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A Call for Volunteers to Confront What We Do Not Know About Tipping Points – The Next Biggest Challenge for Climate Science

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Abstract
Climate scientists, especially those with earth and environmental experience, have been extremely successful in expanding the scope of detection and attribution (D&A) analyses of "know uncertainties) about climate variables that include much more than increases in temperature and some changes in precipitation patterns. They have been favored in those endeavores by some well supported and widely accepted laws of physics with which to anchor their models. Today in the first quarter of 2024, though, they cannot rest on their laurels. A new more difficult task awaits their collective and integrated attention. They need to collectively confront the prospects of large and dangerous tipping points about which, to be honest, we know very little. Our understanding of how, when, and where those thresholds lie and why they even exist is woefully inadequate to support the clamor in many locations for near-term abating and adapting actions supported by enough knowledge to make society confident that their proposed actions will actually work. We need another large concerted research effort, perhaps mimicking their D&A work but perhaps not, designed to advance knowledge in the same was as before in less time than before without accepted fundamental laws of physics as before; they are now confronting "known unknowns" without clear quantification of the inherent uncertainties. This effort will confront two profound budget constraints – limited and vulnerable funding to cover the expense without interruption and a large influx of new and motivated science talent to fill the human capital gap (the current crop of scientists already have too much to do with too little funding). Fundamentally, this perspective piece is a plea designed to motivate the next cohort of climate scientists to pick up the mantle with the promise of support, colleagueship, respect, recognition, mentoring, and life-long friendships.

Keywords: adaptation; Antarctic ice sheet disintegration and/or collapse; attracting new physical and social scientific talent; climate change; climate risk; established and missing physics; known uncertainties; known unknowns; mitigation; new research funding; sea level rise; tipping points

Introduction

I write informed by years of exploration of and weekly zoom conversation about tipping points (TPs) in the climate system with long term colleagues and collaborators: Richard Alley and Benjamin Santer, two of the world's experts on ice sheet dynamics and revolutionary "fingerprinting" technics as well as Henry Jacoby and Richard Richels who join me with considerable expertise in approaching climate change impacts from an iterative risk management perspective. Collectively, we have more than 130 years of engaged experience working together on, among other things, primary and inventive research that lead us to contribute widely to the periodic assessments published by the Intergovernmental Panel on Climate Change (IPCC). We have been working together most recently to crystalize our thoughts about tipping points (TPs) so that we can envision how the climate science community can best respond. We all see our primary task to be rigorously and accessibly informing the downstream work decision-makers who are tasked with framing what to do in response to what are now barely imaginable future risks.

We have been struck by two observations: the climate is changing faster than the science and the science is already way behind the "eight ball". We all entered this project with the expectation that we could argue convincingly that the value of new information about TPs that would inform new coupled and forward-looking modeling would be high, and we have found no reason to doubt that position over the last two years – except for one missing adjective. The value of that information will likely be very high; indeed, even well-informed early efforts should produce benefits that will far exceed the cost of designing and implementing what we see an essential and brand new globally integrated and multidisciplinary research program. We have a vision that needs significant and secure new funding over the foreseeable future and *a large infusion of new and established scientists* who can pick up the mantle of a new global research initiative – thus this call for volunteers.

Motivation

As we thought about what information would be most valuable in this regard, it became clear that our current knowledge base, especially for the critical coupled dynamics of coupled climate systems in understudied regions like the polar regions of the southern hemisphere, is woefully inadequate for the task at hand. That was a bit of a surprise because our detection and attribution (D&A) work was finally progressing so nicely. The conservative finding in IPCC (1995) that "the balance of evidence suggests that there is a discernible human influence on global climate" had been correctly criticized in real time because it was based solely on time series of global mean temperatures and atmospheric concentrations of carbon dioxide. Surely, the critics argued, there must be many other important climate change indicators whose hypothesized correlation with warming could either weaken or strengthen confidence in the most fundamental conclusion of the entire enterprise: the climate is changing inmany ways and humans are largely to blame. The science community responded to this challenge by organizing, expanding and improving its fingerprinting techniques and applying them a wide range of other climate variables like precipitation, humidity and vertical location-specific temperature columns into the atmosphere, and the like. These were suspected correlations based on a few widely accepted and rigorous understandings of some of the fundamental laws of physics that told us where to look and what to expect to find through game-changing research that began to emerge in the 1960's and 1970s with the seminal work of two 2021 Nobel Prize (2024) laureates in physics: Syukuro Manabe (1965) and Klaus Hasselmann (1976).

