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ON MODELING AND INTERPRETING THE ECONOMICS OF CATASTROPHIC CLIMATE CHANGE

Martin L. Weitzman*

Abstract—With climate change as prototype example, this paper analyzes the implications of structural uncertainty for the economics of low-probability, high-impact catastrophes. Even when updated by Bayesian learning, uncertain structural parameters induce a critical "tail fattening" of posterior-predictive distributions. Such fattened tails have strong implications for situations, like climate change, where a catastrophe is theoretically possible because prior knowledge cannot place sufficiently narrow bounds on overall damages. This paper shows that the economic consequences of fat-tailed structural uncertainty (along with unsureness about high-temperature damages) can readily outweigh the effects of discounting in climate-change policy analysis.

I. Introduction

WHAT is the essence of the economic problem posed by climate change? The economic uniqueness of the climate-change problem is not just that today's decisions have difficult-to-reverse impacts that will be felt very far out into the future, thereby straining the concept of time discounting and placing a heavy burden on the choice of an interest rate. Nor does uniqueness come from the unsure outcome of a stochastic process with known structure and known objective-frequency probabilities. Much more unsettling for an application of (present discounted) expected utility analysis are the unknowns: deep structural uncertainty in the science coupled with an economic inability to evaluate meaningfully the catastrophic losses from disastrous temperature changes. The climate science seems to be saying that the probability of a disastrous collapse of planetary welfare is nonnegligible, even if this tiny probability is not objectively knowable. Motivated by the climate-change example, this paper presents a mathematically rigorous (but abstract) economic-statistical model of high-impact, low-probability catastrophes. It also presents some less rigorous numerical calculations suggesting the empirical importance for climate-change analysis of the surprisingly strong theoretical result from the abstract model. The least rigorous part of the paper concludes with some speculative (but, I think, necessary) thoughts about what this all means for climate-change policy.

The next section argues that, were one forced to specify a "best guess" estimate of the extreme bad tail of the relevant probability density function (PDF) of what might eventually happen if only gradually ramped-up remedies are applied, then mean global surface temperature change relative to pre-industrial-revolution levels will in two centuries or so be greater than 10°C with a ballpark probability estimate somewhere around 0.05 and will be greater than 20°C with a ballpark probability estimate somewhere around 0.01. Societies and ecosystems in a world whose average temperature has changed in the geologically instantaneous time of two centuries or so by 10°C–20°C (for U.S. readers: a change of 10°C = a change of 18°F and a change of 20°C = a change of 36°F) are located in *terra incognita,* since such high temperatures have not existed for hundreds of millions of years and such a rate of global temperature change might be unprecedented even on a timescale of billions of years. However measured, the planetary welfare effect of climate changes that might accompany mean temperature increases from 10°C up to 20°C with probabilities anything remotely resembling 5% down to 1% implies a nonnegligible probability of worldwide catastrophe. The paper suggests that the shock value of this kind of numerical example may not be accidental. Rather, it might stem from a deeply rooted theoretical principle—thereby delivering a combined theoretical-empirical punch that is particularly potent for climate-change analysis.

In his book *Catastrophe: Risk and Response,*[1] Richard Posner defines the word "catastrophe" "to designate an event that is believed to have a very low probability of materializing but that if it does materialize will produce a harm so great and sudden as to seem discontinuous with the flow of events that preceded it." Posner adds: "The low probability of such disasters—frequently the *unknown* probability, as in the case of bioterrorism and abrupt global warming—is among the things that baffle efforts at responding rationally to them." In this paper I address what a

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* Department of Economics, Harvard University.

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[1] Posner (2004). See also the insightful review by Parson (2007). Sunstein (2007) covers some similar themes more analytically and from a somewhat different perspective.

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rational economic response in the discipline-imposing form of (present discounted) expected utility theory might offer by way of guidance for thinking coherently about the economics of uncertain catastrophes with tiny but highly unknown probabilities.

Modeling uncertain catastrophes presents some *very* strong challenges to economic analysis, the full implications of which have not yet been adequately confronted. Cost-benefit analysis (CBA) based on expected utility (EU) theory has been applied in practice primarily to cope with uncertainty in the form of a known thin-tailed PDF. This paper shows that there is a rigorous sense in which the relevant posterior-predictive PDF of high-impact, low-probability catastrophes has a built-in tendency to be fat tailed. A fat-tailed PDF assigns a *relatively* much higher probability to rare events in the extreme tails than does a thin-tailed PDF.[2] (Even though both limiting probabilities are infinitesimal, the *ratio* of a thick-tailed probability divided by a thin-tailed probability approaches infinity in the limit.) Not much thought has gone into conceptualizing or modeling what happens to EU-based CBA for fat-tailed disasters. A CBA of a situation with known thin tails, even including whatever elements of subjective arbitrariness it might otherwise contain, can at least in principle make comforting statements of the generic form: "If the PDF tails are cut off here, then EU theory will still capture and convey an accurate approximation of what is important." Such accuracy-of-approximation PDF-tail-cutoff statements, alas, do not exist in this generic sense for what in this paper I am calling "fat-tailed CBA."

Fat-tailed CBA has strong implications that have been neither recognized in the literature nor incorporated into formal CBA modeling of disasters like climate-change catastrophes. These implications raise many disturbing yet important questions, which will be dealt with somewhat speculatively in the concluding sections of this paper. Partially answered questions and speculative thoughts aside, I contend it is nevertheless undeniable that, at least in principle, fat-tailed CBA can turn conventional thin-tail-based climate-change policy advice on its head. This paper shows that it is quite possible, and even numerically plausible, that the answers to the big policy question of what to do about climate change stand or fall to a large extent on the issue of how the high-temperature damages and tail probabilities are conceptualized and modeled. By implication, the policy advice coming out of conventional thin-tailed CBAs of climate change must be treated with skepticism until this low-probability, high-impact aspect is addressed seriously and included empirically in a true fat-tailed CBA.

Standard approaches to modeling the economics of climate change (even those that purport to treat risk by Monte Carlo simulations) very likely fail to account adequately for the implications of large impacts with small probabilities. From inductive experience alone, one cannot acquire sufficiently accurate information about the probabilities of extreme tail disasters to prevent the expected marginal utility of an extra unit of consumption from becoming infinite for any utility function with relative risk aversion everywhere bounded above 0. To close the model in the sense of making expected marginal utility be below $+\infty$ (or expected utility above $-\infty$), the paper relies on a concept akin to the "value of statistical life" (VSL)—except that here it represents something more like the rate of substitution between consumption and the mortality risk of a catastrophic extinction of civilization or the natural world as we know these concepts. With this way of closing the model (which, I will argue, is at least better than the alternatives), subsequent EU-based CBA then depends critically upon an exogenously imposed VSL-like parameter that is a generalization of the value of a statistical human life and is presumably *very* big. Practically, a high VSL-like parameter means for open-ended situations with potentially unlimited downside exposure (like climate change) that a Monte Carlo simulation must go *very* deep into the extreme-negative-impact fat tail to merit credibility as an accurate and fair CBA. In this sense (by making there be such utter dependence upon a concept like the value of a statistical life, which might be *very* big), structural or deep uncertainty is potentially much more of a driving force than discounting or pure risk. For situations where there do not exist prior limits on damages (like climate change from greenhouse warming), CBA is likely to be dominated by considerations and concepts related more to catastrophe insurance than to the consumption smoothing consequences of long-term discounting—even at empirically plausible interest rates.

II. Generalized Climate Sensitivity as a Scaling Factor

The broad thesis of this paper is that PDF tails fattened by structural uncertainty can have a big effect on CBA. The specific example I use to illustrate this thesis is a critical scale parameter that multiplies or amplifies an exogenous shock or perturbation to the system. The purpose of this section is to motivate heuristically, and to derive some extremely crude ballpark numerical estimates for the tail PDF of, this kind of scaling-transfer factor in a context of climate change. Very roughly—at a *very* high level of abstraction and without trying to push an imperfect analogy too far—the generic role of this uncertain multiplicative amplifier or scale parameter might perhaps be illustrated by the role of an uncertain "climate sensitivity" coefficient in climate-change models and discussions of global warming.

