

Fat-Tailed Uncertainty in the Economics of Catastrophic Climate Change

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Abstract

In this article I revisit some basic issues concerning structural uncertainty and catastrophic climate change. My target audience here is general economists, so this article could also be viewed as a somewhat less technical exposition that supplements my previous work. Using empirical examples, I argue that it is implausible that low-probability high-negative-impact events would not much influence an economic analysis of climate change. I then try to integrate the empirical examples and the theory together into a unified package with a unified message that the possibility of ruinous climate change needs to be taken seriously.

JEL classification: Q54; keywords: catastrophic climate change.

1 Introduction

I believe that the most striking feature of the economics of climate change is that its extreme downside is non-negligible. Deep structural uncertainty about the unknown unknowns of what might go very wrong is coupled with essentially unlimited downside liability on possible planetary damages. This is a recipe for producing what are called “fat tails” in the extremes of critical probability distributions. There is a race being run in the extreme tail between how rapidly probabilities are declining relative to how rapidly damages are increasing. Who wins this race, and by how much, depends on how fat (with probability mass) the extreme tails are. It is difficult to judge how fat the tail of ruinous climate change might be because it represents events that are very far outside the realm of ordinary experience.

*(mweitzman@harvard.edu) Without blaming them for the remaining deficiencies in this paper, I am extremely grateful for the constructive comments on an earlier version by James Annan, Daniel Cole, Stephen DeCanio, Baruch Fischhoff, Don Fullerton, John Harte, William Hogan, Matthew Kahn, David Kelly, Michael Oppenheimer, Robert Pindyck, Joseph Romm, and Richard Tol.

In this article, which is part of a symposium on Fat Tails and the Economics of Climate Change, I address some criticisms that have been leveled at previous work of mine on fat tails and the so-called “dismal theorem.”¹ At first I was inclined to debate some of the critics and their criticisms more directly. But on second thought I found myself anxious not to be drawn into being too defensive and having the main focus be on technical details. Instead, I am more keen here to emphasize anew and in fresh language the substantive concepts that, I think, may be more obscured than enlightened by a debate centered on technicalities. I am far more committed to the simple basic ideas that underlie my approach to fat-tailed uncertainty and the economics of catastrophic climate change than I am to the particular mathematical form in which I have chosen to express them. These core concepts could have been wrapped in a variety of alternative mathematical shells – and the particular one that I chose to use previously is somewhat arbitrary. The implications are roughly similar, irrespective of formalization. Some technical details are unavoidable, but if I can give the underlying concepts greater intuitive plausibility, then I believe that this set of ideas will become more self-evident and more self-evidently resistant to several of the criticisms that have been leveled against it.

In the next section, I present an intuitive-empirical argument that deep structural uncertainty lies at the heart of climate change economics. Then, in the following section, I try to explain some of the theory behind fat-tailed extreme events and discuss some possible implications for the analysis of climate change. I offer a few summary remarks in the final section.

2 Some Empirical Examples of Deep Structural Uncertainty about Climate Extremes

In this section I try to make a heuristic-empirical case (in the form of five “stylized facts”) for there being big structural uncertainties in the economics of extreme climate change. I will argue on intuitive grounds that the way in which this deep uncertainty is conceptualized and formalized should influence the outcomes of any reasonable benefit-cost analysis (BCA) of climate change. Further, I will argue that the seeming immunity of the “standard” BCA to the possibility of extreme possibilities seems both peculiar and disturbing. My arguments in this section are not intended to be airtight or rigorous. Rather, this is an intuitive presentation based on some very rough stylized facts.

¹This symposium also includes articles by Nordhaus (2011), Pindyck (2011), and Cooke (2011). The “dismal theorem,” introduced in Weitzman (2009a), will be discussed later in this article.

By benefit-cost analysis (BCA) of climate change, I mean, in the widest sense, some overall economic analysis centered on maximizing (or at least comparing) welfare. My notion of BCA in the present context is so broad that it overlaps with an integrated assessment model (IAM), and here I treat the two as essentially interchangeable. I begin by setting up a straw man that I will label the “standard BCA of climate change.” Of course there is no “standard BCA of climate change,” but I think this is an allowable simplification for purposes of exposition here.

We all know that computer-driven simulations are dependent upon the core assumptions of the underlying model. The intuitive examples presented below are frankly aimed at sowing a few seeds of doubt that the “standard BCA of climate change” fairly represents structural uncertainties about extreme events, and that therefore its conclusions might be less robust than is commonly acknowledged. I do not argue that the standard model is wrong or even implausible, but rather argue that it may not be robust with respect to the modeling of catastrophic outcomes. I will try to make my case by citing five aspects of the climate science and economics that do not seem to me to be adequately captured by the standard BCA. The five examples – which I call “Exhibits 1, 2, 3, 4 and 5” – are limited to structural uncertainty concerning the modeling of climate disasters. While other important aspects of structural uncertainty might also be cited, I restrict my stylized facts to these five examples. In the spirit of performing a kind of a “stress test” on the standard BCA, I naturally concentrate on things that might go wrong rather than things that might go right.

2.1 Exhibit 1: Past and Present Greenhouse Gas Concentrations

“Exhibit 1” concerns the atmospheric level of greenhouse gases (GHGs) over time. Ice core drilling in Antarctica began in the late 1970s and is still ongoing. The record of carbon dioxide (CO₂) and methane (CH₄) trapped in tiny ice-core bubbles currently spans 800,000 years.² It is important to recognize that the numbers in this unparalleled 800,000-year record of GHG levels are among the very best data that exist in the science of paleoclimate. Almost all other data (including past temperatures) are inferred indirectly from proxy variables, whereas these ice-core GHG data are directly observed.

The pre-industrial-revolution level of atmospheric CO₂ (about two centuries ago) was 280 parts per million (ppm). The ice-core data show that CO₂ varied gradually during the last 800,000 years within a relatively narrow range roughly between 180 and 280 ppm and has never been above 300 ppm. Currently, CO₂ is over 390 ppm, and climbing steeply. In 800,000 years, methane has never been higher than 750 parts per billion (ppb), but now

²My numbers are taken from Dieter et al (2008), and supplemented by data from the Keeling curve for more recent times (available online at: ftp://ftp.cmdl.noaa.gov/ccg/co2/trends/co2_mm_mlo.txt).

this extremely potent GHG, which is 22 times more powerful than CO₂ (per century), is at about 1,800 ppb. The sum total of all carbon-dioxide-equivalent (CO₂e) GHGs is currently at about 450 ppm. An even more startling contrast with the 800,000-year record is the rate of change of GHGs: increases in CO₂ were below (and typically well below) 25 ppm within any past sub-period of 1,000 years, while now CO₂ has risen by over 25 ppm in just the last 10 years. Thus, anthropogenic activity has very rapidly elevated atmospheric CO₂ and CH₄ to levels very far outside their natural range. The unprecedented scale and speed of GHG increases brings us into uncharted territory and makes predictions of future climate change very uncertain. Looking ahead a century or two, the levels of atmospheric GHGs that may ultimately be attained (unless decisive measures are undertaken) have likely not existed for tens of millions of years and the speed of this change may be unique on a time scale of hundreds of millions of years.