Their fingerprinting approach to D&A issues matured early in this century. It involved comparison exercises across ensembles populated by models that were different in their details but consistent in their inclusion of basic physics. In the Coupled Model Inter-comparison Project (CMIP) working within the World Climate Research Programme (WCRP-CMIP, 2024), for example, more than 45 modelers who collectively had produced more than 100 distinct models agreed to run their models to explore the sensitivity of their results to different initial conditions. They ran their models with the same driving inputs so that their results would allow the community to explore systematically the implications of known uncertainties and thereby detect attributable fingerprints of climate change forcing. These experiments allowed the community to explore the models' relative "fitness for purpose" across a wide spectrum of important climatic contexts.

Method – A Case Study on Sea Level Rise and the Dynamics of the West Antarctic Ice Sheet

The D&A work was successful in producing a growing the list of increasingly well-understood and well-documented climate impacts, but we are more modest in the foci of our current work on TPs. We have focused our attention on the potential of exaggerated global sea level rise (SLR) that should, were it to occur soon or sometime in the uncertain future, be attributable to the potential disintegration of some of the most studied and largest ice sheets in Antarctica. Accelerated melting of the Greenland Ice Sheet has not escaped our attention in our case study, of course, but SLR impacts around the globe are our primary reasons for concern. Their direct connection to ice sheet dynamics is widely accepted if not adequately calibrated and modeled. We expect that it may ultimately be quantifiable across a wide range of possible futures including moving into the growing dark tails of enormous risk, but we know for sure that protection planning and enormously expensive implementation of protection projects is already underway across the United States and elsewhere. Recent work by the United States Army Corps of Engineers (USACE, 2023) at the request of cities like New York, San Francisco, and Houston are perfect examples of this fact born of recognition that rising seas will create intolerable risk to property and lives.

Despite this myopia, we think that our insights will be applicable to other potential tipping points: the collapse of the Atlantic Meridional Overturning Circulation (AMOC) that is also influenced by melting glaciers in Greenland, the runaway greenhouse warming from the melting of the methane-laden northern latitude tundra, the rapid transformation of the Amazon rainforest resulting in new regional climates that threaten one of the world's biggest carbon sink, worldwide coral bleaching and death, extensive and connected wildfires across the country – disasters that are coming attractions up and down the eastern mountain ranges of the United States, and so on.

Here I support some of our conclusions with evidence drawn from the case study in language that we think applies to a large degree to all of these potential threats – and thus all of the issues that could be investigated only if the climate science community is up to another more dauting task. Coordinated science has done well on D&A over the past few decades, but TPs are not "known uncertainties" of the sort confronted there. They are "known unknowns" for which even the requisite understanding of the basic physics has yet to be achieved and accepted.