[2] As I use the term in this paper a PDF has a "fat" (or "thick" or "heavy") tail when its moment generating function (MGF) is infinite—that is, the tail probability approaches 0 more slowly than exponentially. The standard example of a fat-tailed PDF is the power law (aka polynomial aka Pareto) distribution, although, for example, a log normal PDF is also fat tailed, as is an inverted-normal or inverted-gamma. By this definition a PDF whose MGF is finite has a "thin" tail. A normal or a gamma are examples of thin-tailed PDFs, as is *any* PDF having finite supports. As shown later, the welfare significance of fat versus thin tails comes via a tight connection between the CRRA EU of consumption and the MGF of consumption growth.

Climate sensitivity is a key macro-indicator of the eventual temperature response to greenhouse gas (GHG) changes. Let $\Delta \ln CO_2$ be sustained relative change in atmospheric carbon dioxide while ΔT is equilibrium temperature response. Narrowly defined, climate sensitivity (here denoted S_1) converts $\Delta \ln CO_2$ into ΔT by the formula $\Delta T \approx (S_1/\ln 2) \times \Delta \ln CO_2$. As the Intergovernmental Panel on Climate Change in its IPCC-AR4 (2007) executive summary puts it: "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°C 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." Climate sensitivity is not the same as temperature change, but for the benchmark-serving purposes of my simplistic example I assume the shapes of both PDFs are roughly similar after approximately 200 years because a doubling of anthropogenically injected CO_2-equivalent (CO_2-e) GHGs relative to pre-industrial-revolution levels is essentially unavoidable within about the next 40 years and will plausibly remain well above two times preindustrial levels for at least 100 or more years thereafter.

In this paper I am mostly concerned with the roughly 15% of those S_1 "values substantially higher than 4.5°C" which "cannot be excluded." A grand total of 22 peer-reviewed studies of climate sensitivity published recently in reputable scientific journals and encompassing a wide variety of methodologies (along with 22 imputed PDFs of S_1) lie indirectly behind the above-quoted IPCC-AR4 (2007) summary statement. These 22 recent scientific studies cited by IPCC-AR4 are compiled in table 9.3 and box 10.2. It might be argued that these 22 studies are of uneven reliability and their complicatedly related PDFs cannot easily be combined, but for the simplistic purposes of this illustrative example I do not perform any kind of formal Bayesian model-averaging or meta-analysis (or even engage in informal cherry picking). Instead I just naively assume that all 22 studies have equal credibility and for my purposes here their PDFs can be simplistically aggregated. The upper 5% probability level averaged over all 22 climate-sensitivity studies cited in IPCC-AR4 (2007) is 7°C while the median is 6.4°C,[3] which I take as signifying approximately that $P[S_1 > 7°C] \approx 5\%$. Glancing at table 9.3 and box 10.2 of IPCC-AR4, it is apparent that the upper tails of these 22 PDFs tend to be sufficiently long and fat that one is allowed from a simplistically aggregated PDF of these 22 studies the rough approximation $P[S_1 > 10°C] \approx 1\%$. The actual empirical reason why these upper tails are long and fat dovetails beautifully with the theory of this paper: 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 which case one is forced back onto depending more than one might wish upon the prior PDF, which of necessity is largely subjective and relatively diffuse. As a recent *Science* commentary put it: "Once the world has warmed by 4°C, conditions will be so different from anything we can observe today (and still more different from the last ice age) that it is inherently hard to say where the warming will stop."[4]

A significant supplementary component, which conceptually should be added on to climate-sensitivity S_1, is the powerful self-amplification potential of greenhouse warming due to heat-induced releases of the immense volume of GHGs currently sequestered in arctic permafrost and other boggy soils (mostly as methane, CH_4, a particularly potent GHG). A yet more remote possibility, which in principle should also be included, is heat-induced releases of the even-vaster offshore deposits of CH_4 trapped in the form of hydrates (clathrates)—for which there is a decidedly non-zero probability of destabilized methane seeping into the atmosphere if water temperatures over the continental shelves warm just slightly. Such CH_4-outgassing processes could potentially precipitate (over the long run) a cataclysmic runaway-positive-feedback warming. The very real possibility of endogenous heat-triggered releases at high temperatures of the enormous amounts of naturally sequestered GHGs is a good example of indirect carbon-cycle feedback-forcing effects that I would want to include in the abstract interpretation of a concept of "climate sensitivity" that is relevant for this paper. What matters for the econom-

[3] Details of this calculation are available upon request. Eleven of the studies in table 9.3 overlap with the studies portrayed in box 10.2. Four of these overlapping studies conflict on the numbers given for the upper 5% level. For three of these differences I chose the table 9.3 values on the grounds that all of the box 10.2 values had been modified from the original studies to make them have zero probability mass above 10°C. (The fact that all PDFs in box 10.2 have been normalized to zero probability above 10°C biases my upper-5% averages here toward the low side.) With the fourth conflict (Gregory et al., 2002a), I substituted 8.2°C from box 10.2 for the ∞ in table 9.3 (which arises only because the method of the study itself does not impose any meaningful upper-bound constraint). The only other modification was to average the three reported volcanic-forcing values of Wigley et al. (2005a) in table 9.3 into one upper-5% value of 6.4°C.

[4] Allen and Frame (2007). Let ΔR_f stand for changes in equilibrium "radiative forcing" that eventually induce (approximately) linear temperature equilibrium responses ΔT. The most relevant radiative forcing for climate change is $\Delta R_f = \Delta \ln CO_2$, but there are many other examples of radiative forcing, such as changes in aerosols, particulates, ozone, solar radiation, volcanic activity, other GHGs, and so on. Attempts to identify S_1 in the 22 studies cited in IPCC-AR4 are roughly akin to observing $\Delta T/\Delta R_f$ for various values of ΔR_f and subsequent ΔT. The problem is the presence of significant uncertainties both in empirical measurements and in the not directly observable coefficients plugged into simulation models. This produces a long fat upper tail in the inferred posterior-predictive PDF of S_1. Many physically possible tail-fattening mechanisms might be involved. A recent *Science* article by Roe and Baker (2007) relies on the idea that Gaussian g_1 produces a fat tail in the PDF of $S_1 = 1.2/(1 - g_1)$. I believe that all such thickening mechanisms ultimately trace back to the common theme of this paper that it is difficult to infer (or even to model accurately) the probabilities of events far outside the usual range of experience—which effectively causes the reduced-form posterior-predictive PDF of these rare events to have a fat tail.

ics of climate change is the reduced-form relationship between atmospheric stocks of anthropogenically injected CO_2-e GHGs and temperature change. Instead of S_1, which stands for "climate sensitivity narrowly defined," I work throughout the rest of this paper with S_2, which (abusing scientific terminology somewhat here) stands for a more abstract "generalized climate-sensitivity-like scaling parameter" that includes heat-induced feedbacks on the forcing from the above-mentioned releases of naturally sequestered GHGs, increased respiration of soil microbes, climate-stressed forests, and other weakenings of natural carbon sinks. The transfer from Δ ln [anthropogenically injected CO_2-e GHGs] to eventual ΔT is not linear (and is not even a true long-run equilibrium relationship), but for the purposes of this highly aggregated example the linear approximation is good enough. This suggests that a doubling of anthropogenically injected CO_2-e GHGs causes (very approximately) ultimate temperature change $\Delta T \approx S_2$.

The main point here is that the PDF of S_2 has an even-longer, even-fatter tail than the PDF of S_1. A recent study by Torn and Harte (2006) can be used to give some very rough idea of the relationship of the PDF of S_2 to the PDF of S_1. It is universally accepted that in the absence of any feedback gain, $S_1 = 1.2°C$. If g_1 is the conventional feedback gain parameter associated with S_1, then $S_1 = 1.2/[1 - g_1]$, whose inverse is $g_1 = [S_1 - 1.2]/S_1$. Torn and Harte estimated that heat-induced GHG releases add about 0.067 of gain to the conventional feedback factor, so that (expressed in my language) $S_2 = 1.2/[1 - g_2]$, where $g_2 = g_1 + 0.067$. (The 0.067 is only an estimate in a linearized formula, but it is unclear in which direction higher-order terms would pull the formula, and even if this 0.067 coefficient were considerably lower my point would remain.) Doing the calculations, $P[S_1 > 7°C] = 5\% = P[g_1 > 0.828] = P[g_2 > 0.895]$ implies $P[S_2 > 11.5°C] = 5\%$. Likewise, $P[S_1 > 10°C] = 1\% = P[g_1 > 0.88] = P[g_2 > 0.947]$ implies $P[S_2 > 22.6°C] = 1\%$ and presumably corresponds to a scenario where CH_4 and CO_2 are outgassed on a large scale from degraded permafrost soils, wetlands, and clathrates.[5] The effect of heat-induced GHG releases on the PDF of S_2 is *extremely* nonlinear at the upper end of the PDF of S_2 because, so to speak, "fat tails conjoined with fat tails beget yet-fatter tails."

