed become somewhat less neglected (and this has affected our grantmaking and expansion into other theories of change), we also think this point is easily overstated -- for example Jeff Bezos' philanthropic engagement dwarfs Bill Gates's and is, by and large, until now fairly focused on mature technologies.

On balance, while we do think that reasons (6) and (7) somewhat reduce the strength of the theory of change, we still do think that reasons (1)-(3) make it one of the most compelling theories of change in the climate space and, in particular, we think this theory of change should still play a much stronger role in OECD economies where future emissions are low and partially constrained by policy, but innovation capacity is large.

Policy leadership and paradigm shaping

Related concepts: Risk Neutrality / Patience / Trajectory changes / Advocacy

Given (i) the importance of ideas for shaping trajectories of decision-making and the diffusion of policy ideas and policies -- as discussed under "Trajectory Changes" -- we currently believe that philanthropy focused on shaping ideas could be highly impactful.

However, we also think that tracing the impact of such philanthropy in a clear way is ultimately likely infeasible, and thus we currently believe that claims to high or low impact in this domain will remain unresolvable.

Avoiding carbon-lock in

Related concepts: Risk Neutrality / Patience / Trajectory changes / Neglectedness (geographic) / Advocacy

Carbon lock-in describes a set of phenomena that make current decisions stick for decades, via long-lived carbon and capital intensive assets (e.g. new coal and steel plants), infrastructure choices, and other forms of lock-in (e.g. regulation, special interest coalitions) that limit the speed of (future) low-carbon transitions. For example, while renewables have become much cheaper recently affecting the new-build of coal, the world has a lot of very new coal assets that are, in the best case, challenging to prematurely retire (and in the worst case won't be retired and commit the world to a > 1.5C warming trajectory). This phenomenon, combined with the expected source of future emissions (85% of emissions will be outside the US and EU), provides the principal argument for engaging in emerging economies with current low per-capita emissions.

The following graphs, from Guetschow et al 2021's paper on downscaling SSP/RCP scenarios to the country level (here, however, aggregated to regions) illustrates the basic rationale well. Both historical trends and future projections suggest that emissions will continue to trend downwards in the OECD (and indeed, we believe this is underrepresented in these models, see "Climate attention is not where future emissions are" and "Policy Additionality" above), whereas they will increase sharply in Asia and the Middle East and Africa.

The **basic rationale for interventions to avoid carbon lock-in then is that these trajectories can be positively affected now and that, because of the long-lived consequences of many investment and policy decisions, this will lead to large *additional* emissions reductions in the future.**

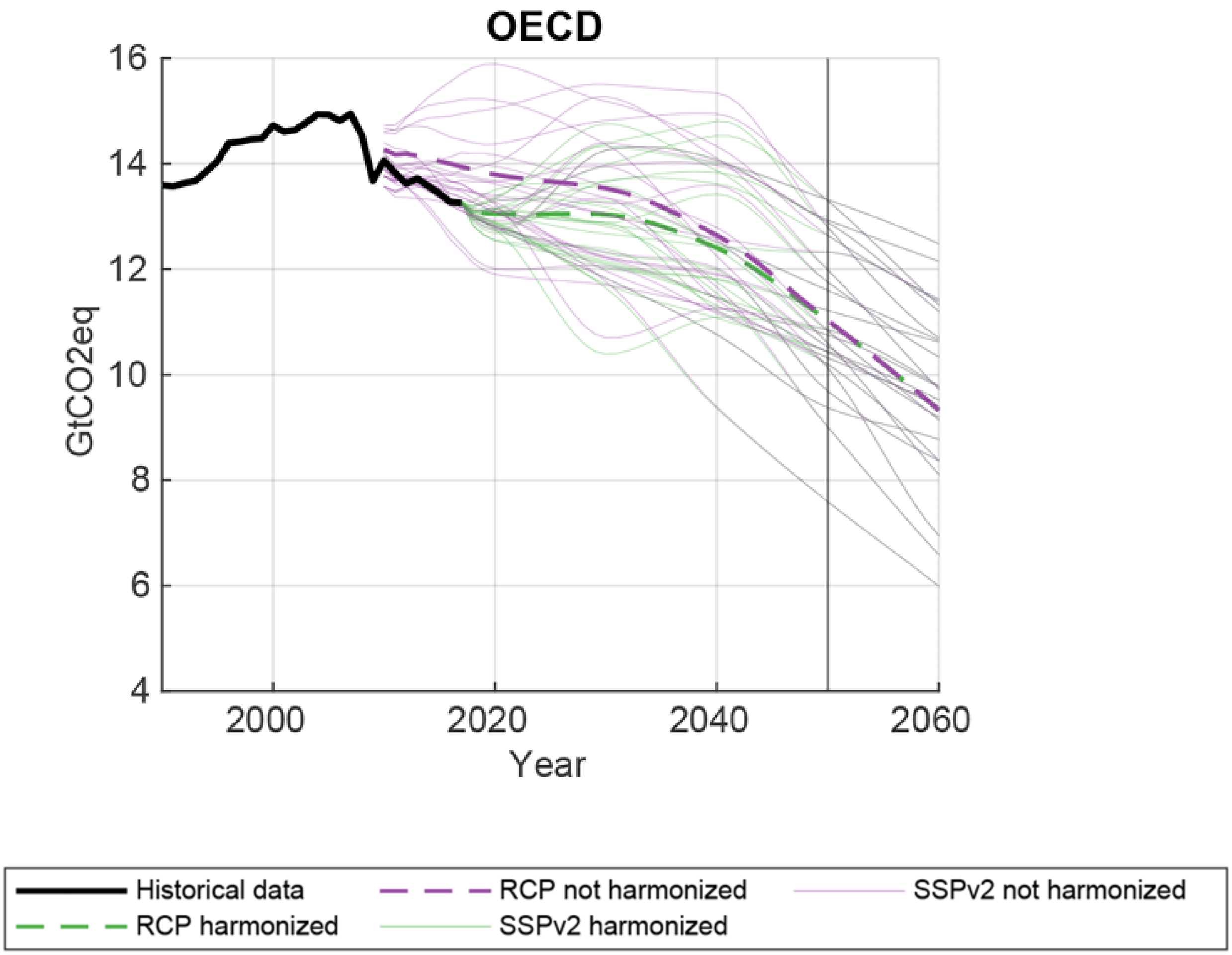
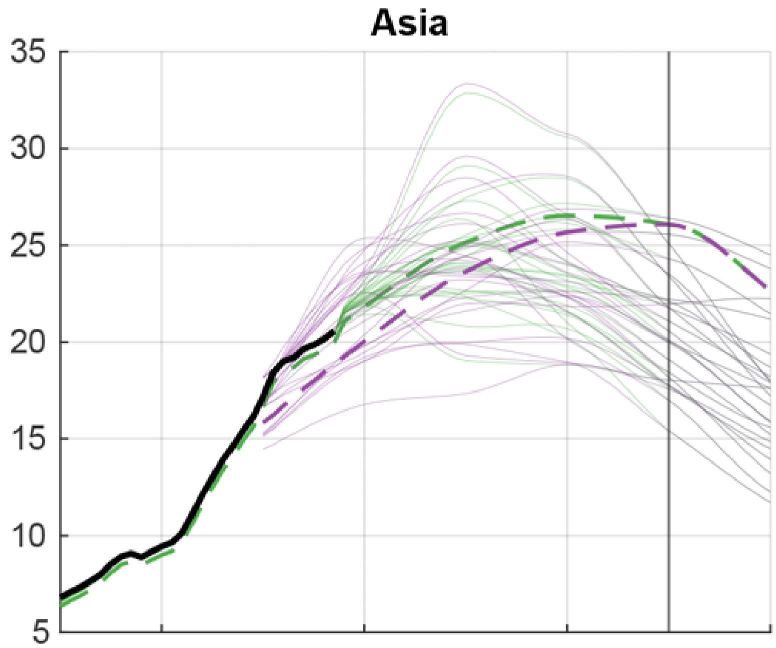


Figure 15: Emissions trajectories for OECD economies (excerpted from [Guetschow et al 2021](), Figure 5)


Asia
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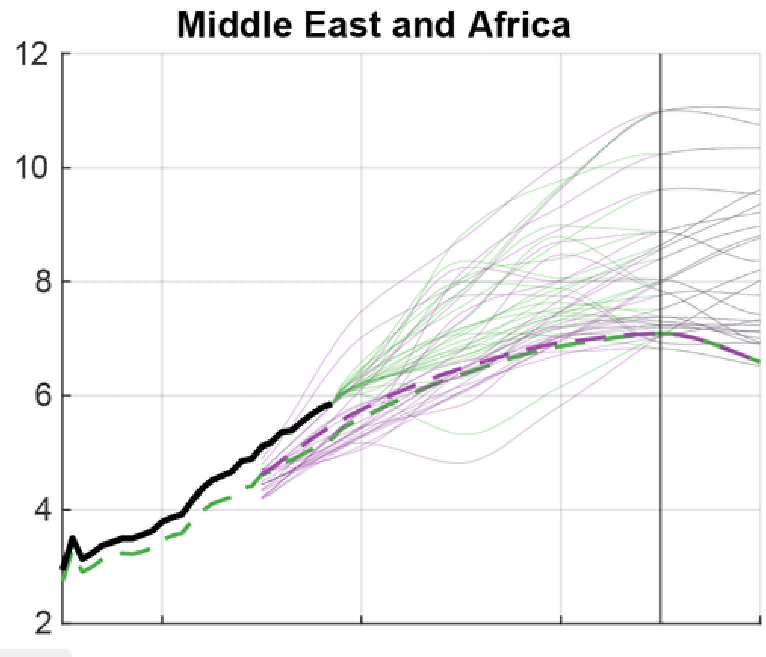

Figure 16: Emissions trajectories in emerging regions (excerpted from Guetschow et al 2021, Figure 5)

Based on the academic literature on carbon lock-in (e.g. see Seto et al 2016 for a review) and our own analysis, we think that **optimal and most additional work to avoid carbon lock-in has**, everything else being equal, the **following characteristics**:

(1) It is **focused on sectors that are not yet trending clean**, i.e. inducing trajectory changes rather than marginally accelerating business as usual transitions (e.g. prioritize inducing change in heavy-duty transport rather than accelerating already locked-in diffusion to electric vehicles in light-duty transport)

(2) It is **focused on sectors with capital-intensive long-lived assets**, such as coal and steel plants **or other choices that have long-lived consequences**, such as about transmission infrastructure, carbon capture hubs, or other nfrastructure.

(3) It is focused on **assets that are harder to retrofit**, e.g. worrying more about inefficient cement plants (with lots of process emissions that are hard to avoid) than aluminium plants that mostly require electricity and that will "automatically" decarbonize as the power sector trends cleaner.