Remarkably, the “standard BCA of climate change” takes little account of the magnitude of the uncertainties involved in extrapolating future climate change so far beyond past experience. Perhaps even more surprising, the gradual tightening of GHG emissions, which emerges as optimal policy from the “standard” BCA, typically attains stabilization at levels of CO₂ that approach 700 ppm (and levels of CO₂e that are even higher). The “standard” BCA thus recommends subjecting the Earth’s system to the unprecedented shock of instantaneously (in geological terms) jolting atmospheric stocks of GHGs up to two and a half times above their highest level over the last 800,000 years – without mentioning the unprecedented nature of this unique planetary experiment. This is my Exhibit 1.

2.2 Exhibit 2: The Uncertainty of the Climate-Change Response

“Exhibit 2” concerns the highly uncertain climate-change response to the kind of unprecedented increases in GHGs that were described in Exhibit 1 above. For specificity, I focus on the uncertainty of so called “equilibrium climate sensitivity,” which is a key macro-indicator of the *eventual* temperature response to GHG changes. This is a good example of a “known unknown.” However, it should be understood that under the rubric of climate sensitivity I am trying to lump together an entire suite of other uncertainties, including some non-negligible unknown unknowns. So “equilibrium climate sensitivity” is to be understood here as a prototype example of uncertainty, or a metaphor, which is being used to illustrate much more generic issues in the economics of uncertain climate change. The insights and results of this “Exhibit 2” are not intended to stand or fall on the single narrow issue of accurately modeling the effects of uncertain climate sensitivity.

The Intergovernmental Panel on Climate Change (IPCC-AR4 (2007)) defines equilibrium

climate sensitivity (hereafter S) this way: “The equilibrium climate sensitivity is a measure of the climate system response to sustained radiative forcing. It is not a projection but is defined as the global average surface warming following a doubling of carbon dioxide concentrations. It is *likely* to be in the range 2 to 4.5°C with a best estimate of 3°C, and is *very unlikely* to be less than 1.5°C. Values substantially higher than 4.5°C cannot be excluded, but agreement of models with observations is not as good for those values.” The actual empirical reason for why the upper tails of these probability distributions are long and seem heavy with probability dovetails with Bayesian theory: inductive knowledge is always useful, of course, but simultaneously it is limited in what it can tell us about extreme events outside the range of experience. In such situations one is forced back onto depending more than one might wish upon prior beliefs in the form of a prior probability distribution, which of necessity is largely subjective and relatively diffuse.

Thus, any curve fitting exercise attempting to attribute probabilities to $S \geq 4.5^\circ\text{C}$, such as I am doing here, is little more than conjectural speculation. My purpose is merely to show that critical results can depend on seemingly casual decisions about how to model tail probabilities. To illustrate some striking implications for the analysis of climate change, I contrast the use of two familiar probability distributions to represent the upper-half tail of climate sensitivity above the median: (1) the Pareto (or Power or Polynomial) distribution, subscripted **P**, which is the prototype example of a fat upper tail; and (2) the Normal distribution, subscripted **N**, which is the prototype of a thin upper tail. (By common definition, probabilities that decline exponentially or faster, like the Normal distribution, are thin tailed while probabilities that decline polynomially or slower, like the Pareto distribution, are fat tailed). The IPCC defines “likely” as a probability above 66% but below 90%, which would mean that the probability that climate sensitivity is greater than 4.5°C ($\text{Prob}[S \geq 4.5^\circ\text{C}]$) is between 5% and 17%. A more recent average estimate of 14 leading climate scientists is $\text{Prob}[S \geq 4.5^\circ\text{C}] = 23\%$.³ Here I choose $\text{Prob}[S \geq 4.5^\circ\text{C}] = 15\%$ and I calibrate parameters of both probability distributions so that $\text{Prob}[S \geq 3^\circ\text{C}] = .5$, and $\text{Prob}[S \geq 4.5^\circ\text{C}] = .15$.⁴

Table 1 presents cumulative probabilities of climate sensitivity for the Pareto and Normal distributions.

³Zickfield, Morgan, Frame, and Keith [2010].

⁴I lean more toward $\text{Prob}[S \geq 4.5^\circ\text{C}] \leq 17\%$ than toward $\text{Prob}[S \geq 4.5^\circ\text{C}] \geq 5\%$ because, for a time horizon of a century and a half or so, it is plausible that the more inclusive and larger “earth system sensitivity” (which includes slow feedbacks like methane releases) matters more than the “fast-feedback equilibrium sensitivity” that IPCC-AR4 refers to. For more on the distinction between fast and slow feedback climate sensitivities see, e.g., Hansen et al (2008).

$\widehat{S} =$	3°C	4.5°C	6°C	8°C	10°C	12°C
$\text{Prob}_{\mathbf{P}}[S \geq \widehat{S}]$.5	.15	.06	.027	.014	.008
$\text{Prob}_{\mathbf{N}}[S \geq \widehat{S}]$.5	.15	.02	.003	7×10^{-7}	3×10^{-10}

Table 1: $\text{Prob}[S \geq \widehat{S}]$ for fat-tailed Pareto and thin-tailed Normal distributions

Table 1 indicates a tremendous difference in upper tail behavior between the fat-tailed Pareto distribution and the thin-tailed Normal distribution. I think that the Pareto distribution of climate sensitivity has a disturbingly large amount of probability ($\text{Prob}_{\mathbf{P}}$) in its upper tail. There is no consensus on how to aggregate the results of many different climate sensitivity studies into one overarching probability density function (PDF), and there is much controversy about how it might be done. But for what it is worth (perhaps very little), the median upper five percent probability level over all 22 climate-sensitivity PDFs cited in IPCC-AR4 is 6.4°C, which fits with the Pareto PDF in Table 1 above.⁵

Table 2 presents some values of probabilities of increased eventual global mean surface temperatures (T) as a function of stationary greenhouse gas concentrations (G).⁶ The first row of Table 2 represents levels of G (measured in ppm of CO₂e). The second row gives the median equilibrium temperature as a function of stabilized GHG stocks. The remaining rows give the probabilities ($\text{Prob}_{\mathbf{P}}$ and $\text{Prob}_{\mathbf{N}}$) of achieving at least the steady state temperature increase represented by the entries in the table (5°C and 10°C) for both of the chosen PDFs (Pareto= \mathbf{P} =fat tail; Normal= \mathbf{N} =thin tail).