[5] I am grateful to John Harte for guiding me through these calculations, although he should not be blamed for how I am interpreting or using the numbers in what follows. The Torn and Harte study is based upon an examination of the 420,000-year record from Antarctic ice cores of temperatures along with associated levels of CO_2 and CH_4. While based on different data and a different methodology, the study of Sheffer, Brovkin, and Cox (2006) supports essentially the same conclusions as Torn and Harte (2006). A completely independent study from simulating an interactive coupled climate-carbon model of intermediate complexity in Matthews and Keith (2007) confirms the existence of a strong carbon-cycle feedback effect with especially powerful temperature amplifications at high climate sensitivities.

Of course my calculations and the numbers above can be criticized, but (quibbles and terminology aside) I don't think climate scientists would say these calculations are fundamentally wrong in principle or there exists a clearly superior method for generating rough estimates of extreme-impact tail probabilities. Without further ado I just assume for purposes of this simplistic example that $P[S_2 > 10°C] \approx 5\%$ and $P[S_2 > 20°C] \approx 1\%$, implying that anthropogenic doubling of CO_2-e eventually causes $P[\Delta T > 10°C] \approx 5\%$ and $P[\Delta T > 20°C] \approx 1\%$, which I take as my base-case tail estimates in what follows. These small probabilities of what amounts to huge climate impacts occurring at some indefinite time in the remote future are wildly uncertain, unbelievably crude ballpark estimates—most definitely *not* based on hard science. But the subject matter of this paper concerns just such kind of situations and my overly simplistic example here does not depend at all on precise numbers or specifications. To the contrary, the major point of this paper is that such numbers and specifications *must* be imprecise and that *this* is a significant part of the climate-change economic-analysis problem, whose strong implications have thus far been ignored.

Stabilizing anthropogenically injected CO_2-e GHG stocks at anything like twice pre-industrial-revolution levels looks now like an extremely ambitious goal. Given current trends in emissions, we will attain such a doubling of anthropogenically injected CO_2-e GHG levels around the middle of this century and will then go far beyond that amount unless drastic measures are taken starting soon. Projecting current trends in business-as-usual GHG emissions, a *tripling* of anthropogenically injected CO_2-e GHG concentrations would be attained relative to pre-industrial-revolution levels by early in the 22nd century. Countering this effect is the idea that we just might begin someday to seriously cut back on GHG emissions (especially if we learn that a high-S_2 catastrophe is looming—although the extraordinarily long inertial lags in the commitment pipeline converting GHG emissions into temperature increases might severely limit this option). On the other hand, maybe currently underdeveloped countries like China and India will develop and industrialize at a blistering pace in the future with even more GHG emissions and even less GHG emissions controls than have thus far been projected. Or, who knows, we might someday discover a revolutionary new carbon-free energy source or make a carbon-fixing technological breakthrough. Perhaps natural carbon-sink sequestration processes will turn out to be weaker (or stronger) than we thought. There is also the unknown role of climate engineering. The recent scientific studies behind my crude ballpark numbers could turn out to too optimistic or too pessimistic—or I might simply be misapplying these numbers by inappropriately using values that are either too high or too low. And so forth and so on. For the purposes of this very crude example (aimed at conveying some very rough empirical sense of the fatness of global-warming tails), I cut

through the overwhelming enormity of climate-change uncertainty and the lack of hard science about tail probabilities by sticking with the overly simplistic story that $P[S_2 > 10°C] \approx P[\Delta T > 10°C] \approx 5\%$ and $P[S_2 > 20°C] \approx P[\Delta T > 20°C] \approx 1\%$. I can't know precisely what these tail probabilities are, of course, but no one can—and *that* is the point here. To paraphrase again the overarching theme of this example: the moral of the story does not depend on the exact numbers or specifications in this drastic oversimplification, and if anything it is enhanced by the fantastic uncertainty of such estimates.

It is difficult to imagine what $\Delta T \approx 10°C–20°C$ might mean for life on Earth, but such high temperatures have not been seen for hundreds of millions of years and such a rate of change over a few centuries would be unprecedented even on a timescale of billions of years. Global average warming of 10°C–20°C masks tremendous local and seasonal variation, which can be expected to produce temperature increases *much* greater than this at particular times in particular places. Because these hypothetical temperature changes would be geologically instantaneous, they would effectively destroy planet Earth as we know it. At a minimum such temperatures would trigger mass species extinctions and biosphere ecosystem disintegration matching or exceeding the immense planetary die-offs associated in Earth's history with a handful of previous geoenvironmental mega-catastrophes. There exist some truly terrifying consequences of mean temperature increases ≈10°C–20°C, such as disintegration of Greenland's and at least the western part of the Antarctic's ice sheets with dramatic raising of sea level by perhaps thirty meters or so, critically important changes in ocean heat transport systems associated with thermohaline circulations, complete disruption of weather, moisture and precipitation patterns at every planetary scale, highly consequential geographic changes in freshwater availability, and regional desertification.

All of the above-mentioned horrifying examples of climate-change mega-disasters are incontrovertibly possible on a timescale of centuries. They were purposely selected to come across as being especially lurid in order to drive home a valid point. The tiny probabilities of nightmare impacts of climate change are all such crude ballpark estimates (and they would occur so far in the future) that there is a tendency in the literature to dismiss altogether these highly uncertain forecasts on the "scientific" grounds that they are much too speculative to be taken seriously. In a classical-frequentist mindset, the tiny probabilities of nightmare catastrophes are so close to 0 that they are highly statistically insignificant at any standard confidence level, and one's first impulse can understandably be to just ignore them or wait for them to become more precise. The main theme of this paper contrasts sharply with the conventional wisdom of *not* taking seriously extreme-temperature-change probabilities *because* such probability estimates aren't based on hard science and are statistically insignificant. This paper shows that the exact opposite logic holds by giving a rigorous Bayesian sense in which, other things being equal, the more speculative and fuzzy are the tiny tail probabilities of extreme events, the less ignorable and the more serious is the impact on present discounted expected utility for a risk-averse agent.

Oversimplifying enormously here, how warm the climate ultimately gets is approximately a product of two factors—anthropogenically injected CO_2-e GHGs and a critical climate-sensitivity-like scaling multiplier. Both factors are uncertain, but the scaling parameter is more open-ended on the high side with a much longer and fatter upper tail. This critical scale parameter reflecting huge scientific uncertainty is then used as a multiplier for converting aggregated GHG emissions—an input mostly reflecting economic uncertainty—into eventual temperature changes. Suppose the true value of this scaling parameter is unknown because of limited past experience, a situation that can be modeled as if inferences must be made inductively from a finite number of data observations. At a sufficiently high level of abstraction, each data point might be interpreted as representing an outcome from a particular scientific or economic study. This paper shows that having an uncertain scale parameter in such a setup can add a significant tail-fattening effect to posterior-predictive PDFs, even when Bayesian learning takes place with arbitrarily large (but finite) amounts of data. Loosely speaking, the driving mechanism is that the operation of taking "expectations of expectations" or "probability distributions of probability distributions" spreads apart and fattens the tails of the reduced-form compounded posterior-predictive PDF. It is inherently difficult to learn from finite samples alone enough about the probabilities of extreme events to thin down the bad tail of the PDF because, by definition, we don't get many data-point observations of such catastrophes. The paper will show that a generalization of this form of interaction can be repackaged and analyzed at an even higher level of abstraction as an aggregative macroeconomic model with essentially the same reduced form (structural uncertainty about some unknown open-ended scaling parameter amplifying an uncertain economic input). This form of interaction (coupled with finite data, under conditions of everywhere-positive relative risk aversion) can have very strong consequences for CBA when catastrophes are theoretically possible, because in such circumstances it can drive applications of EU theory much more than anything else, including discounting.