The Inadequacy of Current Science for Understanding TPs

The climate science community can call TPs by name, but we do not know when we will cross their respective thresholds because we do not understand their dynamics across a complicated, multifaceted, and coupled climate systems. We do not know very much about their triggering mechanisms, but we do know that we sometimes cannot even be confident in the sign of important changes driven my multiple drivers across multiple interconnected systems. We sometimes do not even know what questions are the most important to ask. We are searching in the dark to identify what data will be most valuable to collect and maintain. We are driving blind in any efforts to couple models of the various interacting climate systems, and so we are incapable of examining the their resulting "fitness for (the next) purpose" – in this case, to project credible futures in which massive change can occur suddenly and irreversibly, to explain why those futures might materialize, and to divine when they might become an enormous problem with regard to impacts on humanity in specific and the planet more generally. That, of course, leads to the ultimate objective: design and implement plans about what to do about it, *now or at least as soon as scientifically possible.*

The State of Play in 2024

All is not completely lost, though. To argue why, I accept John Holdren's summary of our response options as reported in Yohe, et al. (2023) – abate (mitigate), adapt, or suffer the consequences.

Beginning at the macro scale of climate action, the value in informing decisions about mitigation lies in its potential to reduce the likelihood of extreme events like crossing tipping points' activating thresholds, or *at least delaying those crossings* to give adaptors more time to do the right things. This speaks directly to hedging against large consequence futures by nations' meeting their published short-term mitigation short-term goals under the United Nations Framework Convention on Climate Change (UNFCCC, 1992) and by planning efficient adjustments to speed up progress toward achieving a net-zero greenhouse gas emissions future sooner rather than later. "Pay now, or pay much more in the future" should become the mantra for emissions abatement.

The value of information about adaption depends on these emissions futures because the impacts of a warming planet depend irreversibly (at least over centuries) upon atmospheric concentrations of heat-trapping gases (Solomon, et al., 2009). In the case of sea level rise (SLR) as reported in USACE (2023), the Corps is working with three future projections: 6 inches, 2 feet, and 6 feet of addition sea levels through 2100. Despite the highest possibility, the adaptation plans that they produced focus nearly all of planners' attention on thoroughly costing-out recommendations designed to cope most inexpensively to the middle scenario.

That is an unsettling basket for most of your action item eggs when the real action could happen in the most dangerous upper tails. Figure 1 from Mulhern (2020), for example, displays a map that depicts the eventual vulnerability of New York City along low (roughly 2 feet by 2100) and high (roughly 6 feet by 2100) SLR futures. The dark blue regions would be vulnerable along the higher scenario even if the city protects itself against flooding into the light blue areas; in fact, those regions would need also some extra protection sometime in the future if the more extreme SLR future were to emerge – a mid-course correction for which the city should be prepared.

It follows that the consumers of our science must come to realize that ignoring the not-implausibility of highest SLR scenarios and the extremes of the damages that they portend means that protection recommendations produced by the USACE could be inadequate, at best, and misplaced at worse; i.e., implementing their plans could actually make the suffering worse in terms of protecting human life and property. The USACE do not even include simple hedging strategies like building protection infrastructures that can be augmented easily if need be or producing contingent "on the shelf" responses for locations whose risk did not make the "take action" cut in 2023.

These observations identify components of at least one specific and hopefully self-contained but nonetheless important research topic – describe rigorously and clearly what would be required to determine in advance which of currently projected SLR extremes is more likely. USACE typically includes 6 feet SLR futures for 2100 as a worst-case reference point for protection analyses because the Intergovernmental Panel on Climate Change (IPCC) reported something very close to in its Sixth Assessment Report (IPCC-AR6, 2022). Focusing attention here on the second panel of Figure 2 which replicates a summary visual of their assessment (their Figure 3 in Cross-Chapter Box SLR) of the then current science supports this point.

In Panel (b), IPCC authors provide an effective visual of distributions of SLR estimates for four different socioeconomic cum carbon forcing trajectories as well as distributions for 2150. More specifically, they show the medians for each projected future with dark colored lines that are surrounded by shaded cones that span the denote 5th through 95th percentile estimates. Notice that, through 2100, the cones together capture the two lowest USACE planning baselines. Notice as well that the authors labelled two more aggressive scenarios as products of "low confidence processes" and identified them as 83rd and 95th percentile futures. These reflect the USACE 6 foot extreme scenario in 2100, and they are the reason why the Corps labelled them "unlikely" in their work so that they could essentially ignore them.