G:	400	500	600	700	800	900
Median T	1.5°	2.5°	3.3°	4.0°	4.5°	5.1°
$\text{Prob}_{\mathbf{P}}[T \geq 5^\circ\text{C}]$	1.5%	6.5%	15%	25%	38%	52%
$\text{Prob}_{\mathbf{N}}[T \geq 5^\circ\text{C}]$	10^{-6}	2.0%	14%	29%	42%	51%
$\text{Prob}_{\mathbf{P}}[T \geq 10^\circ\text{C}]$.20%	.83%	1.9%	3.2%	4.8%	6.6%
$\text{Prob}_{\mathbf{N}}[T \geq 10^\circ\text{C}]$	10^{-30}	10^{-10}	10^{-5}	.1%	.64%	2.1%

⁵For details see Weitzman (2009a).

⁶Table 1 is giving probabilities for climate sensitivity, which corresponds to GHG levels of 560 ppm of CO₂e (a doubling of the pre-industrial-revolution level of 280 ppm). The different values of GHG concentrations in Table 2 give correspondingly different probabilities of temperatures responses, in proportion to the logarithm of GHG concentrations relative to 280ppm.

Table 2: Probabilities of exceeding $T = 5^\circ\text{C}$ and $T = 10^\circ\text{C}$ for given $G = \text{ppm}$ of CO_2e .

What is especially striking to me about Table 2 is the reactivity of high-temperature probabilities to the level of GHGs. The fat-tailed Pareto case seems especially worrisome. One implication is that an optimal policy might be expected to keep GHG levels down and be much less casual about letting CO_2e levels exceed 700 ppm than the “standard” BCA. I believe that Table 2 could be taken as some indirect evidence that the main purpose of keeping GHG concentrations down is effectively to buy insurance against catastrophic global warming. The above examples of the highly uncertain eventual temperature response to unprecedented increases in GHGs constitutes my Exhibit 2.

2.3 Exhibit 3: A Physical Basis for Catastrophic Outcomes

“Exhibit 3” concerns possibly disastrous releases over the long run of bad-feedback components of the carbon cycle that are currently omitted from most general circulation models. The chief concern here is that there may be a significant supplementary component that conceptually should be added on to the so-called “fast feedback” equilibrium concept that IPCC-AR4 works with. This omitted component (which would be part of a more inclusive slow-feedback generalization called “earth system sensitivity”) includes the powerful self-amplification potential of greenhouse warming that is due to heat-induced releases of sequestered carbon. One vivid example is the huge volume of GHGs currently trapped in tundra permafrost and other boggy soils (mostly as methane, a particularly potent GHG). A more remote (but even more vivid) possibility, which in principle should also be included, is heat-induced releases of the even-vaster offshore deposits of methane trapped in the form of clathrates.⁷

There is a very small and unknown (but decidedly non-zero) probability over the long run of having destabilized methane from these offshore clathrate deposits seep into the atmosphere if the temperature of the waters bathing the continental shelves warms just slightly. The amount of methane involved is huge, although it is not precisely known. Most estimates place the carbon-equivalent content of methane hydrate deposits at about the same order of magnitude as all other fossil fuels combined. Over the long run, a methane outgassing-amplifier process could potentially precipitate a disastrous strong-positive-feedback warming.

⁷Clathrates (or hydrates) are methane molecules that are boxed into a semi-stable state by being surrounded by water molecules under high pressure and low temperatures. For more about methane clathrates, see Archer (2007) and the recent article by Shakova et al (2010).

If it occurred at all, such an event would likely take centuries to materialize because the presumed initiator would be the slow-acting gradual warming of ocean waters at the depths of the continental shelves. Thus, while it is a low-probability event that might transpire only centuries from now (if at all), the possibility of a climate meltdown is not just the outcome of a mathematical theory but has a real physical basis. Other examples of an actual physical basis for catastrophic outcomes could be cited, but this one will do here. This is my Exhibit 3.

2.4 Exhibit 4: Damages of Extreme Climate Change

“Exhibit 4” concerns what I view as a somewhat cavalier treatment in the literature of damages or disutilities from extreme temperature changes. The “standard” BCA damages function reduces welfare-equivalent output at mean global temperature change T by a quadratic-polynomial *multiplier* of the form $M(T) = \alpha T^2 / (1 + \alpha T^2)$.⁸ The results in terms of fractional damages to output are given as $M(T)$ in Table 2.

T	2°C	4°C	6°C	8°C	10°C	12°C
$M(T)$	1%	4%	8%	13%	19%	26%

Table 3: Multiplicative-quadratic damages $M(T)$ (as fraction of output)

I do not find the numbers in Table 2 convincing for higher temperatures. At an extraordinarily high global average temperature change of $T=10^\circ\text{C}$, the welfare-equivalent damage in Table 3 is a loss of “only” 19% of world output *at the time of impact*. If the annual growth rate is, say, 2% and the time of impact is, say, two centuries from now, then the welfare difference between no temperature change in 200 years and a temperature change of 10°C in 200 years is the difference between output that is 55 times larger than current levels and output that is 44 times larger than current levels. This is equivalent to a reduction in projected annual growth rates from 2% to 1.9%. Such kind of mild damages function is practically preordained to make extreme climate change look empirically negligible, almost no matter what else is assumed. Conversely, it turns out, fat-tailed temperature PDFs are not by themselves sufficient to make extreme climate change have empirical “bite” without a damages function that is far more immiserising at high temperature changes.

⁸ For the sake of concreteness, I take the value of the parameter α used in the latest version of the Nordhaus’s well known DICE model (Nordhaus (2008)). The DICE model is perhaps the most famous integrated assessment model (IAM) in the economics of climate change. The value $\alpha = .002388$ was used to generate his Figure 3-3 on page 51. I hasten to add in fairness that this specification was intended only to capture low temperature damages and was never intended to be extrapolated to very high temperature changes.