(4) It is **focused on countries where ambitious climate policy is less likely**, i.e. the probability that assets "just" become stranded (imposing high cost to decommission, but this happening) is less likely, but it is more likely that assets emit until the end of their lifetime. E.g. while new gas infrastructure in the EU might need to be decommissioned (or retrofitted) before the end of its lifetime, the fact that the EU's ambitious climate policy (and strong climate constituencies) make this likely to happen should lead us to worry about this less than in other jurisdictions where climate policy is less determined (also see "Policy Additionality").

Of course, this framework is partial, focused on additionality of emissions avoidance (roughly: importance + neglectedness), whereas other factors, such as tractability (ability to make progress), also matter. For example, it could be the case that accelerating the diffusion of a mature and succeeding technology, say solar, is so much more tractable that this outweighs lock-in considerations. It is our current best guess that the difference in tractability does not outweigh lock-in differentials.

We think there are three primary reasons to expect additional philanthropic investment into this theory of change to be quite cost-effective:

- **(1) Neglected:** While impossible to exactly specify (see above), it appears that this work is quite neglected and that philanthropy in emerging economies is heavily focused on a small set of solutions, in particular renewables for clean electricity and forests and other natural climate solutions. This makes it likely that there is a lot of valuable work that could be done to avoid carbon lock-in via support for other clean electricity solutions (nuclear, carbon capture) as well as for many lock-in relevant sectors that are relatively unaffected by electricity, i.e. cement and parts of steel production.

- **(2) Decisions are not locked in:** While a lot of emissions streams are either related to very new infrastructure and likely hard to affect ("committed" emissions) or on a clear locked-in reduction trajectory (light-duty transport,

- **(3) Complementarity with innovation advocacy:** Given the *potential* that we are now on a trajectory towards investing sufficiently into energy innovation, the question arises "*How might we still fail?*". We believe **the primary failure mode if innovation succeeds is that carbon lock-in strongly limits its effects**, thereby committing us to a high-emissions trajectory. We thus think this work is a natural complement to innovation advocacy given that the uncertainties are negatively correlated (also see discussions above).

- **(4) Protection against the worst worlds:** A large amount of climate risk comes from emerging economies repeating the fossil-intensive growth trajectory, as, for example, modeled in SSP5-RCP8.5 scenarios (also see "The geography of 21st century emissions and of climate philanthropy"), i,e. a disproportionately high share of climate risk comes from worlds of high carbon lock-in.

We think there are three primary reasons to be skeptical:

- **(5) Downside risk of conflict with energy poverty alleviation:** There are many ways to affect the emissions trajectory of emerging economies that conflict with the goal of overcoming energy poverty, such as anti-fossil advocacy, or the overwhelming focus on a small set of solutions (e.g. decentralized renewable power) at the exclusion of other possibilities. We think overcoming energy poverty is at least as important as limiting climate change, hence we do not support work that directly or indirectly restricts energy access.

- **(6) Affectability could be quite limited:** It could be the case that trajectories are hardly affectable through advocacy engagement now.

- **(7) Localized effects:** Even if affectability is given, it could be the case that this kind of work is dominated by additional work with global effects, such as additional innovation advocacy. While we do not exclude this possibility, we think this has become less likely to be true as innovation advocacy has become less neglected.

On balance, we think that this is likely one of the highest-impact theories of change and we will continue to act in this space, both through grantmaking and grantmaking research, as well as learning through grantmaking and further intervention prioritization research.

Growing promising organizations

Related concepts: Risk Neutrality / Patience / Catalytic growth / Neglectedness (type of funding)

This is not a fully independent theory of change, but merely applies a "meta" theory of change to organizations considered promising because they are effectively implementing one or more of the other theories of change discussed. As explained under "Catalytic growth" we believe this to be an under-provided strategy to create higher impact.

Other theories of change and interventions we will consider

In terms of research directions, there are a couple of specific interventions and theories of change we will consider in 2022:

Under the theory of change of **avoiding carbon lock-in:**

- (1) We are exploring **how air pollution and climate concerns interact at the level of specific fundable interventions**, whether funding clean air advocacy is a viable intervention to reduce carbon lock-in and realize enormous public health benefits and how this compares with what we are otherwise funding. We are exploring this particularly in the context of Southeast Asia, where a lot of emissions growth is expected to occur, but with significantly lesser existing attention than in China and India.

- (2) We are exploring **whether acceleration of mature technologies, such as solar PV, in emerging economies** where energy demand increases the most is a plausible high-impact theory of change, whether entrepreneurial ecosystems and market conditions can be improved and whether this is still neglected (given recent very large funding commitments for renewables in developing economies).

Under the theory of change of **accelerating innovation of neglected technologies:**

- (3) We will examine whether there are key neglected technologies or geographies where we should consider providing additional support given the recent uptick in attention.

We are also likely to examine whether there are early-stage social movements that could be worth supporting, given that (a) mature climate movements are very well-funded but that (b) in general, these movements have been very impactful.

We remain keen to fund small organizations that have high potential for scale and outsized impact.

Our Grantmaking

There are many ways to slice up the grants, we organize them here by theory of change, but we include all the other aspects they relate to as well

Policy leadership and paradigm shaping

We have, thus far, made only one grant under this theory of change -- described below. We are keen to make more grants in this area if we find similarly promising opportunities.

Promoting a report on the transformational benefits of technology-specific innovation policies

It is a common problem in climate discussions that those policies that have arguably been the most successful in driving decarbonization -- technology-specific innovation and support policies that have driven the success of solar, wind, and electric cars -- have a bad reputation as costly, inefficient, and highly uncertain. While the evidence base for their effectiveness becomes increasingly clear, there is still hesitancy to repeat the same approach for other critically needed technologies -- such as carbon removal, low-carbon steel, carbon capture and storage, and advanced nuclear, to name a few.

We believe changing this is critical, as innovation policy is the most plausible strategy to deal with the central conundrum of the climate challenge -- that future emissions are, by and large, not where the willingness and ability to pay for climate action right now are concentrated.

So, we were excited to be approached to comment on a report prepared by leading international academics that would make those points in a much more rigorous fashion and translate them for policy makers in key economies, including key emerging economies of China, India, and Brazil. Asked whether we could do anything more to support it, it became clear that more could be done to amplify the report's message via a targeted PR effort which is what this grant is about. *The New Economics of Innovation and Transition* is the flagship report of the EEIST project. It was launched at the World Leaders Summit on

Innovation at COP26. A VIP influencer evening reception was held on 4 November in the UK Pavilion at COP, to promote the findings more widely. This can be watched here.

As of this writing, an article making the core arguments of the report in a digestible manner syndicated via the Press Association has been published in over 150 newspapers around the world, including the Daily Mail and the Independent. The report was also featured in an original article in the Guardian, BBC Radio 4, Brazilian media Estado and Brasil Energia (no links) and China Daily, and there are likely other pieces forthcoming. According to analytics, the media amplification of the report has reached an estimated 4M coverage views.

As discussed in the theory of change section, we do not see clear ways of such grants to track impact on emissions directly, but believe it would be a mistake to not make them for this reason.

Avoiding Carbon Lock-In

We have made one grant under this theory of change and are currently examining two further grants.

CATF Globalization and Organizational Investment Grant

This is an organizational investment grant with the intertwined goals of (1) investing in organizational infrastructure and reorganization to (2) enable an ambitious geographical expansion, including expansion into China, India, Asia-Pacific. Middle East and North Africa (MENA) as well as a strengthened presence in Sub-Saharan Africa.

Most climate philanthropy and most environmental NGOs are focused on a fairly narrow set of solutions to the climate challenge -- often driven primarily by ideological preferences rather than evidence-based strategies.

In general, this seems to be *more true* outside the US, with US discourse and policy on solutions being comparatively more technology-inclusive and more positive about technological innovation to address the climate challenge.

For example, emerging climate foundations in key regions such as the *African Climate Foundation* and the *India Climate Cooperative* appear entirely focused on renewables; this is similarly true for those actors heavily involved in China (e.g. the *Energy Foundation China*). It is our current impression that most climate philanthropy targeted outside the OECD is more "classical environmentalist" and, as such, has predictable biases (also see our discussion of funding trends under "Future emissions are not where most climate attention is") .

This provides a strong reason for CATF engagement in those regions: We see the main way in which CATF provides value in challenging and complementing mainstream discourse by (i) emphasizing parts of the problem that need more attention (hard-to-decarbonize sectors, such as industry, heavy-duty transport, etc.) and (ii) solutions that are neglected compared to their potential (carbon capture, super-hot-rock geothermal, advanced nuclear, etc.). In addition, another (iii) important reason to prefer CATF over many other Western environmental NGOs in those regions is the organization's positive development vision, recognizing that a broad portfolio of low-carbon solutions is a much better bet to avoid trade-offs between energy development and climate mitigation than a narrow focus on traditional renewables that risks more fossil build-out in defiance of Western donors or imposing emissions reductions by limiting critically needed energy demand growth.

More specifically, CATF has the following expected foci (which we also expect to change with time):

Region	CATF Foci	Evaluation

China	Supporting China in its low-carbon ambitions through technology support, particularly with CCS and geothermal (possibly, advanced nuclear).	Has a dual nature, as the focus is not only on avoiding lock-in but also in supporting the Chinese innovation system. A great investment even on quite pessimistic assumptions given China's unique dual role as large future emitter and innovator and exporter of low-carbon technology.
Asia-Pacific	Enabling regional integration around CCS and zero-carbon fuel infrastructure as well as driving shipping decarbonization.	Heavily focused on infrastructure (which is good, see above), but we found it too focused on OECD Asia given the large lock-in risks new fossil infrastructure in Southeast Asia. We still include it in the grant given that we expect strategy will develop and we have also not found other actors with convincing decarbonization strategies for Southeast Asia.
India	Creating a model for power grid development accelerating economic growth while incorporating zero carbon options and beginning a targeted development of zero carbon transport and industrial options.	Given widespread energy poverty / lack of energy access in India, the combined focus on grid and low-carbon development appears correct. Population and economic trajectory of India should lead us to expect strongly increasing energy demand that can still be affected, providing a strong rationale for engagement, including on geothermal and (advanced) nuclear support.
Sub-Saharan Africa	Supporting the development of well-functioning power markets, including support for centralized grid build-outs, and support for firm low-carbon power enabling a low-carbon future.	Given the severity of energy poverty in much of the region, we think the focus on combining grid development with low-carbon support is exactly right, a useful alternative to a Western discourse often focused on micro grids. While SSA contributes almost nothing to current emissions, engaging early to reduce

		risk from long-run lock-in is valuable; short-term wins around conventional geothermal acceleration in East Africa seem possible, with more speculative wins around super-hot rock geothermal and advanced nuclear.
Middle East and North Africa	Supporting MENA countries to build a blue hydrogen-export economy that makes decarbonization politically feasible.	MENA is largely neglected by traditional climate philanthropy which makes this expansion particularly valuable. Supporting MENA to become committed to green and blue hydrogen development is a promising strategy to overcome this region's strong opposition to climate policy and provide zero-carbon fuels.