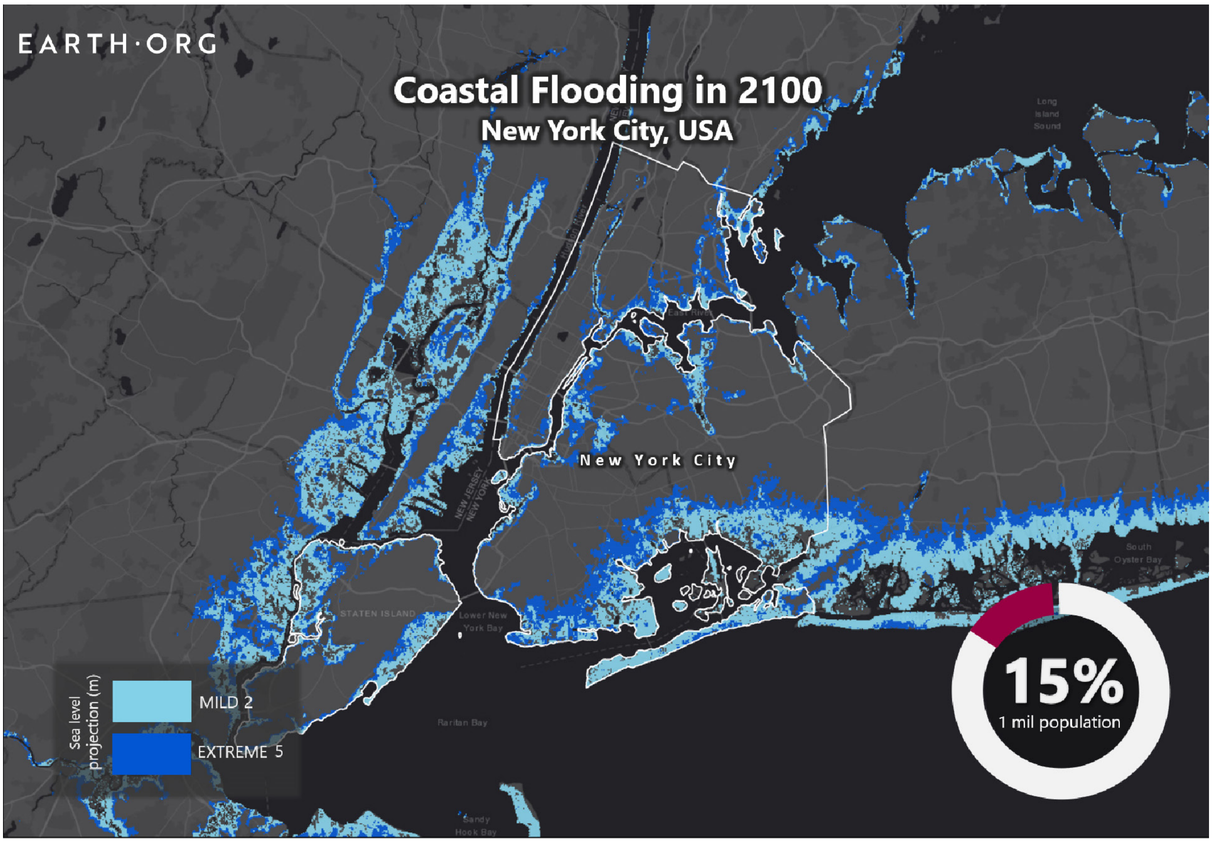

Figure 1: Coastal flooding across New York City in 2100.
The light blue shading shows locations that would be vulnerable to SLR induced flooding at some point in time along scenario that reaches 2 feet by 2100. The dark blue shading adds property that would be added if the much more extreme 6 feet scenario were to materialize.
Source: https://earth.org/data_visualization/sea-level-rise-by-the-end-of-the-century-new-york-city/