So what should the damages function be for very high temperatures? No one knows, of course. Taking an extreme example, suppose for the sake of argument that average global temperatures were to increase by the extraordinary amount of 10°C (with a low probability, and occurring centuries hence). It is true that people live very well in places where the mean temperature is 10°C higher than in Yakutsk, Siberia. However, I do not think that these kinds of analogies can justify using such a comparative-geography approach for estimating welfare-equivalent damages from an average planetary temperature change of 10°C . Global mean temperatures involve a double averaging: across space and over time. A “damages function” is a reduced form representing global welfare losses from global average temperatures, which subsumes a staggering amount of regional, seasonal, and even daily weather heterogeneity. Regional and seasonal climate changes are presumably much more unpredictable than global average surface temperatures. There is just too much structural uncertainty and too much heterogeneity to put trustworthy bounds on the unprecedented almost-unimaginable changes to planetary welfare from average global temperatures increasing by 10°C . When there is such big uncertainty about catastrophic damages, and when the damages function for high temperature changes is so conjectural, the relevant degree of risk aversion, yet another important unknown here, will tend to play a significant role in an economic analysis of climate change.

Of course I have no objective way to determine the magnitudes of high-temperature damages. The last time the world witnessed periods where global average temperatures were very roughly 10°C or so above the present was during the Eocene epoch $\approx 55\text{-}34$ million years ago. During these warming periods the earth was ice free while palm trees and alligators lived near the North Pole. The Eocene was also the last epoch in which there were geologically rapid increases in mean global temperatures of magnitude $\approx 5^{\circ}\text{C}$ or so above an already warm background. Such hyperthermal events occurred over an average period of very roughly $\approx 100,000$ years or so, which is extremely gradual compared to current worst-case anthropogenically-induced trajectories. It is unknown what exactly triggered these Eocene temperature increases, but they were accompanied by equally striking atmospheric carbon increases. One likely culprit is the strong-feedback release of large amounts of methane hydrates from clathrate deposits (Exhibit 3), which is a non-negligible possibility over the next century or two if current GHG emissions trends are extrapolated. The major point here is that relatively rapid changes of global average temperatures $\approx 5^{\circ}\text{C}$ above present values are extremely rare events and are extraordinarily far outside the scope of human experience. For huge temperature increases like $T \approx 10^{\circ}\text{C}$, the planetary effects are even more difficult to imagine. To find a geologically instantaneous increase in average global temperatures of magnitude $T \approx 10^{\circ}\text{C}$, one would have to go back hundreds of millions of years.

For me, 10°C has a vivid image as a kind of a reference point from a recent study, which estimated that global average temperature increases of $\approx 11\text{-}12^{\circ}\text{C}$ (with, importantly, accompanying humidity in the form of high wet-bulb temperatures) would cross an absolute thermodynamic limit to metabolic heat dissipation. Across this threshold, represented by a wet-bulb temperature of 35°C , more than half of today’s human population would be living in places where, at least once a year, there would be periods when death from heat stress would ensue after about six hours of exposure.⁹ (By contrast, today the highest wet bulb temperature anywhere on Earth is about 30°C). The authors of this study furthermore point out: “This likely overestimates what could practically be tolerated: Our [absolute thermodynamic] limit applies to a person out of the sun, in a gale-force wind, doused with water, wearing no clothing and not working.” Even at much lower wet-bulb temperatures than 35°C , life would become debilitating and physical labor would be unthinkable. The massive unrest and uncontainable pressures this might bring to bear on the world’s human population are almost unimaginable. The Earth’s ecology, whose valuation is another big uncertainty, would be upended. A temperature change of $\approx 10^{\circ}\text{C}$ therefore represents an extreme threat to human civilization and global ecology as we now know it, even if, conceivably, it might not necessarily mean the end of *Homo sapiens* as a species. I don’t think that a person needs accurate specific stories about what might happen for $T \approx 10^{\circ}\text{C}$ to imagine truly ruinous catastrophes undoing the planet and severely undermining the security of human civilization – at the very minimum.

It must be said clearly that very high atmospheric temperature changes like $T = 10^{\circ}\text{C}$ can take several centuries to attain. The higher the limiting temperature, the longer it takes to achieve equilibrium because the oceans will first have to absorb the enormous amounts of heat being generated. Alas, if the oceans are building up enormous amounts of heat it could set in motion irreversible long-term methane clathrate releases from the continental shelves, along with some other nasty surprises. Thus, overall damages generated by equilibrium $T = 10^{\circ}\text{C}$ are best conceptualized as associated with *being on the trajectory* whose asymptotic limiting atmospheric temperature change is $T = 10^{\circ}\text{C}$.

As noted above, the “standard” BCA damages function reduces welfare-equivalent consumption by a quadratic-polynomial multiplier. This essentially describes a single-attribute utility function, or, equivalently, a multi-attribute utility function with strong substitutability between the two attributes of consumption and temperature change. This would be an appropriate formulation if the main impact of climate change is, say, to drive up the price of food and increase the demand for air conditioning. This particular choice of functional form allows the economy to easily substitute higher consumption for higher temperatures, since

⁹Sherwood and Huber (2010).

the limiting elasticity of substitution between consumption and higher temperatures is one (due to the multiplicative-polynomial assumption). Very different optimal policies can be produced when other functional forms are used to express the disutility of disastrously high temperatures. For example, suppose that instead of being *multiplicatively separable* (as in the “standard” BCA), the disutility of temperature change is instead *additively separable*. This means that welfare is the analogous additively-separable *arithmetic difference* between a utility function of consumption and a quadratic loss function of temperature changes. This specification amounts to postulating a genuine multi-attribute utility function that describes a situation where the main impact of climate change is on things that are not readily substitutable with material wealth, such as biodiversity and health. If the utility function has a constant coefficient of relative risk aversion of two, it implies an elasticity of substitution between consumption and temperature change of one half. Empirically, using this additive form – even without *any* uncertainty in temperatures – prescribes a significantly more stringent curtailment of GHG emissions than what emerges from the analogous multiplicative form of the “standard” BCA.¹⁰ Allowing the possibility of high temperatures (even with low probabilities) would presumably exaggerate even more this difference. Such fragility to basic functional forms is disturbing because we cannot know confidently which specification of the utility function is more appropriate.

The above discussions and examples of non-robustness with respect to a damages function for high temperatures constitutes my Exhibit 4.