When fed into an economic analysis, the great open-ended uncertainty about eventual mean planetary temperature change cascades into yet much greater, yet much more open-ended uncertainty about eventual changes in welfare. There exists here a very long chain of tenuous inferences fraught with huge uncertainties in every link beginning with unknown base-case GHG emissions; then compounded by huge uncertainties about how available policies and policy

levers transfer into actual GHG emissions; compounded by huge uncertainties about how GHG-flow emissions accumulate via the carbon cycle into GHG-stock concentrations; compounded by huge uncertainties about how and when GHG-stock concentrations translate into global mean temperature changes; compounded by huge uncertainties about how global mean temperature changes decompose into regional temperature and climate changes; compounded by huge uncertainties about how adaptations to, and mitigations of, climate-change damages are translated into utility changes—especially at a regional level; compounded by huge uncertainties about how future regional utility changes are aggregated—and then how they are discounted—to convert everything into expected-present-value global welfare changes. The result of this immense cascading of huge uncertainties is a reduced form of truly stupendous uncertainty about the aggregate expected-present-discounted utility impacts of catastrophic climate change, which mathematically is represented by a very spread out, very fat-tailed PDF of what might be called "welfare sensitivity."

Even if a generalized climate-sensitivity-like scaling parameter such as S_2 could be bounded above by some big number, the value of "welfare sensitivity" is effectively bounded only by some *very* big number representing something like the value of statistical civilization as we know it or maybe even the value of statistical life on Earth as we know it. This is the essential point of this simplistic motivating example. Suppose it were granted for the sake of argument that an abstract climate-sensitivity-like scaling parameter such as S_2 might somehow be constrained at the upper end by some fundamental law of physics that assigns a probability of *exactly* 0 to temperature change being above some critical physical constant instead of continuously higher temperatures occurring with continuously lower probabilities trailing off asymptotically to 0. Even granted such an upper bound on S_2, the essential point here is that the enormous unsureness about (and enormous sensitivity of CBA to) an arbitrarily imposed "damages function" for high temperature changes makes the relevant reduced-form criterion of welfare sensitivity to a fat-tailed generalized scaling parameter seem almost unbelievably uncertain at high temperatures—to the point of being essentially unbounded for practical purposes.

III. The Model

Let C be reduced-form consumption that has been adjusted for welfare by subtracting out all damages from climate change. Adaptation and mitigation are considered to be already included in C. Present consumption is normalized as $C_0 \equiv 1$. Suppose to begin with that the representative agent has a standard familiar utility function of CRRA (constant relative risk aversion) form

$$U(C) = \frac{C^{1-\eta}}{1-\eta} \tag{1}$$

with coefficient η. Marginal utility is $U'(C) = C^{-\eta}$. Later I consider non-CRRA utility.

For analytical crispness, the model of this paper has only two periods—the present and the future. Applied to climate change, I interpret the future as being very roughly about two centuries hence. By using such a sharp formulation I downplay the ability to learn and adapt gradually over time. Likewise I repress the fact that higher ΔT values are correlated with later times of arrival. I argue subsequently in the paper that key insights of this model will remain, *mutatis mutandis,* when additional real-world complexities are layered on—including a more detailed specification of the economics of climate change that incorporates learning along with a realistically long inertial time lag from emitted GHGs to eventual ΔT. The main purpose of this paper is to lay out the essential structure of my argument as simply as possible, leaving more realistic refinements for later work.

Instead of working directly with future damages-adjusted consumption C, in this paper it is more convenient to work with (and think in terms of) $\ln C$. If present consumption is normalized to unity, then the growth of consumption between the two periods is

$$Y \equiv \ln C, \tag{2}$$

where in this model Y is a random variable (RV) capturing *all* uncertainty that influences future values of $\ln C$, including damages of adverse climate change. Throughout this paper, Y encapsulates the reduced-form uncertainty that is at the abstract core of an economic analysis of climate change: the relationship between uncertain post-damages welfare-adjusted C and uncertain ΔT in the background. Thus, the RV Y is to be interpreted as implicitly being some transfer function of the RV ΔT of form $Y = F(\Delta T)$, so that equation (2) means $C = \exp(F(\Delta T))$. For simplicity, in this paper I effectively take $F(\Delta T)$ to be of the linear form $F(\Delta T) = G - \gamma \Delta T$ with known positive constants G and γ, but it could be of the quadratic form $F(\Delta T) = G - \gamma(\Delta T)^2$ or of many other forms. The essence of the structural-uncertainty problem in the economics of climate change concerns the process by which we come to understand underlying structure. Here one requires a model of how inductive knowledge is acquired. This core issue is modeled starkly at a very high reduced-form level of abstraction. I simply pretend the inference mechanism is *as if* we learn the indirect effect of ΔT on C via direct observations of past realizations of Y, which are subsequently incorporated into a Bayesian-updated reduced-form posterior-predictive PDF of Y.

With time-preference parameter β ($0 < \beta \leq 1$), the "stochastic discount factor" or "pricing kernel" is

$$M(C) = \beta \frac{U'(C)}{U'(1)} = \beta \exp(-\eta Y). \tag{3}$$

The amount of present consumption the agent would be willing to give up in the present period to obtain one extra

sure unit of consumption in the future period is $E[M] = \beta E[\exp(-\eta Y)]$, which is a kind of shadow price for discounting future costs and benefits in project analysis. Throughout the paper I use this price of a future sure unit of consumption $E[M]$ as the single most useful overall indicator of the present cost of future uncertainty. Other like indicators—such as welfare-equivalent deterministic consumption or willingness to pay to avoid uncertainty—give similar results, but the required analysis in terms of mean-preserving spreads and so forth is slightly more elaborate and slightly less intuitive. Focusing on the behavior of $E[M]$ is understood in this paper, therefore, as being a metaphor for understanding what drives the results of *all* utility-based welfare calculations in situations of potentially unlimited exposure to catastrophic impacts.

Using standard notation, let lowercase y denote a realization of the uppercase RV Y. If Y has PDF $f(y)$, then

$$E[M] = \beta \int_{-\infty}^{\infty} e^{-\eta y} f(y) dy, \tag{4}$$

which means that $E[M]$ is essentially the Laplace transform or moment-generating function (MGF) of $f(y)$. Properties of the expected stochastic discount factor are thus the same as properties of the MGF of a PDF, about which a great deal is already understood.

A prime example of equation (4) is the special case where $Y \sim N(\mu, s^2)$, which yields the familiar log normal formula

$$E[M] = \exp\left(-\delta - \eta\mu + \frac{1}{2}\eta^2 s^2\right), \tag{5}$$

where $\delta = -\ln \beta$ is the instantaneous rate of pure time preference. Equation (5) shows up in innumerable asset-pricing Euler equation applications as the expected value of the stochastic discount factor or pricing kernel when consumption is log normally distributed. Expression (5) is also the basis of the well-known generalized-Ramsey formula for the risk-free interest rate

$$r^f = \delta + \eta\mu - \frac{1}{2}\eta^2 s^2, \tag{6}$$

which (in its deterministic form, for the special case $s = 0$) plays a key role in recent debates about what social interest rate to use for intergenerational cost-benefit discounting of policies to mitigate GHG emissions. This intergenerational-discounting debate has mainly revolved around choosing "ethical" values of the rate of pure time preference δ, but this paper will demonstrate that, for any $\eta > 0$, the effect of δ in formula (6) is theoretically overshadowed by the effect of the uncertain scaling parameter s. It should be borne in mind that equation (6) is an annuitized version of an interest-rate formula being used here for discounting future climate changes that will play itself out over a timescale of two centuries or so.