Table 8: CATF Foci in regional expansion

These foci compare favorably with our understanding of avoiding carbon lock-in, strongly focused on infrastructure and other long-lived decisions and on sectors of the economy and solutions that are currently quite underfunded in climate philanthropy.

Grants under consideration

We are also investigating further grants under this theory of change, focused on **co-benefits of air pollution and climate advocacy in Southeast Asia** (Clean Air Asia) and **accelerating mature clean** technologies, such as solar PV, **through strengthening cleantech ecosystems** in emerging economies (New Energy Nexus). We currently believe that a fair amount of our future grantmaking will be concentrated in emerging economies, in particular if -- as is to be expected -- the US political opportunity somewhat dries up after the 2022 midterms.

Accelerating innovation in neglected technologies

Capitalizing on the Biden moment

Based on our analysis of the special opportunity for climate impact under a Democratic President in a political environment with unusual willingness to spend boldly in the wake of COVID-19, we deployed USD 850,000 to the Clean Air Task Force (CATF) and USD 400,000 to Carbon180 directly after the Biden victory to enable those organizations to optimally engage with the incoming administration and utilize the momentum to push for innovation in neglected technologies.

While the bipartisan infrastructure bill has become law, the "Build Back Better Plan", the Democrats-only climate and social spending bill, has not yet passed the Senate, not allowing for a final analysis of impact of those grants and our predictions. **However, from our intermediate understanding, we believe that the grants have been quite successful.**

Several of Carbon180's policy suggestions have recently been taken up by US policymakers. C180 recommended that the Department of Energy launch an initiative to reduce the cost of carbon removal to $100 per ton and recommended that appropriations for carbon removal be significantly increased. Both ideas were implemented: the recently enacted infrastructure bill contains 3.5 billion in new funds for direct air capture (DAC) efforts.[30] Similarly, the infrastructure package reflects many of CATF's priorities, such as increased support for carbon capture and storage (CCS) and hydrogen infrastructure, industrial decarbonization, and advanced nuclear demonstration, with a total of USD 30bn for clean energy provisions championed by CATF. Because both Carbon180's and CATF's

[30] In 2015, C180 was among the first organizations to advocate for DAC to be eligible for 45Q, the federal tax credit for carbon sequestration, and has consistently worked to raise the value of DAC under that program.

foci are overall fairly neglected on the political left, there are few fervent advocates for carbon removal, CCS, and advanced nuclear, it is plausible that these organizations had significant impact in the provisions of the infrastructure bill they worked on. We will provide a more detailed and rigorous retrospective grant analysis once the Build Back Better Plan has passed as well (in 2022).

Innovation advocacy in Europe

While we believe that the US climate policy debate has become significantly more innovation-oriented, this is far less true in Europe. With at least five[31] think tanks or advocacy organizations in the US strongly focused on energy innovation for global decarbonization, it is our impression that this perspective is less reflected in Europe and that, while attention to climate and energy innovation is high, the innovation debate and policy is, by and large, more incremental and less technology-inclusive than in the United States, leaving significant potential for improving energy innovation policy and the energy innovation ecosystem more broadly. Given the large resource mobilization and commitment to climate action in Europe, this appears very valuable.

At the same time, this type of work is fairly underfunded philanthropically, for example a recent ClimateWorks analysis on European climate spending (ClimateWorks 2021b) listed “innovation” under “other strategies”, with a funding level of USD 1.5m/year, about 1/10 of philanthropy targeted at life-style changes.

This is why we are **excited to scale a new organization, Future Cleantech Architects (FCA)**, to help positively shape German, European, and global debates on innovation priorities. Over the past year, after being approached as advisors, we have closely observed

[31] CATF, ITIF, Breakthrough Institute, ThirdWay, Breakthrough Energy

this organization have had impressive initial successes[32] and we are now ready to invest in its ambitious growth, **supporting the organizational development as well as key programs in hard-to-decarbonize sectors requiring more innovation, namely zero-carbon fuels, industry, long-duration storage, and carbon removal technologies**. We believe that if FCA is successful this could significantly improve the German and European climate policy response, while -- at the same time -- this kind of organization is much rarer in Europe than in the United States.

Advanced nuclear for hard-to-decarbonize sectors and committed emissions

We believe that TerraPraxis continues to do incredibly important work around shaping a conversation for advanced nuclear to address critical decarbonization challenges, such as the decarbonization of hard-to-decarbonize sectors and the conundrum of how to deal with lots of very new coal plants that are unlikely to be prematurely retired.

For this reason, beyond our incubation grant in 2020 (see below) we also made a grant to support an event at COP-26 focused on coal-repowering, described in more detail here.

[32] Such as conducting and publishing a cleantech R&D priorities survey through the World Economic Forum and the TEC committee of UN Climate Change, hosting a cleantech innovation call with three UN organizations and presenting key neglected R&D needs in two events at COP 26 in Glasgow as well as taking the only European perspective in the release of ITIF's 2021 Energy Energy Innovation Index.

Growing promising organizations

We made four grants related to this theory of change (some of which already discussed above, as they are mixed grants):

- (1) Our initial 2020 grant to TerraPraxis (then "Energy for Humanity") was the first major philanthropic commitment for this organization and helped them establish their presence and attract significant additional funding, about 4x our initial investment.
- (2) In late 2021, we decided to make an explicit organizational investment grant to TerraPraxis, funding a COO and a Fundraising Director position, to accelerate the growth of the organization, enable leadership to more fully focus on programmatic work, and complement funding from other sources fully dedicated to programmatic work.
- (3) Our late 2021 Future Cleantech Architects (FCA) scaling grant contains an explicit organizational development component to allow the organization to grow healthily and attract additional funding.
- (4) Our late 2021 CATF Globalization & Organizational Investment Grant contains significant investments into strategy, operations, and HR capacity and we believe this aspect of the grant to be the most additional.

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Background

Conservative impact analysis example of innovation in neglected tech

An example of advocacy's benefit within the climate space, and more specifically driving carbon capture[33] deployment and innovation, is the work of the Clean Air Task Force (CATF) that went into the 2018 reform of a tax credit 45Q, making it the most significant carbon capture incentive policy in the world. The significant role played in this by CATF was analysed as part of Founders Pledge's 2018 Climate Change Report. Were it not for this advocacy work, it is quite plausible that this increase in 45Q would not have occurred, or may have occurred much later.[34]

Even making highly conservative assumptions, our impact evaluation of this intervention finds that the role of advocacy is highly cost-effective. When evaluating the impact of CATF's work on 45Q, we conservatively assume that:

1. There is a 50% chance that 45Q has no impact on the development of carbon capture.
2. Even if CATF had not championed it, there is a 95% probability that other organisations would have pushed 45Q anyway.
3. Even if that had not happened, we assume that the increase in 45Q tax credits would have occurred two years later than it did.

All of these assumptions are extremely conservative, and are very much biased against finding advocacy to be an effective intervention.

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[33] Carbon capture is a critical technology, which -- according to median expert views -- is of vital importance if we are to have a chance of decarbonising globally, as are policies to accelerate innovation and deployment.
[34] "It is difficult to know what would have happened had CATF not acted in the way they did, and it is possible that another group would have stepped in to fill their role. **But the above evidence suggests that CATF's work was probably not replaceable** in this way. [...] One of the sources stated that **without CATF, it is very unlikely that the bill would exist at all**." (FP Climate Report 2018, p. 152)

However, even biasing ourselves against it, we still find that this advocacy will reduce one tonne of carbon for $1.63 in the US domestically, based on numbers provided by CATF on the cost of this advocacy (~4 million) and the estimated effects.[35]

But emissions reductions in jurisdictions that are decarbonizing and becoming a lower share of global emissions already (such as the US, UK, and EU) are not the primary target, rather the goal are cost reductions that materially impact global emissions trajectories.

Even if one assumes a modest learning rate of 10% per doubling of capacity – for reference, solar is approximately 30%, so this is a fairly conservative estimate – 45Q could halve the cost of CCS by 2030 when applying a sime one-factor learning model.[36]

[35] As estimated in this study. Note that we are not taking this study at face value, we are assigning a 50% probability that 45Q has zero effect.
[36] In their 2019 status report (p. 24), the Global CCS institute uses a learning rate of 8% as a conservative (not best-guess) estimate of the learning rate. We will use this rate in our next iteration of this estimate, but do not believe this to change results in a qualitative sense.

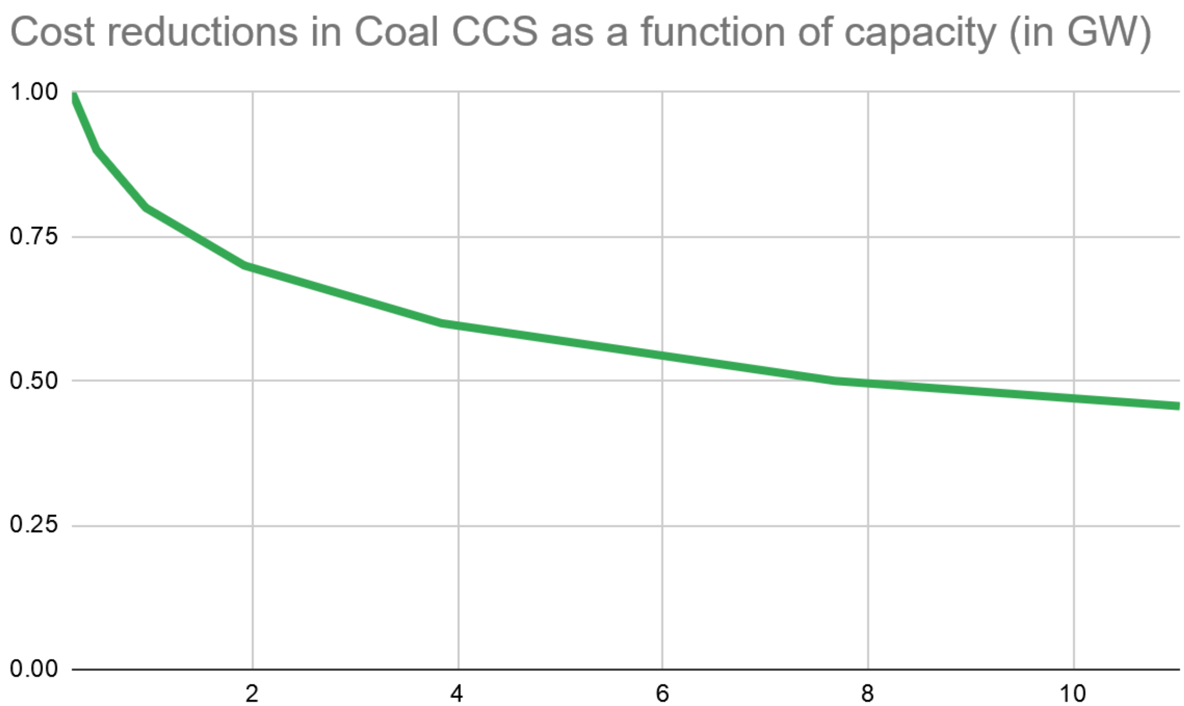


Figure 17: Capacity additions due to 45Q based on Charles River Associates Study, with initial capacity at 240MWe (PetraNova) and a simple one-factor learning model with 10% learning per doubling of cumulative capacity.