2.5 Exhibit 5: Discounting the Distant Future

“Exhibit 5” concerns the notorious issue of how to discount the distant future. The effects of global warming and climate change will be spread out over centuries and even millennia from now. The logic of compounding a constant positive interest rate forces us to say that what one might conceptualize as monumental – even earth-shaking – events like disastrous climate change, do not much matter when they occur in the deep future. Perhaps even more disturbing, when exponential discounting is extended over very long time periods there is a truly extraordinary dependence of BCA on the choice of a discount rate. Seemingly

¹⁰With a coefficient of relative risk aversion of two, the above additively-separable specification is mathematically equivalent to the constant elasticity of substitution (CES) specification of Sterner and Persson (2008) with elasticity of substitution one half. In their pioneering study, Sterner and Persson showed empirically – by plugging it into Nordhaus’s deterministic DICE model – that their CES (or, equivalently, my additive) welfare specification prescribes a significantly more aggressive policy response to global warming (with a significantly higher carbon tax) than the analogous multiplicative specification of the “standard” BCA. The disturbingly significant distinction between utility functions that are multiplicatively-separable in consumption and temperature change and utility functions that are additively separable in consumption and temperature change is explored in some detail in Weitzman (2009b).

insignificant differences in discount rates can make an enormous difference in the present discounted value of distant future payoffs. In many long run situations, almost any answer to a BCA question can be defended by one or another particular choice of a discount rate. This is true in general, but it is an especially acute problem when distant future events like climate change (especially catastrophic climate change) are being discounted.

There is a high degree of uncertainty about what should be taken as the appropriate real rate of return on capital in the long run, accompanied by much controversy about its implications for long-run discounting. There is no deep reason or principle that allows us to extrapolate past rates of return on capital into the distant future. The industrial revolution itself began some two centuries ago in Britain, and only slowly thereafter permeated throughout the world. The seeming trendlessness of some past rates of return is a purely empirical reduced-form observation, which is not based on any underlying theory that would allow us to confidently project past numbers far into the future. There are a great many fundamental factors that cannot easily be extrapolated, just one of which is the unknown future rate of technological progress. Even leaving aside the question of how to project future interest rates, additional issues for climate change involve which interest rate to choose out of a multitude of different rates of return that exist in the real world.¹¹ Furthermore, there is a strong normative element having to do with what is the “right” rate, which brings an ethical dimension to discounting climate change across many future generations that is difficult to evaluate and incorporate into standard BCA. This normative issue is further complicated when the event affecting future generations is a low-probability high-impact catastrophic outcome. If the utility function is additively separable in damages and consumption, then the appropriate interest rate for discounting damages is not the rate of return on capital but rather the rate of pure time preference, which is arguably near zero for a normative evaluation of climate change.

The constant interest rates used for discounting in the “standard” BCA would be viewed by many people as severely biasing BCA toward minimizing into near-nothingness the present discounted value of distant future events, like climate change. This kind of exponential discounting, perhaps more than anything else, makes scientists and the general public suspicious of the economist’s “standard” BCA of climate change, since it trivializes even truly enormous distant-future impacts. To be honest, I think that among economists there are few who do not feel uneasy about evaluating distant-future climate-change impacts this way. One line of research, in which I have been involved, shows that when the discount rate itself is uncertain it implies that the “effective” discount rate declines over time to its lowest possible value (see Weitzman (1998)). Empirically, this effect can be quite powerful (see Weitzman

¹¹For more on this see Weitzman (2007).

2010). The driving force is a “fear factor” from risk aversion to permanent productivity shocks representing bad future states of the world. Whatever its source, the unknown discount rate (and the extraordinary sensitivity of policy to its choice) is yet another big structural uncertainty in the economic analysis of climate change, especially for evaluating possible catastrophes. This is my Exhibit 5.

2.6 A Long Chain of Structural Uncertainties

The above five “exhibits” could readily be extended to incorporate yet more examples of structural uncertainty, but enough is enough. To summarize, the economics of climate change consists of a very long chain of tenuous inferences fraught with big uncertainties in every link: beginning with unknown base-case GHG emissions; compounded by big uncertainties about how available policies and policy levers will affect actual GHG emissions; compounded by big uncertainties about how GHG flow emissions accumulate via the carbon cycle into GHG stock concentrations; compounded by big uncertainties about how and when GHG stock concentrations translate into global average temperature changes; compounded by big uncertainties about how global average temperature changes decompose into specific changes in regional weather patterns; compounded by big uncertainties about how adaptations to, and mitigations of, climate-change damages at a regional level are translated into regional utility changes via an appropriate “damages function”; compounded by big uncertainties about how future regional utility changes are aggregated into a worldwide utility function and what its overall degree of risk aversion should be; compounded by big uncertainties about what discount rate should be used to convert everything into expected-present-discounted values. The result of this lengthy cascading of big uncertainties is a reduced form of truly extraordinary uncertainty about the aggregate welfare impacts of catastrophic climate change, which is represented mathematically by a PDF that is spread out and heavy with probability in the tails.

What I would wish the reader to take away from these five exhibits is the notion that the seeming immunity of the “standard” BCA to such stylized facts is both peculiar and disturbing. An unprecedented and uncontrolled experiment is being performed by subjecting planet Earth to the shock of a geologically-instantaneous injection of massive amounts of GHGs. Yet the standard BCA seems almost impervious to the extraordinarily uncertain probabilities and consequences of catastrophic climate change. In light of the above five exhibits of structural uncertainty, the reader should feel intuitively that it goes against common sense that a climate-change BCA does *not* much depend upon how potential future disasters are modeled and incorporated into the BCA. This uneasy intuitive feeling based

on stylized empirical facts is my opening argument. I turn next to the theory.

3 The Dismal Theorem, Infinity, and BCA: A Theoretical Framework

I begin this section by asking why it is relevant in the first place to have any supporting theory if the five stylized-fact “exhibits” from the previous section are convincing. Why aren’t these stylized facts alone sufficient evidence that there is a problem with the “standard BCA of climate change”? My answer is that a combined theoretical plus empirical-intuitive argument delivers a particularly powerful one-two punch at the treatment of structural uncertainty in the standard BCA. In this respect I believe that the whole of my argument is bigger than the sum of its two parts. The theoretical part reinforces the empirical part by placing it within a formal mathematical framework. When the intuitive “exhibits” are seen as reflecting some formalized theoretical structure, then it becomes less easy to brush them aside as mere sniping at an established model. In this theoretical section, as in the empirical section above, I emphasize the intuitive plausibility of the case I am trying to make – focusing here on the underlying logic that drives the theory.