To create families of probability distributions that are simultaneously fairly general and analytically tractable, the following generating mechanism is employed. Suppose Z represents an RV normalized to have mean 0 and variance 1. Let $\phi(z)$ be any piecewise-continuous PDF satisfying $\int_{-\infty}^{\infty} z\phi(z)dz = 0$ and $\int_{-\infty}^{\infty} z^2\phi(z)dz = 1$, where it should be noted that the PDF $\phi(z)$ is allowed to be extremely general. For example, the distribution of Z might have finite support (like the uniform distribution, which signifies that unbounded catastrophes will be absolutely excluded conditional on the value of the finite lower support being known), or it might have unbounded range (like the normal, which allows unbounded catastrophes to occur but assigns them a thin bad tail *conditional* on the variance being known). The only restrictions placed on $\phi(z)$ are the weak regularity conditions that $\phi(z) > 0$ within some neighborhood of $z = 0$, and that $E[\exp(-\alpha z)] < \infty$ for all $\alpha > 0$, which is automatically satisfied if Z has finite lower support.

With μ and $s > 0$ given, make the affine change of RV: $Y = sZ + \mu$. The conditional PDF of y is then

$$h(y|s) = \frac{1}{s}\phi\left(\frac{y-\mu}{s}\right), \tag{7}$$

where μ, s are structural parameters having the interpretation: $\mu = E[Y]$, $s^2 = V[Y]$.

For this paper, what matters most is structural uncertainty about the scale parameter controlling the tail spread of a probability distribution, which is the most critical unknown in this setup. This scale parameter s may be loosely conceptualized as a highly stylized abstract generalization of a climate-sensitivity-like amplifying or scaling multiplier resembling S_2. (In this crude analogy, $Z \leftrightarrow \Delta \ln CO_2/\ln 2$, $SZ \leftrightarrow \Delta T$, $Y \leftrightarrow G - \gamma\Delta T$.) Without significant loss of generality, assume for ease of exposition that in equation (7) the mean μ is known, while the standard-deviation scale parameter s is unknown. The case where μ and s are both unknown involves more intricate notation but otherwise gives essentially identical results.

The point of departure here is that the conditional PDF of growth rates $h(y|s)$ is given to the agent in the form of equation (7) and, while the true value of s is unknown, the situation is *as if* some finite number of i.i.d. observations are available on which to base an estimate of s via some process of inductive reasoning. Suppose that the agent has observed the random sample $\mathbf{y} = (y_1, \ldots, y_n)$ of growth-rate data realizations from n independent draws of the distribution $h(y|s)$ defined by equation (7) for some unknown fixed value of s. An example relevant to this paper is where the sample space represents the outcomes of various economic-scientific studies and the data $\mathbf{y} = (y_1, \ldots, y_n)$ are interpreted at a very high level of abstraction as the findings of n such studies. If we are allowed to make the further abstraction that "inductive knowledge" is what we learn

from empirical data-evidence, then n here can be crudely interpreted as a measure of the degree of inductive knowledge of the situation.

The likelihood function is

$$L(s; \mathbf{y}) \propto \prod_{j=1}^{n} h(y_j|s). \tag{8}$$

Choose the prior PDF of S as

$$p_0(s) \propto s^{-k} \tag{9}$$

for some number k, crudely identifiable with the strength of prior knowledge. As k can be chosen to be arbitrarily large, the nondogmatic prior distribution (9) can be made to place arbitrarily small prior probability weight on big values of s. It should be appreciated that any scale-invariant prior must be of the form (9). Scale invariance (discussed in the Bayesian-statistical literature) is considered desirable as a description of a "noninformative" reference or default prior that favors no particular value of the scaling parameter s over any other. For such a noninformative reference or default prior, it seems not unreasonable to impose a condition of scale invariance from first principles. Suppose that the action taken in any decision problem should not depend upon the unit of measurement. Then the only prior consistent with this plausible principle of scale invariance holding over *all* possible decision problems must satisfy the condition $p_0(s) \propto p_0(\alpha s)$, and the only way this can hold for all $\alpha > 0$, $s > 0$ is when the (necessarily improper) PDF has form (9).

The posterior PDF $p_n(s|\mathbf{y})$ is proportional to the prior PDF $p_0(s)$ times the likelihood PDF $L(s;\mathbf{y})$:

$$p_n(s|\mathbf{y}) \propto p_0(s) \prod_{j=1}^{n} h(y_j|s). \tag{10}$$

Integrating s out of equation (7), the unconditional or marginal posterior-predictive PDF of y (to be plugged into equation [4]) is

$$f(y) = \int_0^{\infty} h(y|s)\, p_n(s|\mathbf{y}) ds. \tag{11}$$

Consider the prototype specification: $Z \sim N(0, 1)$; $Y|\mu, s \sim N(\mu, s^2)$; μ known; PDF of s is equation (10). Sample variance is $\nu_n \equiv \sum_{j=1}^{n} (y_j - \mu)^2/n$. Any standard textbook on Bayesian statistical theory indicates that, for this prototype case, the posterior-predictive PDF (11) is the Student-t

$$f(y) \propto \left(1 + \frac{(y-\mu)^2}{n\nu_n}\right)^{-(n+k)/2} \tag{12}$$

with $n + k$ degrees of freedom. Asymptotically, the limiting tail behavior of equation (12) is a fat-tailed power-law PDF whose exponent is the sum of inductive plus prior knowledge $n + k$.

When the posterior-predictive distribution of Y is equation (12) (from s being unknown), then equation (4) becomes

$$E[M] = +\infty, \tag{13}$$

because the MGF of a Student-t distribution is infinite.[6] What accounts technically for the economically stunning counterintuitiveness of the finding (13) is a form of pointwise but nonuniform convergence. When $n \to \infty$ in equation (12), $f(y)$ becomes the familiar normal form $\exp(-(y - \mu)^2/2\nu_\infty^2)$, which then, as $y \to -\infty$, approaches 0 faster than $\exp(-\eta y)$ approaches infinity, thereby leading to the well-known finite formula (5) for $E[M]$. Given any fixed n, on the other hand, as $y \to -\infty$ expression (12) tends to 0 only as fast as the power-law polynomial $(-y)^{-(n+k)}$, so that now in formula (4) it is the exponential term $\exp(-\eta y)$ that dominates asymptotically, thereby causing $E[M] \to +\infty$.

Something quite extraordinary seems to be happening here, which is crying out for further elucidation! Thousands of applications of EU theory in thousands of articles and books are based on formulas like (5) or (6). Yet when it is acknowledged that s is unknown (with a standard noninformative reference prior) and its value in formula (5) or (6) must instead be inferred as if from a data sample that can be *arbitrarily large* (but finite), expected marginal utility explodes. The question then naturally arises: What is EU theory trying to tell us when its conclusions for a host of important applications—in CBA, asset pricing, and many other fields of economics—seem so sensitive merely to the recognition that conditioned on finite realized data the distribution implied by the normal is the Student-t?

The Student-t "child" posterior-predictive density from a large number of observations looks almost exactly like its bell-shaped normal "parent" except that the probabilities are somewhat more stretched out, making the tails appear relatively fatter at the expense of a slightly flatter center. In the limit, the ratio of the fat Student-t tail probability divided by the thin normal tail probability approaches infinity, even while both tail probabilities are approaching 0. Intuitively, a

[6] The example in this section with these particular functional forms leading to existence problems from indefinite expected-utility integrals blowing up was first articulated in the important pioneering note of Geweke (2001). Weitzman (2007a) extended this example to a nonergodic evolutionary stochastic process and developed some implications for asset pricing in a nonstationary setting. For the application here to the economics of catastrophic climate change I believe the nonergodic evolutionary formulation is actually more relevant and gives stronger insights, but it is just not worth the additional complexity for what is essentially an applied paper whose basic points are adequately conveyed by the simpler stationary case. The same comment applies to modeling the PDFs of S_1, S_2, or ΔT in a less abstract way that ties the analysis more directly and more specifically to the scientific climate-change literature as it stands now.

normal density "becomes" a Student-t from a tail-fattening spreading-apart of probabilities caused by the variance of the normal having itself a (inverted gamma) probability distribution. It is then no surprise from EU theory that people are more averse qualitatively to a relatively fat-tailed Student-t posterior-predictive child distribution than they are to the relatively thin-tailed normal parent which begets it. A perhaps more surprising consequence of EU theory is the quantitative strength of this endogenously derived aversion to the effects of unknown tail structure. The story behind this quantitative strength is that fattened posterior-predictive bad tails represent structural or deep uncertainty about the possibility of rare high-impact disasters that—using colorful language here—"scare" any agent having a utility function with relative risk aversion everywhere bounded above 0.