The trouble with these judgements is that neither the IPCC authors nor the USACE really had any basis for assessing anything informative about the likelihoods above the 6 feet SLR extremes. It is bad form in IPCC-world to mix confidence language (IPCC, 2018) with likelihood assessments but we should have extremely low confidence in the statement that 6 feet is unlikely because process understanding of how that might materialize is current absent (except the easily criticized coupling a high forcing scenario with a dysfunctional world economic and development order (Riahi, et al., 2024). Perhaps more importantly, there is nothing in what the IPCC used to draw its second panel that makes any reference to what is going on in Antarctica because, in the global models that produced its content, the entire continent was portrayed as a stable white mountain for the next 130 years. That is not their fault, of course, because IPCC assesses recently published science and is proscribed by its charter from producing new science. The fault lies in reporting something that cannot be supported by existing science. If the truth had been presented, IPCC authors would have reported that there are simply no credible models of the southern oceans coupled with dynamic Antarctic ice sheets from which to draw any conclusion except that something dramatic and unexpected could happen and we do not know when.

This does not mean that coastal governments should not plan large adaptation projects that will cost tens of billions of dollars to build as a matter of course starting as soon as possible. Instead, it means that planners must include least cost augmentation options for mid-course corrections (a least cost hedge) and practitioners must demand new and improved science that enlightens their understanding about what is happening so they understand why they should keep a wary eye on remote locations around the world were all of the action is beginning to occur.