In the previous section I argued that it is only common sense that climate-change policy implications should depend on the treatment of low-probability extreme-impact outcomes. The main question I attempted to address in Weitzman (2009a) was whether such intuitive dependence is reflecting some deeper principle. My answer was that there is indeed a basic underlying theoretical principle (the “Dismal Theorem” – hereafter DT) that points in this direction. The simple logic can be grasped intuitively without using (or understanding) the fancy math required to state and prove a formal version of the principle. In this section I restate the theoretical arguments underlying the DT in what is hopefully a more intuitive form.

Let welfare W stand for expected present discounted utility, whose theoretical upper bound is B . Let $D \equiv B - W$ be expected present discounted *dis*utility. Here D stands for what might be called the “diswelfare” of climate change. Assume for the sake of argument that D is “essentially” unbounded in the particular case of climate change because global liability is “essentially” unlimited in a worst-case scenario. (More later on what happens when D is, technically, bounded.) Because the integral over a nonnegative probability measure is one, the PDF of $\ln D$ must decline to zero. In other words, extreme outcomes can happen, but their likelihood diminishes to zero as a function of how extreme the outcome might be. The idea that extreme outcomes cannot be eliminated altogether, but are hy-

pothetically possible with some positive probability, is not at all unique to climate change. Almost nothing in our world has a probability of exactly zero or exactly one. What is worrisome is not the fact that the upper tail of the PDF of $\ln D$ is *long* (reflecting the fact that a meaningful bound on diswelfare does not exist), but that it might be *fat* (reflecting the fact that the probability of a ruinous outcome is not sufficiently small to give comfort). The critical question, which tail fatness quantifies, is *how fast* does the probability of a catastrophe decline relative to the welfare impact of the catastrophe.

Unless otherwise noted, my default meaning of the term “fat tail” (or “thin tail”)¹² henceforth concerns the upper tail of the PDF of $\ln D$, resulting from whatever combination of probabilistic temperature changes, temperature-sensitive damages, utility functions, discounting, and so forth, makes this come about. *This* is the PDF that ultimately matters. Empirically, it is not the fatness of the tail of the climate sensitivity PDF *alone* or the reactivity of the damages function to high temperatures *alone*, or the degree of relative risk aversion *alone*, or the rate of pure time preference *alone*, or any other factor *alone*, that counts, but rather the *interaction* of all such factors in determining the upper-tail fatness of the PDF of $\ln D$. For example, other things being equal the PDF of $\ln D$ has a relatively fatter tail the larger is the probability of high temperatures and the greater is the reactivity of the damages function to high temperatures, but neither condition alone implies a fat-tailed PDF of $\ln D$. It may seem arcane, but the upper-tail fatness of the reduced form PDF of $\ln D$ is the core issue in the economics of catastrophic climate change. Of course it is extremely difficult to know the fatness of the upper tail of the PDF of $\ln D$, which is precisely the main point of this line of research and this article.

In Weitzman (2009a), I indicated a theoretical tendency for the PDF of $\ln D$ to have a fat tail. Conceptually, the underlying mechanism is fairly straightforward. Structural uncertainty essentially means that the probabilities are unsure. A formal Bayesian translation might be that the structural parameters of the relevant PDFs are themselves uncertain and have their own PDFs. Weitzman (2009a) expressed this idea in a formal argument that the reduced form “posterior predictive” PDF (in Bayesian jargon) of $\ln D$ tends to be fat tailed because the structural parameters are unknown. Loosely speaking, the driving mechanism is that the operation of taking “expectations of expectations” or “probability distributions

¹²There is some wiggle room in the definition of what constitutes a fat-tailed PDF or a thin-tailed PDF, but everyone agrees that probabilities declining exponentially or faster (like the Normal) are thin tailed, while probabilities declining polynomially or slower (like the Pareto) are fat tailed. The standard example of a fat-tailed PDF is the power law (aka Pareto aka inverted polynomial) distribution, although, for example, a Student- t or inverted-gamma PDF is also fat-tailed. A normal or a gamma are examples of thin-tailed PDFs, as is any PDF having finite supports, like a uniform distribution or a discrete-point finite distribution. Although both PDFs must approach a limit of zero, the ratio of a fat-tailed probability divided by a thin-tailed probability goes to infinity in the limit.

of probability distributions” tends to spread apart and fatten the tails of the compounded posterior-predictive PDF. From past samples alone, it is inherently difficult to learn enough about the probabilities of extreme future events to thin down the “bad” tail of the PDF, because we don’t have much data about analogous past extreme events. This mechanism provides at least some kind of a generic story about why fat tails might be inherent in some situations.

The part of the distribution of possible future outcomes that we might know now (from inductive information of a form conveyed by past data) concerns the relatively more likely outcomes in the middle of the probability distribution. From past observations, plausible interpolations or extrapolations, and the law of large numbers, there may be at least some modicum of confidence in being able to construct a reasonable picture of the central regions of the posterior-predictive PDF. As we move towards probabilities in the periphery of the distribution, however, we are increasingly moving into the unknown territory of subjective uncertainty, where our probability estimates of the probability distributions themselves become increasingly diffuse because the frequencies of rare events in the tails cannot be pinned down by previous experiences. From past data alone, it is not possible to know enough now about the frequencies of future extreme tail events to make the outcomes of a BCA be independent from artificially-imposed limitations on the extent of possibly ruinous disasters. Climate-change economics generally, and the fatness of climate-change tails specifically, are prototypical examples of this principle, because we are trying to extrapolate inductive knowledge far outside the range of limited past experience. To put a sharp point on this seemingly abstract issue, the thin-tailed PDFs that implicitly support gradualist conclusions have at least some theoretical tendency to morph into fat-tailed PDFs when it is admitted that we are unsure about the functional forms or structural parameters behind these implicitly assumed thin-tailed PDFs – at least where high temperatures are concerned.