IV. The Key Role of a "VSL-Like Parameter"

To jump ahead of the story just a bit, last section's general model has essentially the same unsettling property as the disturbing Normal → Student-t example given at the end of the section—namely that $E[M]$ is unbounded. The core underlying problem is the difficulty of learning limiting tail behavior inductively from finite data. Seemingly thin-tailed probability distributions (like the normal), which are actually only thin-tailed conditional on known structural parameters of the model (like the standard deviation), become tail-fattened (like the Student-t) after integrating out the structural-parameter uncertainty. This core issue is generic and cannot be eliminated in any clean way. When combined with unlimited downside exposure it must influence any utility function sensitive to low values of consumption.

Technically, for the analysis to proceed further some mathematical mechanism is required to close the model in the sense of bounding $E[M]$. A variety of bounding mechanisms are possible, with the broad general conclusions of the model not being tied to any one particular bounding mechanism. This paper closes the model by placing an ad hoc positive lower bound on consumption, which is denoted D (for "death"), so that always $C \geq D > 0$. The lower bound D is not *completely* arbitrary, however, because it can be related conceptually to a "fear of ruin" or a "value of statistical life" (VSL) parameter.[7] This has the advantage of tying conclusions to a familiar economic concept whose ballpark estimates can at least convey some extremely crude quantitative implications for the economics of climate change. In this empirical sense the glass is half full (which is more than can be said for other ways of closing this model). However, the glass is half empty in the empirical sense that an accurate CBA of climate change can end up being distressingly dependent on some very large VSL-like coefficient about whose size we are highly unsure.

The critical coefficient that is behind the lower bound on consumption is called the *VSL-like parameter* and is denoted λ. This "VSL-like parameter" λ is intended to be akin to the already somewhat vague concept of the value of a human statistical life, only in the context here it represents the yet far fuzzier concept of something more like the value of statistical civilization as we know it, or perhaps even the value of statistical life on Earth (as we know it). In this paper I am just going to take λ to be some very big number that indirectly controls the convergence of the integral defining $E[M]$ by implicitly generating a lower bound $D(\lambda) > 0$ on consumption. An empirical first approximation of λ (normalized per capita) might be given by conventional estimates of the value of a statistical human life, which may be much too small for the purposes at hand but will at least give some crude empirical idea of what is implied numerically as a point of departure.

The basic idea is that a society trading off a decreased probability of its own catastrophic demise against the cost of lowering the probability of that catastrophe is facing a decision problem conceptually analogous to how a person might make a tradeoff between decreased consumption as against a lower probability of that person's own individually catastrophic end. However artificial or peculiar the use of a VSL-like parameter to close this model might seem in a context of global climate change, other ways of closing this model seem to me even more artificial or peculiar. I am not trying to argue that a VSL-like parameter (as described above) naturally and intuitively suggests itself as a great candidate for closing this model—I am just saying that it seems better than the alternatives. In this spirit, suppose for the sake of developing the argument that the analysis is allowed to proceed as if the treatment of the most catastrophic conceivable impact of climate change is very roughly analogous to the simplest possible economic model of the behavior of an individual agent who is trading off increased consumption against a slightly increased probability of death.

Let D be a disastrously low value of consumption representing the analog of a starvation level, below which the individual dies. Let the utility associated with death be normalized at 0. The utility function $U(C; D)$ is chosen to be of the analytically convenient CRRA form

$$U(C; D) = \frac{C^{1-\eta} - D^{1-\eta}}{1 - \eta} \tag{14}$$

for $C \geq D$, and $U(C; D) \equiv 0$ for $0 \leq C < D$. The constant CRRA coefficient in equation (14) is η.

[7] The parameter λ that is being used here to truncate the extent of catastrophic damages is akin to the "fear of ruin" coefficient introduced by Aumann and Kurz (1977) to characterize an individual's "attitude toward risking his fortune" in binary lotteries. Foncel and Treich (2005) later analyzed this fear-of-ruin coefficient and showed that it is basically the same thing analytically as VSL. The particular utility function I use later in this section is essentially identical (but with a different purpose in a different context) to a specification used recently by Hall and Jones (2007), which, according to them, is supported by being broadly consistent with a wide array of stylized facts about health spending and empirical VSL estimates.

Without loss of generality, current consumption is normalized as it was before at $C = 1$. For simplicity, suppose the agent begins with something close to a zero probability of death in the current period. Let $A(q)$ be the amount of extra consumption the individual requires within this period to exactly compensate for $P[C \leq D] = q$ within this period. In free translation, q is the probability of death. From EU theory, $A(q)$ satisfies the equation $(1 - q)U(1 + A(q); D) \equiv U(1; D)$, which, when differentiated with respect to q and evaluated at $q = 0$ yields

$$-U(1; D) + U_1(1; D)\lambda = 0, \tag{15}$$

where $\lambda \equiv A'(0)$. Note that the "VSL-like parameter" λ is defined as the rate of substitution between consumption and mortality risk, here being $A'(0)$.

Equation (15) can be inverted to give the implied lower bound on consumption D as an implicit function of the VSL-like parameter λ. Inverting equation (15) for isoelastic utility function (14) yields

$$D(\lambda) = [1 + (\eta - 1)\lambda]^{-1/(\eta-1)}. \tag{16}$$

To ensure the reasonable condition that $D(\lambda)$ in equation (16) declines monotonically in λ requires that $\eta > 1$, which is hereby assumed.

From a wide variety of empirical studies in disparate contexts, a plausible value of the coefficient of relative risk aversion might be 2.[8] Very rough ballpark estimates of the per capita value of a statistical human life might be of the order of magnitude of a hundred times per capita consumption.[9] Plugging $\eta \approx 2$, $\lambda \approx 100$ into formula (16) gives $D(100) \approx 0.01$. An interpretation of λ as a parameter representing the per capita value of statistical civilization or the per capita value of statistical life on Earth (as we currently know or understand these concepts) presumably involves much higher values of λ than $\approx$100. Choosing, for example, $\lambda \approx 1{,}000$ gives $D(1{,}000) \approx 0.001$. In any event, I note here for later reference that a Monte Carlo simulation assessing the EU impacts of losing up to 99% (much less 99.9%) of welfare-equivalent consumption in the bad fat tail is very different from any simulations now being done with any existing empirical model of climate change.

V. The Dismal Theorem

Let $E[M|\lambda]$ represent the expected value of a stochastic discount factor $M(C)$ given by formula (3) when $C \geq D(\lambda)$ (or, equivalently, $Y \geq \ln D(\lambda)$) and given by $M(C) = (D(\lambda))^{-\eta}$ when $C < D(\lambda)$ (or, equivalently, $Y < \ln D(\lambda)$), where $D(\lambda)$ is defined by equation (16). The following "dismal theorem" (hereafter sometimes abbreviated "DT") shows under quite general circumstances what happens to the price of future consumption $E[M|\lambda]$ when λ might be *very* big.

Theorem 1. *For* **any given** *n and k,*

$$\lim_{\lambda\to\infty} E[M|\lambda] = +\infty. \tag{17}$$

Proof. Combining the interpretation of $D(\lambda)$ from equation (16) with equations (4) and (11)—and tracing the links of equations from (16) all the way back to (7)—implies that

$$E[M|\lambda] \propto \int_0^\infty \frac{1}{s^{k+n+1}} \prod_{j=1}^{n} \phi\left(\frac{y_j - \mu}{s}\right) \times \left[\int_{\ln D(\lambda)}^{\infty} e^{-\eta y} \phi\left(\frac{y - \mu}{s}\right) dy\right] ds. \tag{18}$$

Make the change of variable $z = (y - \mu)/s$, use the fact from equation (16) that $D(\infty) = 0$, and reverse the order of integration to rewrite equation (18) as

$$\lim_{\lambda\to\infty} E[M|\lambda] \propto \int_{-\infty}^{\infty} \phi(z) \left[\int_0^\infty e^{-\eta z s} \frac{1}{s^{k+n}} \prod_{j=1}^{n} \phi\left(\frac{y_j - \mu}{s}\right) ds\right] dz. \tag{19}$$

Pick any value of z' for which simultaneously $z' < 0$ and $\phi(z) > 0$ in an open neighborhood of $z = z'$. Then note that

$$\lim_{s\to\infty} \left\{e^{-\eta z' s} \frac{1}{s^{k+n}}\right\} = +\infty, \tag{20}$$

implying equation (19) also approaches $+\infty$ as $\lambda \to \infty$, which concludes this proof sketch.[10] ■

[8] Two is the point estimate for η selected by Hall and Jones (2007) in a conceptually similar model and defended by them with references to a wide range of studies on page 61 of their paper.