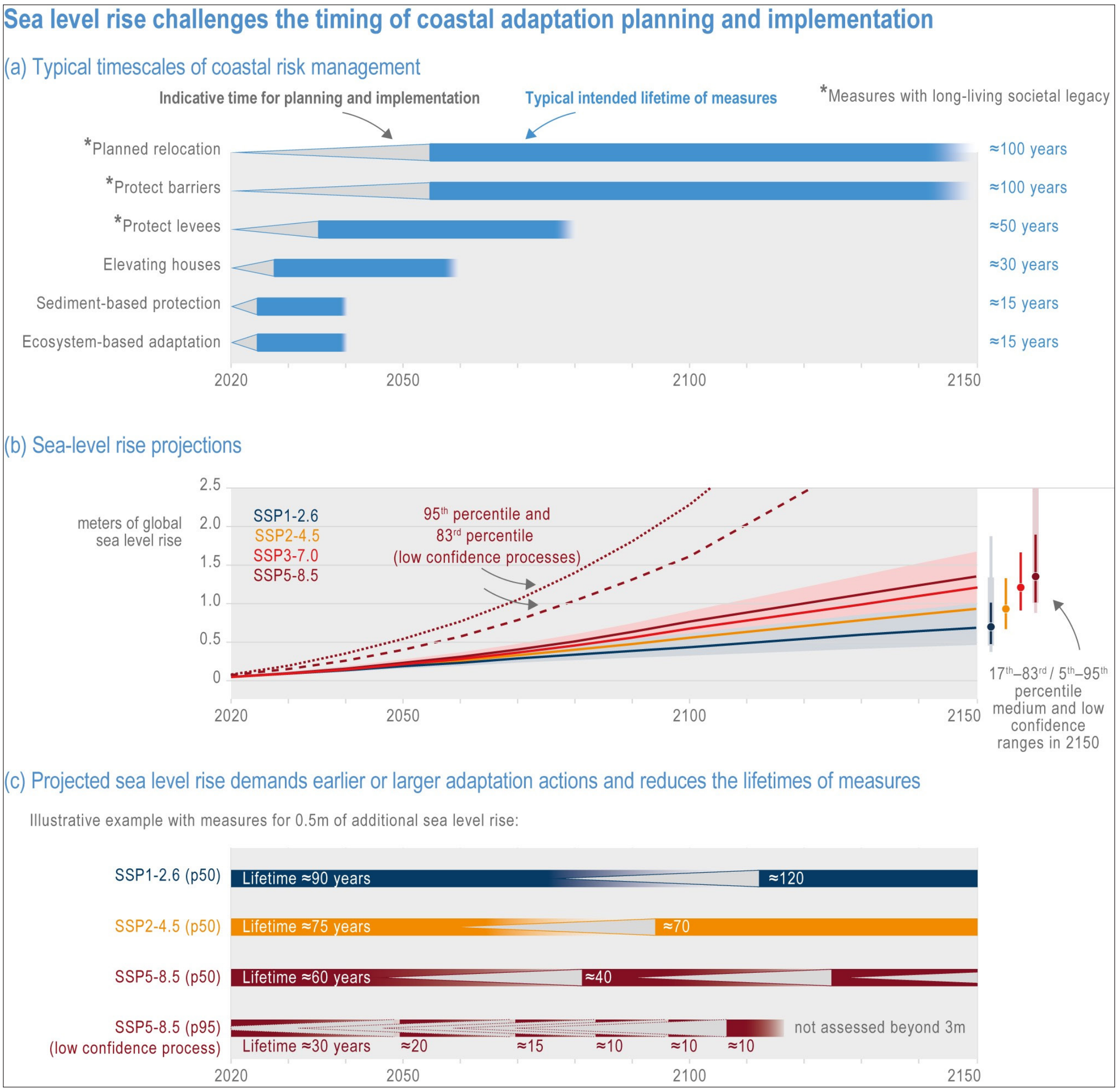

Figure 2: Sea level rise challenges the timing of planning and implementing coastal adaptation.
Panel (a) reports anticipated timescales of coastal risk management investments; it includes building protective barriers and highlights protecting existing levies.

Panel (b) depicts alternative SLR pathways (medians and 5th – 95th percentile ranges for four alternative socioeconomic and forcing combinations; it includes low confidence scenarios without underlying assumptions and without additional contributions from Antarctic ice sheets.

Panel (c) makes the point that the character of effective adaptation is sensitive to the speed of SLR – a faster pace favors shorter projects or at least cost-effective mid-course augmentation strategies.
Source: https://www.ipcc.ch/report/ar6/wg2/downloads/figures/IPCC_AR6_WGII_Figure_3_Cross-Chapter_Box_SLR_1.png

Characteristics of an Effective Research Program Moving Forward From 2024

Thinking about the sensitivity of making appropriate adaptation decisions more broadly in real time makes it clear that future research must take account of boundary conditions that are firmly anchored on what has been happening globally within the climate system rit-large over the past few years and how those conditions are captured in modelling components. The data from 2023 and into 2024 are particularly striking in this regard. We are currently passing the 1.5 degree Celsius benchmark, so what has thus far been an aspirational temperature target for global mitigation efforts is rapidly becoming a disappearing object in our rear-view mirror. Figure 3 from Copernicus (2024) makes this point visually by tracing with daily ocean temperature over the past half century or so; 2023 stands out as a statistically significant very sore thumb month after month, and 2024 is starting out like it will continue the trend.