A fat upper tail of the PDF of $\ln D$ makes the willingness to pay (WTP) to avoid extreme climate changes very large, indeed arbitrarily large if the coefficient of relative risk aversion is bounded above one. In Weitzman (2009a) I presented a formal argument within a specific mathematical structure, but this formal argument could have been embedded in alternative mathematical structures – with the same basic message. The particular formal argument I gave was in the form of what I called a “dismal theorem” (DT). In this particular formalization, the limiting expected stochastic discount factor is infinite (or, what I take to be equivalent for purposes here, the limiting WTP to avoid fat-tailed disasters constitutes all of output). Of course, in the real world WTP is not 100% of output. Presumably the PDF in the bad fat tail is thinned, or even truncated, perhaps from considerations akin to what lies behind the value of a statistical life (VSL) – after all, we would not pay an

infinite amount to eliminate the fat upper tail of climate-change catastrophes. Alas, in whatever way the bad fat tail is thinned or truncated, a climate-change BCA based upon it might remain sensitive to the details of the thinning or truncation mechanism, because the disutility of extreme climate change is “essentially” unbounded.¹³ Later I discuss the meaning of this potential lack of robustness in climate-change BCA and speculate on some of its actionable consequences.

3.1 Interpreting Infinity

Disagreements abound concerning how to interpret the infinity symbol that appears in the formulation of the DT. There is a natural tendency to scoff at economic models that yield infinite outcomes. This reaction is presumably inspired by the idea that infinity is a ridiculous result, and that, therefore, an economic model that yields an infinity symbol as an outcome is fundamentally misspecified, and thus dismissable. Critics cite examples to argue earnestly that expected disutility from climate change cannot actually be infinite, as if this were an indictment of the entire fat-tailed methodology. I believe that, in the particular case of climate change, the infinity symbol is trying to tell us something important. That is, the infinite limit in the DT is a formal mathematical way of saying that structural uncertainty in the form of fat tails is, at least in theory, capable of swamping the outcome of any BCA that disregards this uncertainty.

The key issue here is *not* a mathematically illegitimate use of an infinite limit in the DT. Infinity is a side-show that has unfortunately diverted attention from the main issue, which is non-robustness. It is easy to modify utility functions, to add on VSL-like restrictions, to truncate probability distributions arbitrarily, or to introduce ad hoc priors that cut off or otherwise severely dampen low values of welfare-equivalent consumption. Introducing any of these (or many other attenuating mechanisms) formally replaces the infinity symbol by some uncomfortably large, but finite, number. Unfortunately, removing the infinite limit in these or other ways does not eliminate the underlying problem because it then comes back to haunt us in the form of a WTP that is arbitrarily large in order to erase the structural uncertainty. Just how large the WTP is can depend greatly upon obscure details about how the upper tail of the PDF of $\ln D$ has been thinned or bounded.

Consider imposing VSL-like restrictions as a case in point of just how fuzzy an actual upper bound might be on the WTP to avoid planet-ruining climate change. Individuals make choices all the time that involve trading off some amount of monetary wealth for some change in the probability of exposure to fatal risks. The “value of a statistical

¹³There is “essentially” unlimited liability here because global stakeholders cannot short the planet as a hedge against catastrophic climate change.

life” (abbreviated VSL) is typically defined as the money premium ΔM a person would be willing to pay to avoid exposure to a tiny increased probability of death Δq , per increment of probability change. A large number of studies have been used to estimate the VSL. Some time period is chosen, over which period ΔM and Δq are measured, and then the ratio $VSL = \Delta M / \Delta q$ is calculated. Very rough VSL values have typically been estimated for the U.S. at about \$10 million.¹⁴ In the U.S., average per-capita income is \approx \$50,000 per year, so that the VSL represents some 200 years of income per unit change in mortality probability. In the case of climate change, GHG concentrations of 800 ppm of CO_{2e} in Table 2 imply that $\text{Prob}_{\mathbf{P}}[T \geq 10^\circ\text{C}] \approx 1/20$. Suppose for the sake of argument that $T \geq 10^\circ\text{C}$ represents something like the “death of humanity.” Then naively-calculated per-capita WTP to avoid this scenario is about ten years’ worth of income, a big number. The fact that this scenario would occur centuries from now, if it occurred at all, would lower this WTP. The fact that this event represents the “death of humanity,” as contrasted with the death of a single human, would raise this WTP, perhaps considerably. And so forth. This numerical example has many serious flaws, is easy to criticize, and is extraordinarily non-robust. But I think that it illustrates how difficult it can be in practice to place an upper bound on the WTP to avoid ruinous climate change.

Thus, although one can easily remove the infinity symbol from the DT, one cannot so easily “remove” the underlying substantive economic problem of extreme sensitivity to fat tails and the resulting conundrum of deciding policy under such circumstances. The overwhelming majority of real-world BCAs have thin upper tails in $\ln D$ from limited exposure to globally catastrophic risks. However, a few very important real-world situations have effectively *unlimited* exposure due to structural uncertainty about their potentially open-ended catastrophic reach. Climate change is unusual in potentially affecting the entire worldwide portfolio of utility by threatening to drive all of planetary welfare to disastrously low levels in the most extreme scenarios.

3.2 Some Implications of the DT for BCA and Policy

The “standard” BCA approach appears to offer a constructive ongoing scientific-economic research program for generating ever more precise outputs from ever more precise inputs. By contrast, my main message can seem off-putting because it can be painted as anti-scientific and anti-economic. Fat tails and the resulting limitations they impose on the ability of BCA to reach robust conclusions are frustrating for economists. After all, we make a living from plugging rough numbers into simple models and reaching specific conclusions (more or

¹⁴See, e.g., the numbers cited in Viscusi and Aldi (2003), updated into 2010 dollars.

less) on the basis of those numbers. What quantitative advice are we supposed to provide to policy makers and politicians about *how much* effort to spend on averting climate change if the conclusions from modeling fat-tailed uncertainties are not clear-cut? Practical men and women of action have a low tolerance for vagueness and crave some kind of an answer, so they have little patience for even a whiff of fuzziness from two-handed economists. It is threatening for us economists to admit that constructive “can do” climate-change BCA may be up against some basic limitations on the ability of quantitative analysis to yield robust policy advice. But if this is the way things are with the economics of climate change, then this is the way things are. Non-robustness to subjective assumptions about catastrophic outcomes is an inconvenient truth to be lived with rather than a fact to be denied or evaded just because it looks less scientifically objective in BCA. If this limits the ability to give fine-grained and concrete answers to an impatient public, then so be it.