Conservatively assuming that a 50% cost reduction will result in a 10% increase towards average levels of carbon capture deployment in representative 1.5 degree scenarios[37], and combining this with our previous conservative assumptions, we estimate the global cost of saved carbon to be approximately $0.11/tonne. This is a notable reduction on the domestic cost of reductions ($1.63/tonne). Note again what this estimate assumes:

- A 5% chance that CATF was essential in moving a CCS incentive policy forward in time by 2 years.

[37] This is the average of the four representative scenarios discussed in IPCC 1.5 SPM.

- A 50% chance that this policy has no effect at all.
- If the policy has the estimated effects, a 10% learning rate and a 50% cost reduction leading to additional deployment of CCS at 10% of average CCS deployment levels in representative IPCC 1.5 degree scenarios.

While this estimate will clearly be wrong, the assumptions are chosen such that it is likely that this estimate is too conservative -- it could easily be the case that the actual cost-effectiveness is 10x or 100x of that estimate, while it seems significantly less likely that the estimate is too optimistic. Of course, not every project of CATF is as impactful as their work on 45Q, we should not take the conservative estimate of this work as the conservative guess of their work in general. Rather, it makes sense to assume that the average project is 10x less cost-effective than this project, resulting in a cost of USD 1/tCO2e as a conservative guess.

Why we think these calculations make sense

One should always take the precise cost effectiveness estimates with a grain of salt, they are illustrative, not meant to be precise estimates. While we try to think carefully about relevant factors and how to specify them, these estimates are clearly wrong.

Indeed, at first glance, such calculations seem implausible -- like a silly marketing pitch. Is it really plausible that top-charity interventions are 10-100x more cost-effective than offsets?

But it is easy to see from basic principles that offsets cannot be close to cost-effective because offsets are always about direct interventions, whereas the world as a whole is spending hundreds of billions on climate and this is spending that can be affected by advocacy. For offsets to be anywhere near the best advocacy charities, such as CATF and Carbon180, it would need to be true that there is almost nothing that can be done to improve societal resource allocation on climate.

This is deeply implausible because it is one of the most striking facts about the climate challenge that societal resource allocation is not optimal, leaving vast rooms for impact for charities that move the needle so that government budgets are spent more in line with global decarbonization priorities. Governments and the private sector are [under-investing in innovation](#) and are underinvesting unequally across technologies. We show this for philanthropy [here](#) but, ultimately more importantly, this imbalance also persists in government innovation budgets, as clearly evidenced by the recent [*Energizing America*](#) report (graph from page 7):

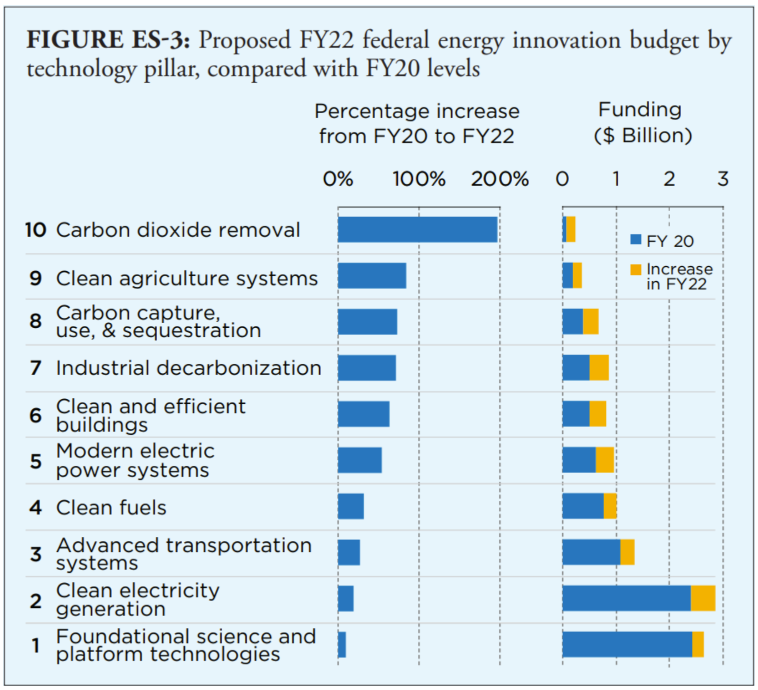


Figure 18: Top innovation priorities as per Energizing America report (p. 7 of that report).

Importantly, the top four innovation priorities -- where funding should increase the most relative to current levels -- all are key priorities of top-charities recommended by Founders Pledge (Carbon180 specializes on carbon removal, which is captured by "carbon dioxide removal" and "clean agriculture systems", whereas CATF specializes in carbon capture (3rd priority), industrial decarbonization and clean fuels).

Scenario % assigned to SSP/RCP Combinations

We integrate two pieces of "evidence" to form probabilities over the different SSP/RCP scenarios specified in the literature that are used in the Climate Philanthropy Prioritizer:

- (1) Compatibility with IEA 2005-2040 energy modeling projections, based on Pielke et al 2021
  - We divide a uniform prior by the average error of scenarios with existing and predicted trends (using data from Pielke et al 2021).
  - This leads to a ~7x differential in plausibility, e.g SSP2-45 becomes 7x more plausible than SSP1-19 based on this "update"

- (2) Given (1) heavily relies on extrapolating from current trends but we believe that there is a 10-30% chance of fundamental geopolitical changes (such as intense competition between great powers, etc.) making conditions quite different from today we are including a "worst worlds" scenario, where 80% of the probability mass are distributed uniformly and the remaining 20% are distributed across the higher three RCP scenarios of SSP 3-5, as those SSPs exemplify more challenging worlds for decarbonization. Given the implausibility of RCP 8.5, we only distribute half as much additional probability to SSP5-RCP8.5 than the other scenarios in this update.
  - The differential here is much weaker, less than 2x

- (3) We weight both updates 2x as strong as the uniform "prior".

- (4) Results by RCP below, this is consistent with Liu & Raftery (2021) and other assessments, we are slightly more pessimistic in line with our reasoning about extrapolating from current trends underestimating risks.

RCP	**%**	**Cum %**
RCP 1.9	9.48	9.48
RCP 2.6	10.76	20.24
RCP 3.4	18.33	38.57
RCP 4.5	23.67	62.24
RCP 6	23.07	85.31
> RPC6 & < RCP 8.5	11.66	96.97
RCP 8.5	3.03	100.00

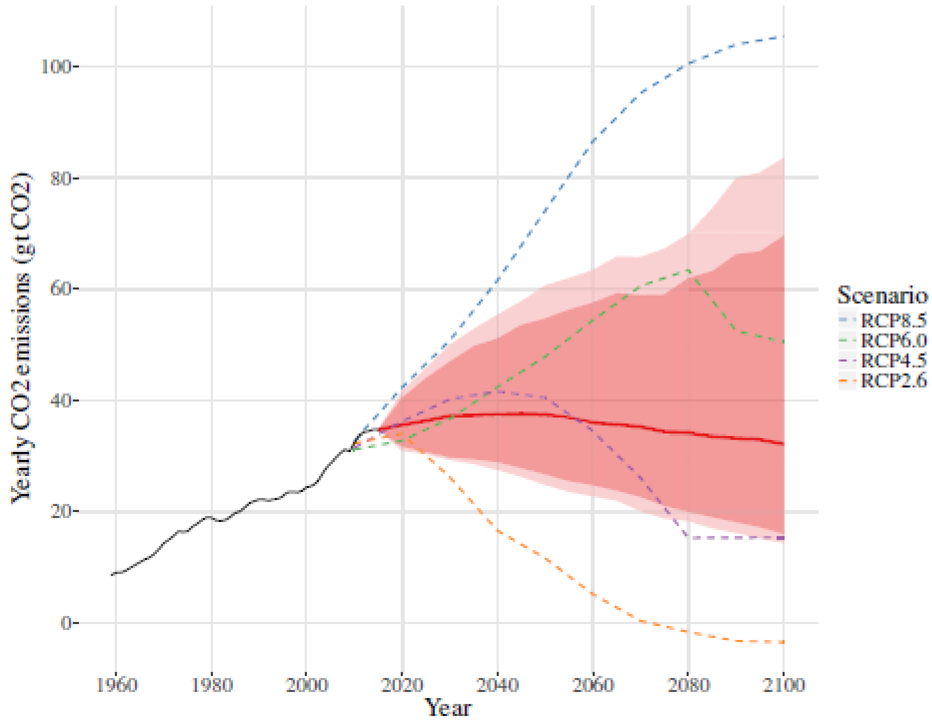


Fig. 1 Updated probabilistic forecast of CO_2 Emissions, based on data to 2015 and the method of Raftery et al.[1]. The forecast median of yearly global emissions in 2100 is now 34 Giga tons.