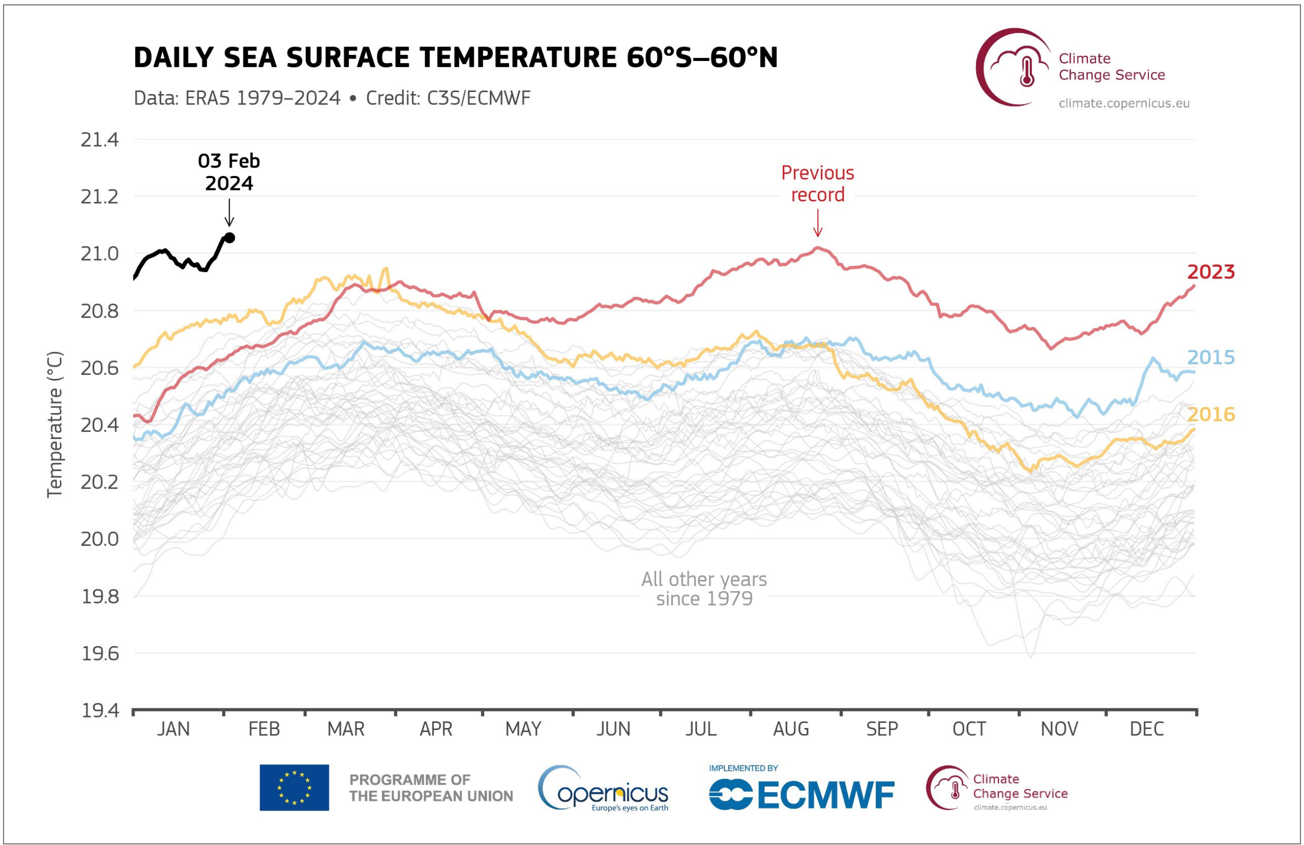

Figure 3: Monthly estimates of global mean sea temperature.

Calibrated in degrees centigrade, estimated average daily temperatures are plotted horizontally from January through December for each year between 1850 and 2023. Pre-industrial temperatures hovered around an estimated average of 19.4 degrees, so 2024 started about 1.5 degrees above pre-industrial levels.

Source: https://climate.copernicus.eu/warmest-january-record-12-month-average-over-15degc-above-preindustrial

In addition to recognizing a new boundary condition, new projections of the future beginning in 2024 should focus more attention to risk patterns over space and time to provide a richer space within which to test coupled models' relative "fitness for (the) purpose" of not only detecting and attributing what has happened, but also projecting useful and credible distributions of future outcomes. These distributions must be calibrated in metrics that display both changes in climatic variables of particular import and socio-economic variables that drive human well-being across the planet's diverse populations. In that regard, apparently random events that occur in real life as the future unfolds should provide valuable context for testing the "fitness for purpose" of various models, identifying important components of the climate system that must be included moving forward, and indicating what new data and process understandings are required.