BCA is valuable, even indispensable, as a disciplined framework for organizing information and keeping score. But all BCAs are not created equal. In rare situations with effectively unlimited downside liability, like climate change, BCAs can be fragile to specifications of extreme tail events. Perhaps economists need to emphasize more openly to the policy makers, the politicians, and the public that while formal BCA can be helpful, in the particular case of climate change there is a danger of possible overconfidence from undue reliance on subjective judgements about the probabilities and welfare impacts of extreme events. What we can do constructively as economists is to better explain both the magnitudes of the unprecedented structural uncertainties involved and why this feature limits what we can say, and then present the best BCAs and the most honest sensitivity analyses that we can under fat-tailed circumstances, including many different functional forms for extremes. At the end of the day, policy makers must decide what to do on the basis of an admittedly sketchy economic analysis of a gray area that just cannot be forced to render clear robust answers. The moral of the dismal theorem is that, under extreme tail uncertainty, seemingly casual decisions about functional forms, parameter values, and tail fatness can dominate BCA. Economists should not pursue a narrow, superficially crisp, analysis by blowing away the low-probability high-impact catastrophic scenarios as if this is a necessary price we must pay for the worthy goal of giving answers and advice to policy makers. An artificial infatuation with crispness is likely to make our analyses go seriously askew and undermine the credibility of what we have to offer by effectively marginalizing the very possibilities that make climate change so grave in the first place.

3.3 Role of Systemic Inertia and Learning

The issue of how to deal with the deep structural uncertainties in climate change would be completely different and immensely simpler if systemic inertias, like the time required for the system to naturally remove extra atmospheric CO₂, were short, as is the case for many airborne pollutants like sulfur dioxide, nitrous oxides, and particulates. Then an important component of an optimal strategy might be along the lines of “wait and see.” With strong reversibility, an optimal climate-change policy would logically involve (among other elements) waiting to learn how far out on the bad fat tail the planet will end up, followed by midcourse corrections if we seem to be headed for a disaster. Alas, the problem of climate change seems bedeviled at almost every turn by significant stock-accumulation inertias – in atmospheric CO₂, in the absorption of heat or CO₂ by the oceans, and in many other relevant physical and biological processes – that are slow to respond to attempts at reversal.

Take atmospheric CO₂ as a prime example. Solomon et al [2009] calculated how concentrations of CO₂ would be expected to fall off over time if *all* anthropogenic emissions were to cease immediately, following a future 2% annual growth rate of emissions up to peak concentrations of 450, 550, 650, 750, 850 and 1,200 ppm. As the authors state: “The example of a sudden cessation of emissions provides an upper bound to how much reversibility is possible, if, for example, unexpectedly damaging climate changes were to be observed.” Results differed for different trajectories and scenarios, but a crude rule of thumb seemed to be that approximately 70% of the peak concentration level over the preindustrial level of 280 ppm persevered after 100 years of *zero* emissions, while approximately 40% of the peak concentration level over the preindustrial level of 280 ppm persevered after 1,000 years of *zero* emissions. Were atmospheric CO₂ concentrations to peak at 800 ppm, followed forever thereafter by *zero* emissions, then atmospheric concentrations would be ≈ 650 ppm after 100 years and ≈ 500 ppm after 1,000 years. A different recent study by Archer et al [2009] reached essentially the same conclusions. These numbers do not look to me like evidence supporting “wait and see” policies. The capacity of the oceans to take up atmospheric heat, the saturation of carbon sinks, the loss of albedo, and many, many other relevant mechanisms, tell a similar story of long stock-accumulation irreversibilities relative to the time it takes to filter out and act upon meaningful signals of impending disasters.

3.4 Alternative Specifications and Catastrophic Outcomes

Many researchers promote alternative specifications that imply outcomes that are much less extreme than those implied by my specifications. I am not arguing that these alternative

formulations are wrong or even implausible. I am merely pointing out that they are not likely to be robust with respect to assumptions about extreme catastrophic climate change and that they therefore fail an important “stress test.” Of course the reader should weigh the plausibility of the arguments and the reasonableness of the various specifications on their own merits. But it is difficult to form opinions about probabilities of climate-change extremes, or about disutility functions for extreme temperatures, or about lots of other things that are relevant for deciding the tail fatness of the PDF of $\ln D$. Suppose, for the sake of argument, that a policy maker believes the probability is 50% that my fat-tailed specification is correct and 50% that the thin-tailed specification of someone else is correct. Then, other things being equal, rational policy should lean more in the direction of my fat-tailed conclusions than in the direction of someone else’s thin-tailed conclusions because of the highly asymmetric consequences of fat tails vs. thin tails. In this sense, whether it is fair or unfair, the playing field is not level between me and someone else. To further illustrate this point, suppose one person advises you that a fire insurance policy protecting your house against extreme losses is unnecessary because so few houses of your kind burn to the ground, while another person advises you that a complete fire insurance policy is necessary in your case. Other things being equal, should you flip a coin to decide what to do just because both advisers seem to be giving equally credible guidance?

Climate change is not the only possible catastrophic threat to humanity. In Weitzman (2009a) I listed what I consider to be the half-dozen or so serious contenders with climate change for potentially catastrophic global impacts with non-negligible probabilities: biotechnology, nanotechnology, asteroids, “strangelets,” pandemics, runaway rogue computers, nuclear proliferation – and went on to give a few tentative reasons why I think that climate change is especially worrisome. It may well be that each of the other half-dozen or so serious candidates for fat-tailed disasters deserves its own ballpark estimate of tail probabilities along with extremely crude calculations of policy implications, which is about the best we can do with potential catastrophes. Even if this were true, however, it would not lessen the need to reckon with the strong potential implications of fat tails for BCA-like calculations in the particular case of climate change.

4 Conclusion

Taking fat tails into account has implications for climate-change research and policy. Perhaps more research is needed about the extreme tails of relevant PDFs relative to more research about central tendencies. The fatness of the bad fat tail of proposed solutions (such as, perhaps, the possibility that buried CO₂ might escape) needs to be weighed against the

fatness of the tail of the climate-change problem itself. With fat tails we might want more explicit contingency planning for bad outcomes, including, perhaps, a niche role for last-resort portfolio options like geoengineering. And so forth.

Qualitatively, fat tails favor more aggressive policies to lower GHGs than the “standard” BCA. Alas, the quantitative implications are less clear. As this article has stressed, the natural consequence of fat-tailed uncertainty should be to make economists less confident about climate-change BCA and to make them adopt a more modest tone that befits less robust policy advice. My own conclusion is that the sheer magnitude of the deep structural uncertainties concerning catastrophic outcomes, and the way we express this in our models, is likely to influence plausible applications of BCA to the economics of climate change now and for some time to come.

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