Figure 19: from Liu & Raftery (2021)

Scenario	%	Odds compared to average scenario	Plausibility change through scenario	Plausibility change through worst worlds
SSP1-19	2.30	0.64	0.31	0.80
SSP1-26	2.60	0.73	0.52	0.80
SSP1-34	3.04	0.85	0.82	0.80
SSP1-45	3.13	0.88	0.89	0.80
SSP1-60	3.42	0.96	1.09	0.80
SSP1-Baseline	2.98	0.84	0.79	0.80
SSP2-19	2.36	0.66	0.35	0.80
SSP2-26	2.84	0.80	0.69	0.80
SSP2-34	4.55	1.27	1.88	0.80
SSP2-45	4.86	1.36	2.10	0.80
SSP2-60	3.77	1.05	1.34	0.80
SSP2-Baseline	3.34	0.93	1.04	0.80
SSP3-34	3.63	1.02	1.24	0.80
SSP3-45	6.08	1.70	2.30	1.46
SSP3-60	4.11	1.15	0.92	1.46
SSP3-Baseline	3.78	1.06	0.69	1.46
SSP4-19	2.35	0.66	0.35	0.80
SSP4-26	2.67	0.75	0.57	0.80
SSP4-34	4.38	1.23	1.76	0.80
SSP4-45	5.89	1.65	2.16	1.46

SSP4-60	5.16	1.45	1.66	1.46
SSP4-Baseline	4.54	1.27	1.22	1.46
SSP5-19	2.47	0.69	0.43	0.80
SSP5-26	2.64	0.74	0.55	0.80
SSP5-34	2.74	0.77	0.62	0.80
SSP5-45	3.71	1.04	0.64	1.46
SSP5-60	3.63	1.02	0.58	1.46
SSP5-Baseline	3.03	0.85	0.49	1.13

Background on analysis of climate philanthropy data

What we did:

- analyse large grants made or announced by the Bezos Earth Fund and the Global Energy Alliance for People and Planet, categorize them according to the ClimateWorks philanthropy data (sector and region), add this data to the ClimateWorks data, include uncertainties

Step by step (how we made the sheets and the calculations):

1. Find out which large grants were made
2. Put data about Bezos Earth Fund and Global Energy Alliance for People and the Planet into this sheet: …. ("Programme name", "Amount [$M]" and "Description of programme")
3. If possible add the period over which the grant is planned to be dispersed ("Best guess period [years]"). If no information is available about that estimate the period (see estimation table). For estimated periods set "Min. period [years]" = 1 and "Max. period [years]" =10 (very conservative estimate)
4. Add the "Region" and the "Sector" for each grant (note: Funding by region is based on the geography of intervention, not the geography of the funder or recipient - ClimateWorks definition)
    a. For different regions of intervention: split up the grant (either by dividing it by the number of regions or a percentage estimate)
5. For every programme: calculate the "Best guess amount per year [$M]", "Min. amount per year [$M]" and "Max. amount per year [$M]" by diving the total "Amount [$M]" with the corresponding period of years

For the regions of intervention of the Global Energy Alliance for People and Planet:
"The alliance has identified an addressable market of 90+ countries, of which we plan to invest in 60+ that present the greatest impact across carbon, access, and jobs – as well as political vision, commitment, and leadership. The alliance will prioritize countries that meet one or more of the following criteria: (1) The country does not meet the Modern Energy Minimum threshold of 1,000kWh per person per annum; (2) The country has unreliable access to energy, meaning the country experiences grid outages and disruptions of more than 12 hours per month on average; (3) The country faces barriers to achieving universal access to electricity, and therefore less than 100% of the population is electrified; (4) The country is lower or lower-middle income and has a significant amount of existing and/or planned coal plants." (https://www.globalenergyalliance.org/)

Examples of programs: Kongo, Indonesia, India, Nigeria, Ethiopia

That is why we assumed that the money from the Global Energy Alliance for People and Planet is going to be split up between the regions Africa, Asia and Oceania, Indonesia and India. We used the ClimateWorks data on Clean Electricity in these regions as a prior. Then we estimated the minimum, best guess and maxium share of each region from the total ~500 $M spent per year from the Global Energy Alliance for People and Planet (2.5$B over 5 years).

Our estimates:

	ClimateWorks Clean Electricity [$M]	**Shares**	**Min**	**Best guess**	**Max**
Africa	19	0.4	0.2	0.4	0.48
India	23	0.5	0.2	0.4	0.6

Indonesia	0.2	0.004	0.004	0.05	0.1
Asia and Oceania	3	0.07	0.07	0.15	0.4

Estimation table

Total amount [$M]	**Best guess period [years]**
< 7	1
7-16	2
> 16	3
~ 40	3
~ 100	5
~ 1000	9

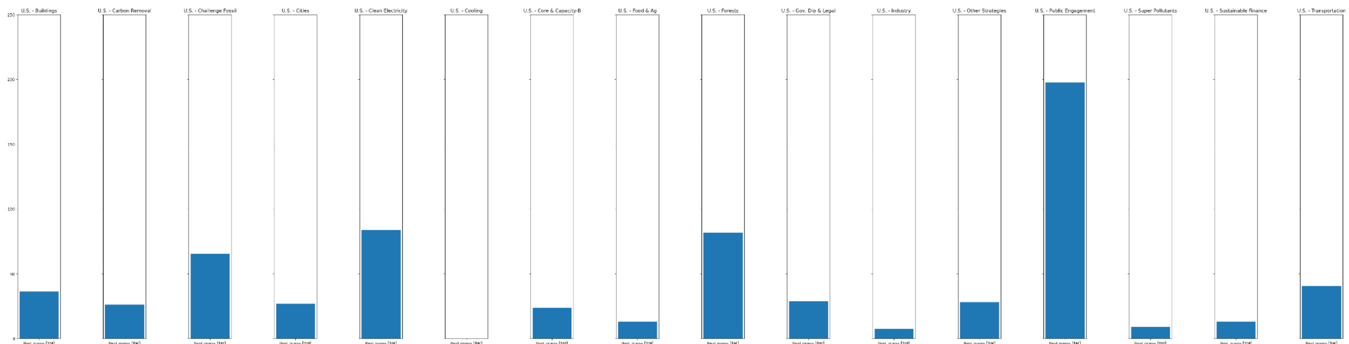
Uncertainties

- period
- regions
- sectors

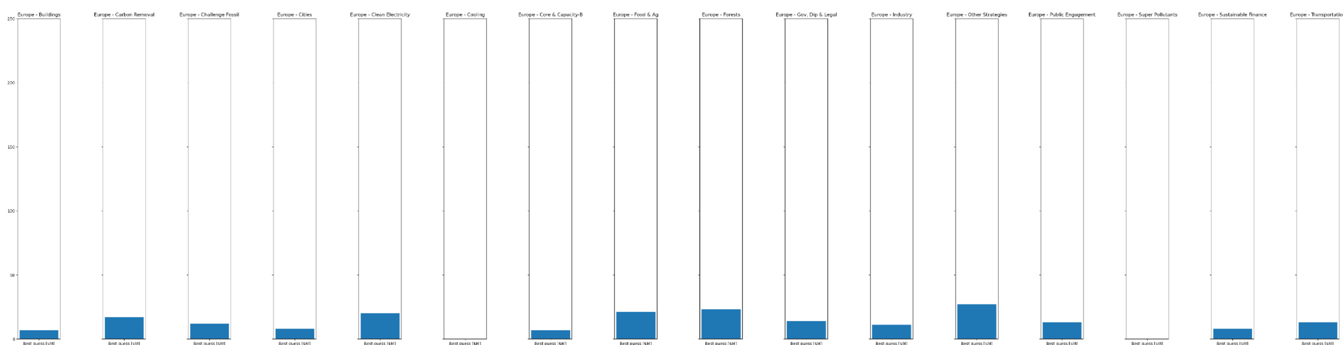
ClimateWorks and our best guess for allocation [in million USD]

Build-ings	Car-bon Re-moval	Chal-lenge Fossil	Cities	Clean Elec-tricity	Cool-ing	Core &Ca-pacity-B	Food & Ag	Fo-rests	Gov, Dip & Legal	In-dus-try	Other Strat-egies	Public En-gage-ment	Super Pollu-tants	Sus-tain-able Fi-nance	Tran-spor-tation

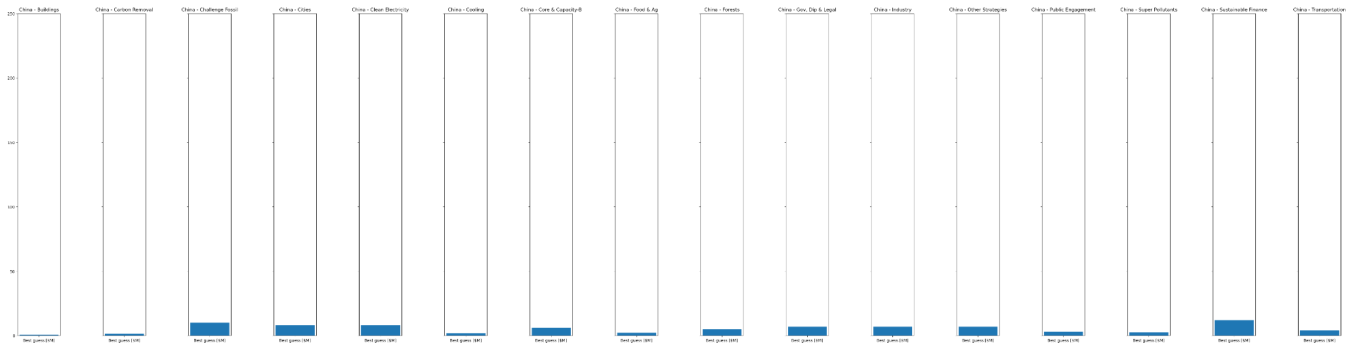
US:



Europe:

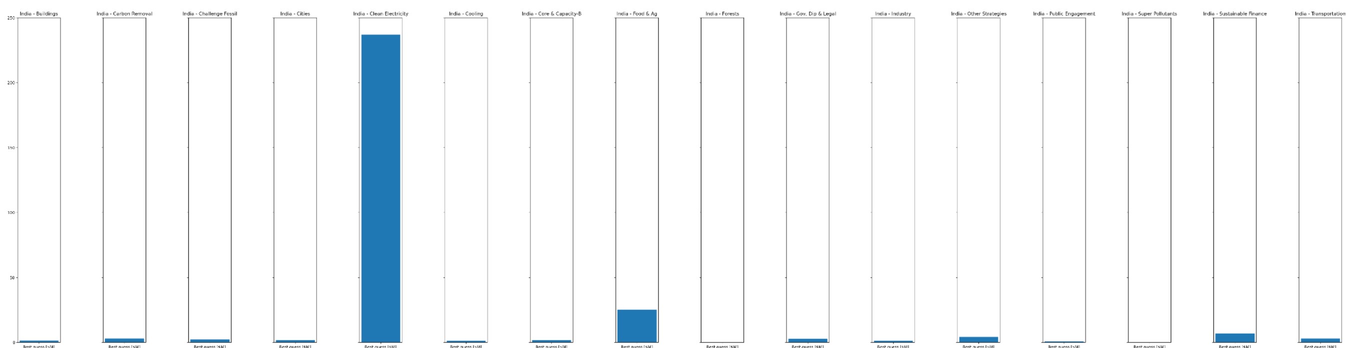


China:

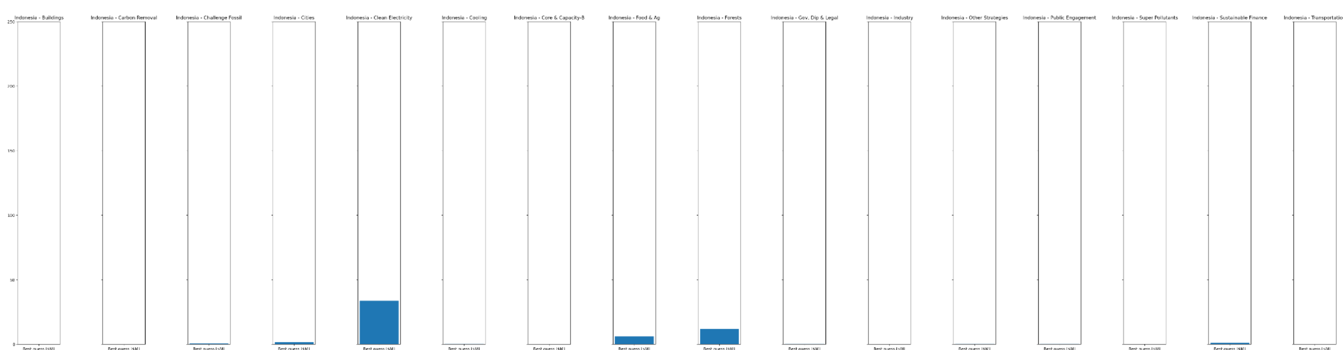


Build- ings	Car- bon Re- mova l	Chal- lenge Fossil	Citie s	Clean Elec- tricity	Cool - ing	Core &Ca- pacity -B	Food & Ag	Fo- rests	Gov, Dip & Legal	In- dus -try	Other Strat e-gies	Public En- gage- ment	Super Pollu- tants	Sus- tain- able Fi- nance	Tran- spor- tatio n

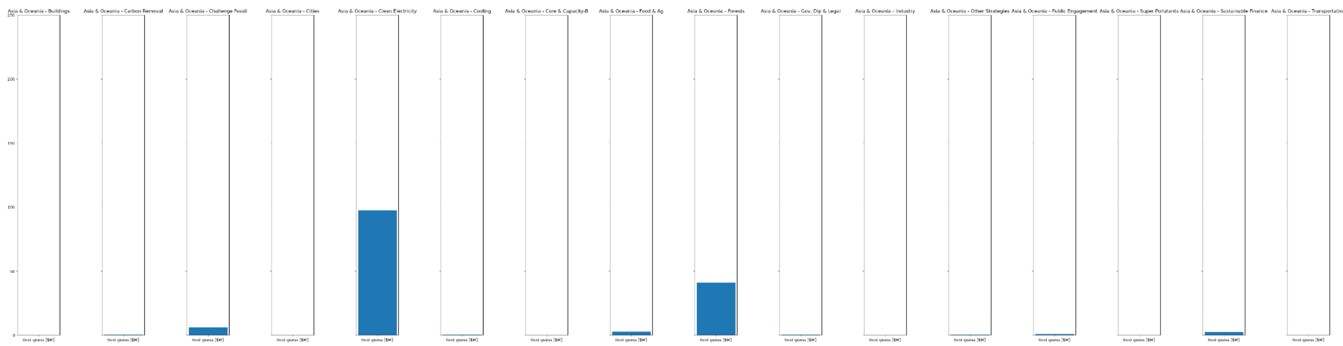
India:



Indonesia:

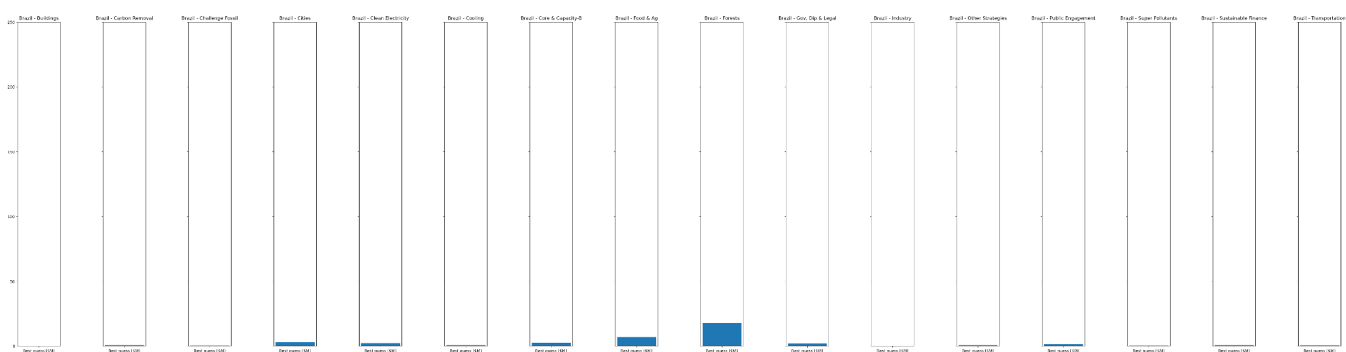


Asia & Oceania:

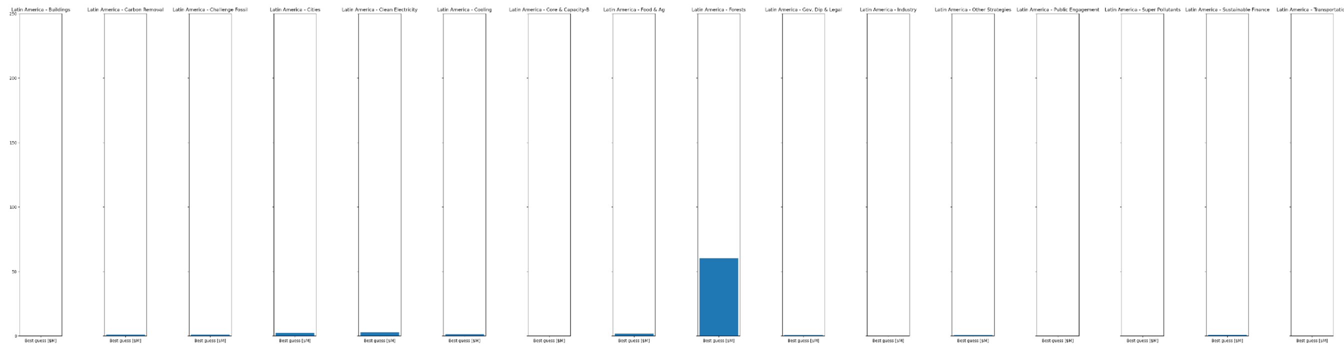


Build-ings	Car-bon Re-moval	Chal-lenge Fossil	Cities	Clean Elec-tricity	Cool-ing	Core &Ca-pacity-B	Food & Ag	Fo-rests	Gov, Dip & Legal	In-dus-try	Other Strat e-gies	Public En-gage-ment	Super Pollu-tants	Sus-tain-able Fi-nance	Tran-spor-tation

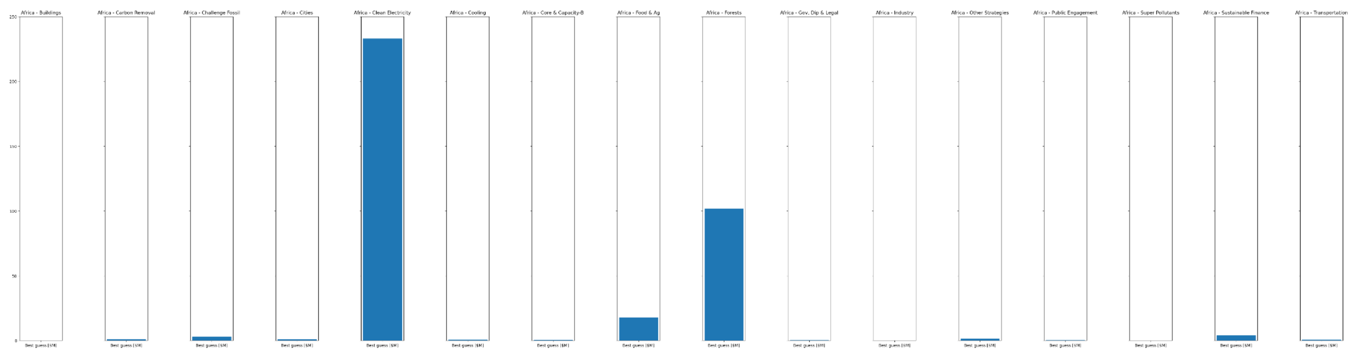
Brazil:



Latin America:

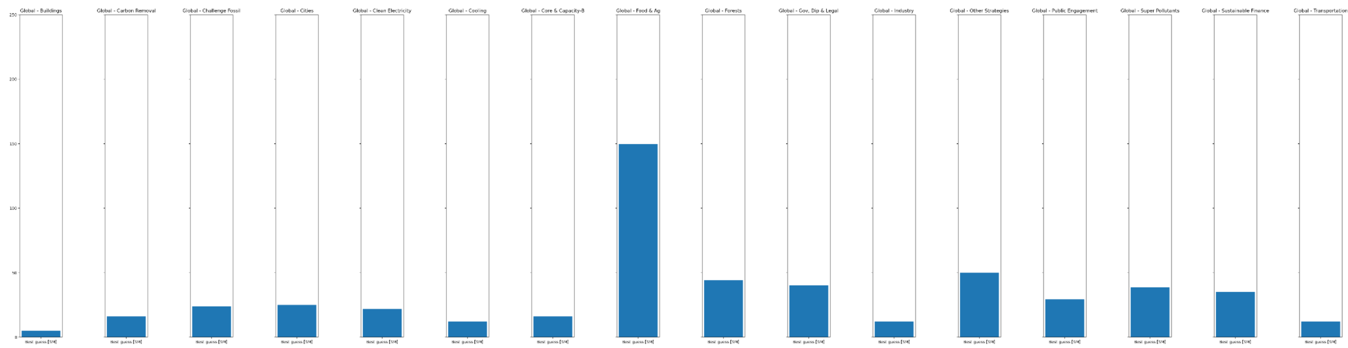


Africa:

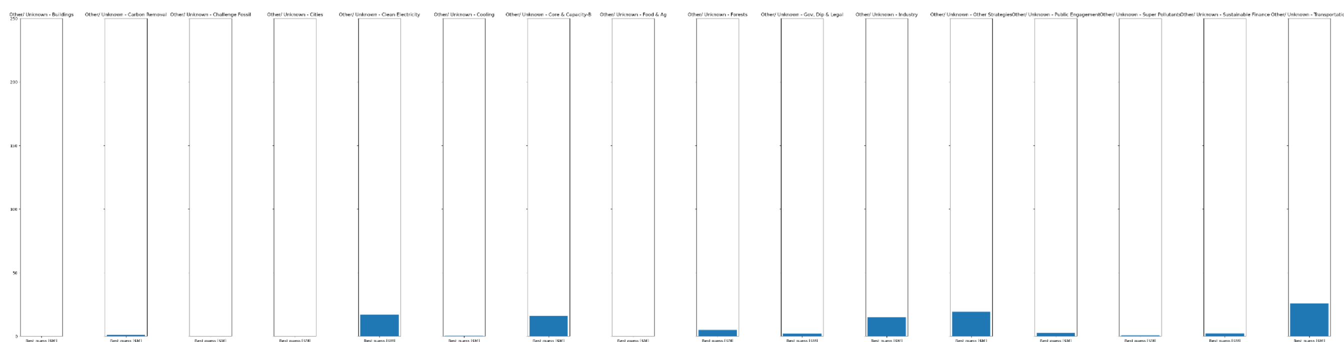


Build-ings	Car-bon Re-moval	Chal-lenge Fossil	Cities	Clean Elec-tricity	Cool-ing	Core &Ca-pacity-B	Food & Ag	Fo-rests	Gov, Dip & Legal	In-dus-try	Other Strat e-gies	Public En-gage-ment	Super Pollu-tants	Sus-tain-able Fi-nance	Tran-spor-tation

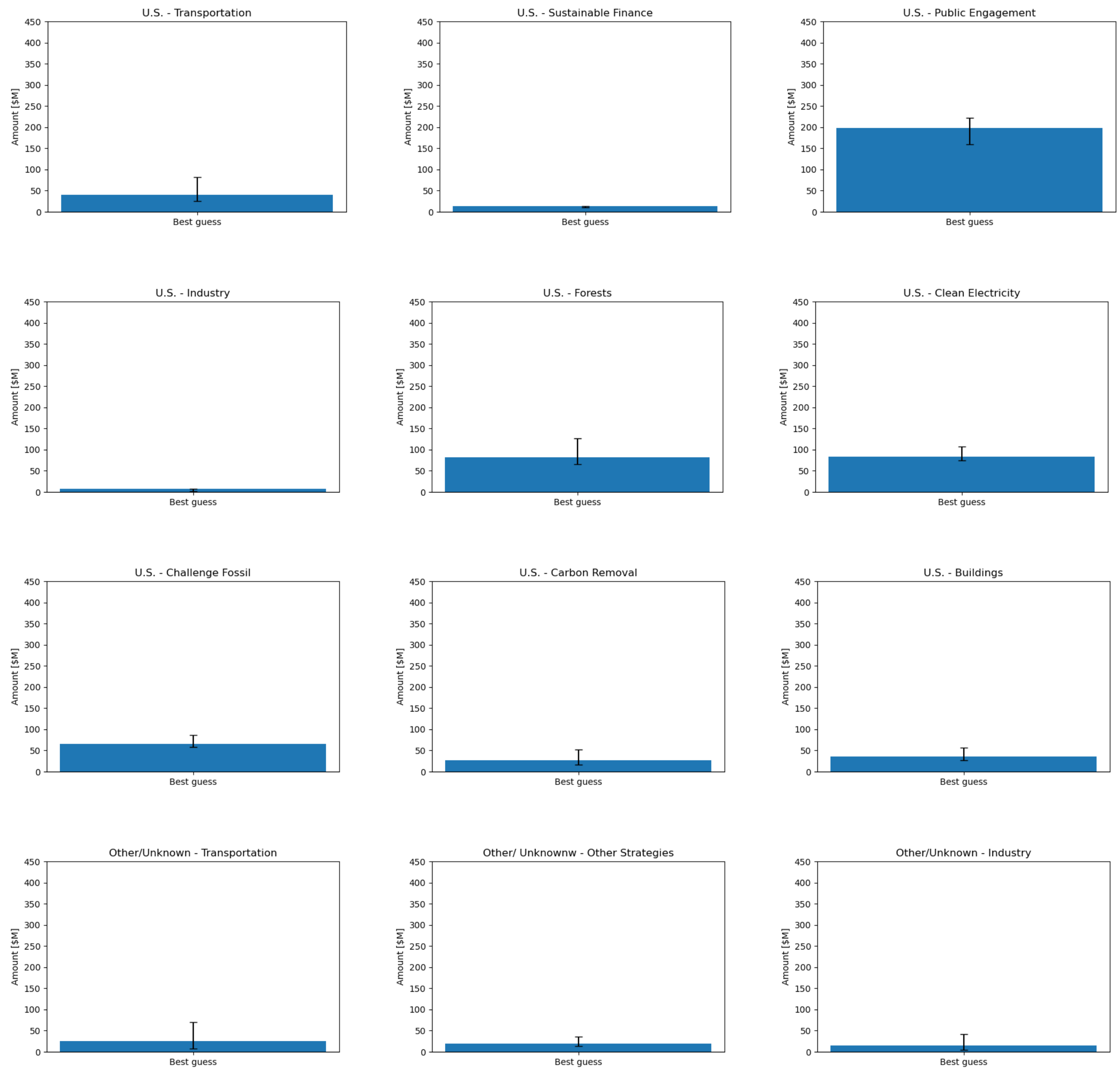
Global:

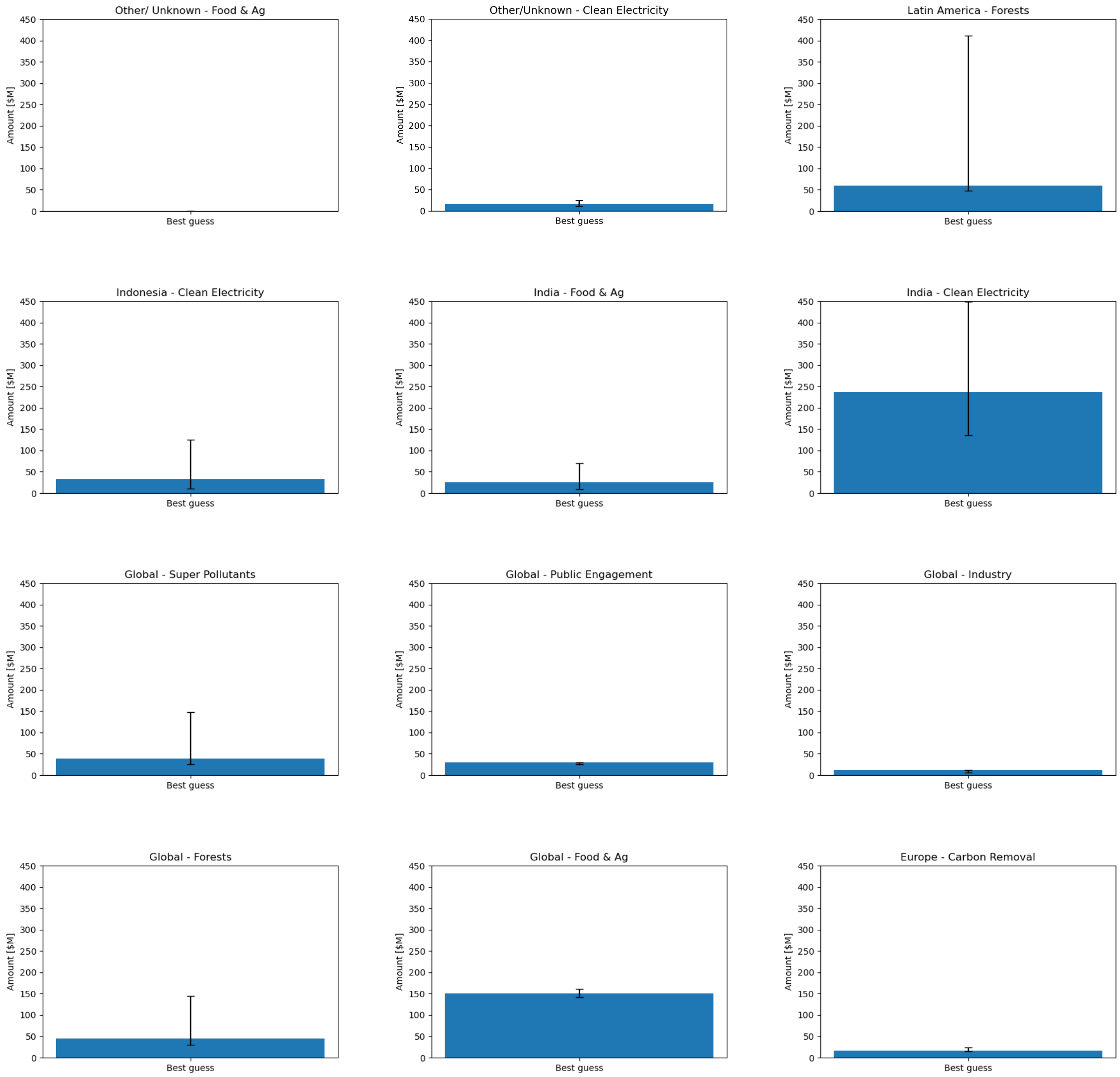


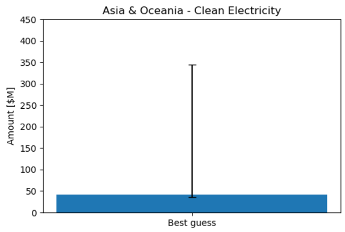
Other/ Unknown:

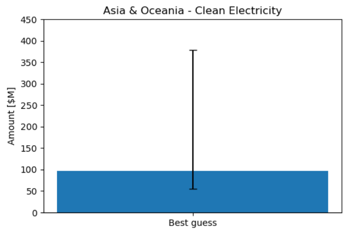


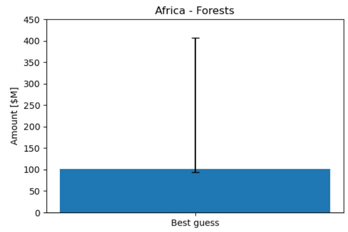
ClimateWorks and our best guess for allocation with error bars [in million USD]

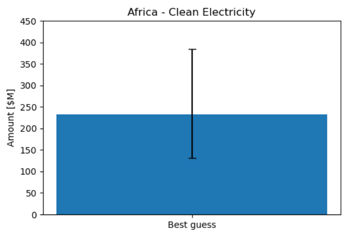



Other/ Unknown - Food & Ag
Other/Unknown - Clean Electricity
Latin America - Forests
Indonesia - Clean Electricity
India - Food & Ag
India - Clean Electricity
Global - Super Pollutants
Global - Public Engagement
Global - Industry
Global - Forests
Global - Food & Ag
Europe - Carbon Removal
Amount [$M]
0
50
100
150
200
250
300
350
400
450
Best guess