[9] For this particular application of using a VSL-like parameter to analyze the extent of the worst imaginable climate-change catastrophe, I think that the most one might hope for is accuracy to within about an order of magnitude—anything more being false precision. Even the empirical estimates for the value of a much better defined statistical human life have a disturbingly wide range, but $\lambda \approx 100$ is roughly consistent with the meta-analysis in Bellavance, Dionne, and Lebeau (2007) or the survey of Viscusi and Aldy (2003).

[10] This is only a highly compressed, loose sketch of the *structure* of a proof. It is being included here primarily to provide *some* motivation for the formulas in the analysis, which comes next, that depend upon equation (20). In this spirit, the purpose of this "proof sketch" is to give at least a minimal quick-and-dirty indication of where equation (20) is coming from. A rigorous proof can be built around the very significant (perhaps even seminal) contribution of Michael Schwarz to decision-making under extreme uncertainty. An important result proved in Schwarz (1999) is that, in the limit, the tails of $f(y)$ defined by equation (11) are power-law of order $n + k$. From this fact, a rigorous proof of theorem 1 then proceeds along the lines sketched here.

The underlying logic behind the strong result of theorem 1 is described by the limiting behavior of equation (20) for large values of s. Given any values of n and k, the *probability* of a disaster declines *polynomially* in the scale s of the disaster from equation (20), while the marginal-utility *impact* of a disaster increases *exponentially* in the scale s of the disaster. It is intuitive, and can readily be proved, that the tail of the RV Y essentially behaves like the tail of the RV S. Therefore, irrespective of the original parent distribution, the effect of an uncertain scale parameter fattens the tail of the posterior-predictive child distribution so that it behaves asymptotically like a power-law distribution with coefficient from equation (20) equal to $n + k$. In this sense, power-law tails need not be postulated, because they are essentially unavoidable in posterior-predictive PDFs.[11] No matter the (finite) number of observations, the race to the bottom of the bad tail between a polynomially contracting probability times an exponentially expanding marginal-utility impact is won in the limit every time by the marginal-utility impact—for *any* utility function having positive relative risk aversion in the limit as $C \to 0^+$. This point is important: utility isoelasticity per se is inessential to the reasoning here (although it makes the argument easier to understand), because the expected stochastic discount factor $E[M] \to +\infty$ in this setup for *any* relatively risk-averse utility function satisfying the curvature requirement $\inf_{C>0} [-CU''(C)/U'(C)] > 0$.

I want to emphasize emphatically: the key issue here is not a mathematically illegitimate use of the symbol $+\infty$ in formulas (13) or (17), which incorrectly seems to offer a deceptively easy way out of the dilemma that $E[M] \to +\infty$ by somehow discrediting this application of EU theory on the narrow grounds that infinities are not allowed in a legitimate theory of choice under uncertainty. It is easy to put arbitrary bounds on utility functions, to truncate probability distributions arbitrarily, or to introduce ad hoc priors that arbitrarily cut off or otherwise severely dampen high values of S or low values of C. Introducing any of these changes formally closes the model in the sense of replacing the symbol $+\infty$ by an arbitrarily large but finite number. Indeed, the model of this paper has been closed in just such a fashion by placing a lower bound on consumption of the form $C \geq D$, where the lower bound $D(\lambda) > 0$ is defined indirectly by a "value of statistical life" parameter λ. However, removing the infinity symbol in this or any other way does not eliminate the underlying problem because it then comes back to haunt in the form of an arbitrarily large expected stochastic discount factor, whose exact value depends sensitively upon obscure bounds, truncations, severely dampened or cut-off prior PDFs, or whatever other tricks have been used to banish the $+\infty$ symbol. One can easily remove the $+\infty$ in formulas (13) or (17), but one cannot so easily remove the underlying economic problem that expected stochastic discount factors—which lie at the heart of cost-benefit, asset-pricing, and many other important applications of EU theory—can become arbitrarily large just from unobjectionable statistical inferences about limiting tail behavior. The take-away message here is that reasonable attempts to constrict the length or the fatness of the "bad" tail (or to modify the utility function) still can leave us with uncomfortably big numbers whose exact value depends nonrobustly upon artificial constraints or parameters that we really do not understand. The only legitimate way to avoid this potential problem is when there exists strong a priori knowledge that restrains the extent of total damages. If a particular type of idiosyncratic uncertainty affects only one small part of an individual's or a society's overall portfolio of assets, exposure is naturally limited to that specific component and bad-tail fatness is not such a paramount concern. However, some very few but very important real-world situations have potentially unlimited exposure due to structural uncertainty about their potentially open-ended catastrophic reach. Climate change potentially affects the whole worldwide portfolio of utility by threatening to drive all of planetary welfare to disastrously low levels in the most extreme scenarios.

The interpretation and application of theorem 1 is sensitive to a subtle but important behind-the-scene tug of war between pointwise but nonuniform limiting behavior in λ and pointwise but nonuniform limiting behavior in n. This kind of bedeviling nonuniform convergence haunts fat-tailed CBA and turns numerical climate-change applications of DT into a practical nightmare. To see more clearly how the issue of determining $E[M]$ under pointwise but nonuniform convergence plays itself out, suppose that, unbeknownst to the agent, the "true" value of s is s^*. Since the prior $p_0(s)$ by equation (9) assigns positive probability to an open interval around s^*, the imposed specification has sufficient regularity for large-sample likelihood dominance to cause strong (that is, almost sure) convergence of the posterior distribution (10) of S to its true data-generating process (DGP) value $s = s^*$. This in turn means that the posterior-predictive PDF of growth rates (11) converges strongly to its true DGP distribution $h(y|s^*)$ and—for *any given* $\lambda < \infty$—$E[M|\lambda]$ converges strongly to its true value:

$$n \to \infty \Rightarrow E[M|\lambda] \underset{a.s.}{\longrightarrow} \beta \int_{-\infty}^{\infty} e^{-\eta y} \frac{1}{s^*} \phi\left(\frac{y-\mu}{s^*}\right) dy. \quad (21)$$

Condition (21) signifies that for any given $\lambda < \infty$ (which via equation [16] puts a positive lower bound $D(\lambda)$ on C,

[11] As stated here, DT depends upon an invariant prior of the polynomial (aka power-law aka Pareto) form (9), but this is not much of a limitation because k can be *any number*. To undo the infinite limit in (17) requires a *noninvariant* prior that additionally approaches 0 faster than *any polynomial in* $1/s$ (as $s \to \infty$). In such a case the limit in (17) is a finite number, but its (potentially arbitrarily large) value will depend critically upon the strong *a priori* knowledge embodied in the presumed-known parameters of such a noninvariant prior—and the prior-sensitivity message that such a formulation ends up delivering is very similar anyway to the message delivered by the model of this paper.

and thereby a finite upper bound on M), in the limit as full structural knowledge is approached (because $n \to \infty$), $E[M|\lambda]$ goes to its true value. What is happening here is that as the strength of inductive knowledge n is increasing in the form of more and more data observations piling up, it is becoming increasingly apparent that the probability of C being anywhere remotely as low as the cutoff $D(\lambda)$ is ignorable—even after taking into account the possible EU impacts of disastrously low utilities for C close to $D(\lambda)$. A conventional pure-risk-like application of thin-tailed EU theory essentially corresponds, then, to a situation where there is sufficient inductive-plus-prior knowledge to identify the relevant structure because $n + k$ is reasonably large relative to the VSL-like parameter λ—and relative to the much less controversial parameters β and η.