We also need to strengthen our understandings at scale so we can begin to produce and explore coupled modelling efforts across multiple research efforts over multiple TPs. Coupled models are necessary, of course, because the climate's component systems are coupled. For example, changes in ice sheet force ocean changes and ocean changes force ice sheet changes. That much is clear, but even the sign of the combined changes is not known because it has not been studied.

Finally, I offer one warning. New coupled models also have the potential of producing wrong science because of system interactions within their structures. If one component is wrong and models of other components are using its outputs, then they can be doing wrong or at least misguided science. The skill of such modelling efforts is, in short, ultimately determined by the weakest link in the models' causal chains.

Some Concluding Remarks About Structure and Process

In light of all of this, it is apparent that humanity needs urgently to fund a new round of integrated multidisciplinary research with lots of new funding – an investment in new and value knowledge whose price tag would be much smaller than the social expense than would be incurred from the extra suffering imbedded in the dark tails of the future materialize if society has done little or nothing to find out how and why that might happen.

Perhaps this will require engaging multiple research efforts in existing large research centers at home and abroad who had earlier been assigned the task of using model comparison experiments to determine "fitness for purpose" that led to effective D&A investigations – as emphasized above, this work exploited multiple signals that anthropogenic warming was changing the climate in potentially devastating ways that could be explained in large measure by some fundamental and well accepted physics. Our collective ability to attribute detected changes to human activity is much stronger as a result of these efforts, but that process took decades to reach this state, and we don't have that much time when it comes to TPs. Other research structures and processes may be more effective, and that is certainly a topic for collective deliberation.

Funding Is Not the Only Scarce Resource

Funding will not be the only scarce resource standing in the way of progress on TPs. Human capital is not currently in sufficient supply to meet the challenge and so a new coterie of scientists have to step up. This is a fact because the established cohort working on climate change and climate risk already have too much on their plates. Newly minted scientists need to be attracted to this work, but that will not happen spontaneously. They need to be encouraged in their training and by invitations to working meetings where they will learn in real time what is going on and where the critical gaps in knowledge lie.

I have another case study to report in this regard. Upwards of 30% of the participants who were invited each year to the annual meetings of the Energy Modelling Forum (EMF, 2013) in Snowmass, Colorado on climate change impacts were selected because they were skilled in a relevant science or social science but new to the integrated topic of climate change. They were invited to share their work and hear reactions from the people whose work could might soon fill the reference lists of their future publications. Over 15 years, collaborations born of those meetings produced over 1000 contributions to the academic literature. Many participants continued to work on climate after just one year and populated a new generation of scholars across multiple related disciplines. These experiences are reasons for hope as we start the new phase of research described here.

The Pitch – A Call for Help

The title of this journal is Advances in Earth and Environmental Science and this perspective has added the adjectives "future" and/or "aspirational" to its scope. If you are reading this, you know that that was ok with the editors. They agree that humanity needs your help.

I wrote to encourage new and established scientists and social scientists of all flavors to volunteer to work on some part of the tipping point problem. Get up to speed. Pay attention to who is doing what, and lend your voice. Tell our recommitted community what you are doing and describe what you are finding. In turn, you will find yourself being engaged in collaborative working meetings with experts from around the globe. From that group, you will find encouragement, critical and productive review, as well as engaged mentoring. You will be included in collaborations that will not only produce path breaking science, but also lifelong colleagues. Many of those colleagues and collaborators will become some of your best friends.

Closing on a personal note, I have best friends living or spending a lot of time on six different continents. I hardly ever see them in person any more (at least since COVID), but it always seems like no time has passed whenever our paths do cross. Your new colleagues will know your work and contribute to it, but they will do more than that. They will also get to know you and your families (and you, in turn, will get to know them and theirs). You will be a citizen of the world. You will be contributing to the global public good not to mention the welfare of people and things that live and work near where you live. Your own work will be noticed and rewarded; but more importantly, you will know the satisfaction of working to grow an essential body of knowledge.

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