Asia & Oceania - Clean Electricity
Amount [$M]
Best guess


Asia & Oceania - Clean Electricity
Amount [$M]
Best guess


Africa - Forests
Amount [$M]
Best guess


Africa - Clean Electricity
Amount [$M]
Best guess

Best guess, min and max amount per year (sum of ClimateWorks and new data from Bezos Earth Fund and the Global Energy Alliance for People and Planet)

Sector	Region	Best guess [$M]	Min [$M]	Max [$M]
Buildings	Africa	0.1	0.1	0.1
Carbon Dioxide Removal	Africa	1.1	1.1	1.1
Challenge Fossil	Africa	2.9	2.9	2.9
Cities	Africa	1	1	1
Clean Electricity	Africa	232.8888889	131.5	384
Cooling	Africa	0.7	0.7	0.7
Core & Capacity-Building	Africa	0.4	0.4	0.4
Food & Agriculture	Africa	18	18	18
Forests	Africa	101.8725926	93.40688889	407.0688889
Governance, Diplomacy, & Legal	Africa	0.2	0.2	0.2
Industry	Africa	0	0	0
Other Climate Change Mitigation Strategies	Africa	1.3	1.3	1.3
Public Engagement	Africa	0.2	0.2	0.2

Super Pollutants	Africa	0	0	0
Sustainable Finance	Africa	4	4	4
Transportation	Africa	0.8	0.8	0.8
Buildings	Asia and Oceania	0	0	0
Carbon Dioxide Removal	Asia and Oceania	0.4	0.4	0.4
Challenge Fossil	Asia and Oceania	6	6	6
Cities	Asia and Oceania	0	0	0
Clean Electricity	Asia and Oceania	97.44444444	55.5	378
Cooling	Asia and Oceania	0.5	0.5	0.5
Core & Capacity-Building	Asia and Oceania	0	0	0
Food & Agriculture	Asia and Oceania	2.6	2.6	2.6
Forests	Asia and Oceania	41.03703704	35.09333333	343.7333333

Governance, Diplomacy, & Legal	Asia and Oceania	0.5	0.5	0.5
Industry	Asia and Oceania	0	0	0
Other Climate Change Mitigation Strategies	Asia and Oceania	0.6	0.6	0.6
Public Engagement	Asia and Oceania	0.7	0.7	0.7
Super Pollutants	Asia and Oceania	0	0	0
Sustainable Finance	Asia and Oceania	2.5	2.5	2.5
Transportation	Asia and Oceania	0	0	0
Buildings	Brazil	0.1	0.1	0.1
Carbon Dioxide Removal	Brazil	0.7	0.7	0.7
Challenge Fossil	Brazil	0.2	0.2	0.2
Cities	Brazil	2.9	2.9	2.9
Clean Electricity	Brazil	2.2	2.2	2.2
Cooling	Brazil	0.7	0.7	0.7

Core & Capacity-Building	Brazil	2.4	2.4	2.4
Food & Agriculture	Brazil	7	7	7
Forests	Brazil	18	18	18
Governance, Diplomacy, & Legal	Brazil	2.1	2.1	2.1
Industry	Brazil	0	0	0
Other Climate Change Mitigation Strategies	Brazil	0.7	0.7	0.7
Public Engagement	Brazil	1.3	1.3	1.3
Super Pollutants	Brazil	0.3	0.3	0.3
Sustainable Finance	Brazil	0.8	0.8	0.8
Transportation	Brazil	0.6	0.6	0.6
Buildings	China	0.7	0.7	0.7
Carbon Dioxide Removal	China	1.3	1.3	1.3
Challenge Fossil	China	10	10	10
Cities	China	8	8	8
Clean Electricity	China	8	8	8
Cooling	China	1.9	1.9	1.9

Core & Capacity-Building	China	6	6	6
Food & Agriculture	China	2.2	2.2	2.2
Forests	China	5	5	5
Governance, Diplomacy, & Legal	China	7	7	7
Industry	China	7	7	7
Other Climate Change Mitigation Strategies	China	7	7	7
Public Engagement	China	2.9	2.9	2.9
Super Pollutants	China	2.5	2.5	2.5
Sustainable Finance	China	12	12	12
Transportation	China	4	4	4
Buildings	Europe	7	7	7
Carbon Dioxide Removal	Europe	17.125	14.9375	23.375
Challenge Fossil	Europe	12	12	12
Cities	Europe	8	8	8
Clean Electricity	Europe	20	20	20
Cooling	Europe	0	0	0

Core & Capacity-Building	Europe	7	7	7
Food & Agriculture	Europe	21	21	21
Forests	Europe	23	23	23
Governance, Diplomacy, & Legal	Europe	14	14	14
Industry	Europe	11	11	11
Other Climate Change Mitigation Strategies	Europe	27	27	27
Public Engagement	Europe	13	13	13
Super Pollutants	Europe	0.3	0.3	0.3
Sustainable Finance	Europe	8	8	8
Transportation	Europe	13	13	13
Buildings	Global	5	5	5
Carbon Dioxide Removal	Global	16	16	16
Challenge Fossil	Global	24	24	24
Cities	Global	25	25	25
Clean Electricity	Global	22	22	22
Cooling	Global	12	12	12

Core & Capacity-Building	Global	16	16	16
Food & Agriculture	Global	149.7611111	141.2411111	160.4111111
Forests	Global	43.95	29.695	143.95
Governance, Diplomacy, & Legal	Global	40	40	40
Industry	Global	12.25	6.625	12.25
Other Climate Change Mitigation Strategies	Global	50	50	50
Public Engagement	Global	29.2	25.42	29.2
Super Pollutants	Global	38.4	24.7	148
Sustainable Finance	Global	35	35	35
Transportation	Global	12	12	12
Buildings	India	1.4	1.4	1.4
Carbon Dioxide Removal	India	3	3	3
Challenge Fossil	India	2.2	2.2	2.2
Cities	India	1.5	1.5	1.5
Clean Electricity	India	236.8888889	135.5	448
Cooling	India	1.2	1.2	1.2

Core & Capacity-Building	India	1.5	1.5	1.5
Food & Agriculture	India	25.22	9.666	69.66
Forests	India	0.1	0.1	0.1
Governance, Diplomacy, & Legal	India	2.6	2.6	2.6
Industry	India	1.2	1.2	1.2
Other Climate Change Mitigation Strategies	India	4	4	4
Public Engagement	India	0.7	0.7	0.7
Super Pollutants	India	0.3	0.3	0.3
Sustainable Finance	India	7	7	7
Transportation	India	2.9	2.9	2.9
Buildings	Indonesia	0	0	0
Carbon Dioxide Removal	Indonesia	0	0	0
Challenge Fossil	Indonesia	0.6	0.6	0.6
Cities	Indonesia	1.5	1.5	1.5
Clean Electricity	Indonesia	33.53333333	9.7	125.2
Cooling	Indonesia	0.2	0.2	0.2

Core & Capacity-Building	Indonesia	0	0	0
Food & Agriculture	Indonesia	6	6	6
Forests	Indonesia	12	12	12
Governance, Diplomacy, & Legal	Indonesia	0.3	0.3	0.3
Industry	Indonesia	0.1	0.1	0.1
Other Climate Change Mitigation Strategies	Indonesia	0.2	0.2	0.2
Public Engagement	Indonesia	0.3	0.3	0.3
Super Pollutants	Indonesia	0	0	0
Sustainable Finance	Indonesia	1.2	1.2	1.2
Transportation	Indonesia	0	0	0
Buildings	Latin America	0	0	0
Carbon Dioxide Removal	Latin America	1	1	1
Challenge Fossil	Latin America	1	1	1
Cities	Latin America	2.2	2.2	2.2
Clean Electricity	Latin America	2.6	2.6	2.6
Cooling	Latin America	1.4	1.4	1.4

Core & Capacity-Building	Latin America	0.2	0.2	0.2
Food & Agriculture	Latin America	1.9	1.9	1.9
Forests	Latin America	60.34703704	48.34833333	411.4833333
Governance, Diplomacy, & Legal	Latin America	0.4	0.4	0.4
Industry	Latin America	0	0	0
Other Climate Change Mitigation Strategies	Latin America	0.4	0.4	0.4
Public Engagement	Latin America	0.1	0.1	0.1
Super Pollutants	Latin America	0.2	0.2	0.2
Sustainable Finance	Latin America	0.8	0.8	0.8
Transportation	Latin America	0.2	0.2	0.2
Buildings	Other/ Unknown	0	0	0
Carbon Dioxide Removal	Other/ Unknown	0.9	0.9	0.9
Challenge Fossil	Other/ Unknown	0	0	0
Cities	Other/ Unknown	0	0	0

Clean Electricity	Other/ Unknown	17	10.6	25
Cooling	Other/ Unknown	0.2	0.2	0.2
Core & Capacity-Building	Other/ Unknown	16	16	16
Food & Agriculture	Other/ Unknown	0.07	0.007	0.07
Forests	Other/ Unknown	5	5	5
Governance, Diplomacy, & Legal	Other/ Unknown	2.1	2.1	2.1
Industry	Other/ Unknown	15.16666667	4.1	41
Other Climate Change Mitigation Strategies	Other/ Unknown	19.33333333	13.5	36
Public Engagement	Other/ Unknown	2.4	2.4	2.4
Super Pollutants	Other/ Unknown	0.7	0.7	0.7
Sustainable Finance	Other/ Unknown	2.2	2.2	2.2

Transportation	Other/ Unknown	25.85	7.55	70.1
Buildings	U.S.	36.33333333	27.3	57
Carbon Dioxide Removal	U.S.	26.125	16.9375	52.375
Challenge Fossil	U.S.	65.41666667	58.125	86.25
Cities	U.S.	27	27	27
Clean Electricity	U.S.	83.75	73.75	107.5
Cooling	U.S.	0.1	0.1	0.1
Core & Capacity-Building	U.S.	24	24	24
Food & Agriculture	U.S.	13	13	13
Forests	U.S.	81.77555556	66.22155556	126.2155556
Governance, Diplomacy, & Legal	U.S.	29	29	29
Industry	U.S.	7.55	1.925	7.55
Other Climate Change Mitigation Strategies	U.S.	28	28	28
Public Engagement	U.S.	197.5666667	159.89	221.9
Super Pollutants	U.S.	9	9	9

Sustainable Finance	U.S.	13	10.3	13
Transportation	U.S.	40.58333333	26	82.25