Concerning conventional parameters β and η, we have at least some rough idea of what might be empirically relevant (say $\beta \approx 99\%$ per year and $\eta \approx 2$). In complete contrast, any discussion about climate change concerning the empirically relevant value of the nonconventional VSL-like parameter λ belongs to a much more abstract realm of discourse. It is therefore understandable to want climate-change CBA to be restricted to dealing only with modest damages by disregarding nightmare scenarios (as being "too speculative" or "not based on hard science") via chopping off the really-bad tail and then ignoring it. This is the de facto strategy employed by most of those relatively few existing CBAs of climate change that even bother to concern themselves at all with a formal treatment of uncertain high-impact damages. Alas, to be confident in the validity of such a cutoff strategy in a situation where we are grossly unsure about λ or D effectively requires uniform convergence of $E[M]$ for all conceivable values of λ or D. Otherwise, for any given level of inductive-plus-prior knowledge $n + k$, a skeptical critic could always come back and ask how robust is CBA to the highly unsure truncation value of $D(\lambda)$. Similar robustness questions apply to any a priori presumption or imposition of thin-tailed PDFs.

Note well that with equation (21) the a.s. convergence of $E[M|\lambda]$ to its true value is pointwise but not uniform in n. No matter how much data-evidence n exists—or even can be imagined to exist—DT says that $E[M|\lambda]$ is always exceedingly sensitive to very large values of λ. If "risk" means that the DGP is known exactly (only the outcome is random), while "uncertainty" means that (as well as the outcome being random) the parameters of the DGP are unknown and must be estimated statistically, then DT can be interpreted as saying that structural "uncertainty" can always trump pure "risk" for situations of potentially unlimited downside exposure when no plausible bound $D(\lambda) > 0$ can confidently be imposed by prior knowledge. DT can therefore be interpreted as implying a spirit in which it may be unnecessary to append to the theory of decision-making under uncertainty an ad hoc extra postulate of "ambiguity aversion." At least for situations where there is fundamental uncertainty about an open-ended catastrophe coexisting with fear of ruin, EU theory itself already tells us precisely how the "ambiguity" of structural-parameter uncertainty can be especially important and why people may be much more averse to it than to pure objective-frequency "risk."

The dismal theorem makes a general point but also has a particular application to the economics of climate change. The general point is that theorem 1 embodies a very strong form of a "generalized precautionary principle" for situations of potentially unlimited downside exposure. From experience alone one cannot acquire sufficiently accurate information about the probabilities of disasters in the bad tail to make $E[M]$ or $E[U]$ independent of the VSL-like parameter λ—thereby potentially allowing this VSL-like-parameter aspect to dominate CBA applications of EU theory under conditions of potentially unlimited liability.

The part of the distribution of possible future outcomes that can most readily be learned (from inductive information of a form as if conveyed by data) concerns the relatively more likely outcomes in the middle of the distribution. From previous experience, 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 toward probabilities in the periphery of the distribution, however, we are increasingly moving into the unknown territory of subjective uncertainty where our probability estimate of the probability distributions themselves becomes increasingly diffuse because the frequencies of rare events in the tails cannot be pinned down by previous experiences or past observations. It is not possible to learn enough about the frequency of extreme tail events from finite samples alone to make $E[M]$ or $E[U]$ independent of artificially imposed bounds on the extent of possibly ruinous disasters. This principle is true even in the stationary model of this paper where an ergodic theorem holds, but it applies much more forcefully to an evolutionary process like real-world anthropogenic warming.[12] Climate-change economics generally—and the fatness of climate-sensitivity tails specifically—are prototype examples of this principle, because we are trying to extrapolate inductive knowledge far outside the range of limited past experience.

VI. What Is the Dismal Theorem Trying to Tell Us?

A common reaction to the conundrum for CBA implied by DT is to acknowledge its mathematical logic but to wonder how it is to be used constructively for deciding what to do in practice. Is DT an economics version of an impossibility theorem which signifies that there are fat-tailed situations where economic analysis is up against a very

[12] This principle comes across with much greater force in an evolutionary world based upon an analytically more complicated nonstationary nonergodic stochastic process modeled along the lines of Weitzman (2007a).

strong constraint on the ability of *any* quantitative analysis to inform us *without* committing to a VSL-like parameter and an empirical CBA framework that is based upon *some* explicit numerical estimates of the miniscule probabilities of all levels of catastrophic impacts down to absolute disaster? Even if it were true that DT represents a valid economic-statistical precautionary principle which, at least theoretically, might dominate decision-making, would not putting into practice this "generalized precautionary principle" freeze all progress if taken too literally? Considering the enormous inertias that are involved in the buildup of GHGs, and the warming consequences, is the possibility of learning and mid-course corrections a plausible counterweight to DT, or, at the opposite extreme, has the commitment of GHG stocks in the ultra-long pipeline already fattened the bad tail so much that it doesn't make much difference what is done in the near future about GHG emissions? How should the bad fat tail of climate uncertainty be compared with the bad fat tails of various proposed solutions such as nuclear power, geoengineering, or carbon sequestration in the ocean floor? Other things being equal, the dismal theorem suggests as a policy response to climate change a relatively more cautious approach to GHG emissions, but how much more caution is warranted?

I simply do not know the full answers to the extraordinarily wide range of legitimate questions that DT raises. I don't think anyone does. But I also don't think that such questions can be allowed in good conscience to be simply brushed aside by arguing, in effect, that when probabilities are small and *im*precise, then they should be set *precisely* to 0. To the extent that uncertainty is formally considered at all in the economics of climate change, the artificial practice of using thin-tailed PDFs—especially the usual practice of imposing *de minimis* low-probability-threshold cutoffs that casually dictate what part of the high-impact bad tail is to be truncated and discarded from CBA—seems arbitrary and problematic.[13] In the spirit that the unsettling questions raised by fat-tailed CBA for the economics of climate change must be addressed seriously, even while admitting that we do not now know all of the answers, I offer here some speculative thoughts on what it all means. Even if the quantitative magnitude of what DT implies for climate-change policy seems somewhat hazy, the qualitative direction of the policy advice is nevertheless quite clear.

Any interpretation or application of the dismal theorem is rendered exceedingly tricky by the bedeviling (for CBA) nonuniform convergence of $E[M]$ or $E[U]$ in its other parameters relative to the key VSL-like parameter λ. This nonuniform convergence enables $E[M]$ or $E[U]$ to explode (for any other given parameter values) as $\lambda \to \infty$. One might try to argue that the values of $E[M]$ or $E[U]$ are ultimately an empirical matter to be decided empirically (by analytical formulas or simulation results), with relevant parameter values of λ, n, k, δ, η, μ, and so forth being taken together as an empirically plausible ensemble. The idea that the values of $E[M]$ or $E[U]$ should depend on testable, empirically reasonable values of λ and the other parameters is, of course, right on some level—and it sounds reassuring. Yet, as a practical matter, the fact that $E[M]$ and $E[U]$ are so sensitive to large values of λ (or small values of D), about which we can have little confidence in our own a priori knowledge, casts a very long shadow over any empirical CBA of a situation to which the dismal theorem might apply. In ordinary, limited-exposure or thin-tailed situations, there is at least the underlying theoretical reassurance that finite-cutoff-based CBA might (at least in principle) be an arbitrarily close approximation to something that is accurate and objective. In fat-tailed, unlimited-exposure DT situations, by contrast, there is no such theoretical assurance underpinning the arbitrary cutoffs, which is ultimately due to the haunting lack of uniform convergence of $E[M]$ or $E[U]$ with respect to λ or D.

One does not want to abandon lightly the ideal that CBA should bring independent empirical discipline to any application by being based upon empirically reasonable parameter values. Even when DT applies, CBA based upon empirically reasonable functional forms and parameter values (including λ) might reveal useful information. Simultaneously one does not want to be obtuse by insisting that DT per se makes no practical difference for CBA because the VSL-like coefficient λ is just another parameter to be determined empirically and then simply plugged into the analysis along with some extrapolative guesses about the form of the "damages function" for high-temperature catastrophes (combined with speculative extreme-tail probabilities). So a tricky balance is required between being overawed by DT into abandoning CBA altogether and being underawed by DT into insisting that it is just another empirical issue to be sorted out by business-as-usual CBA.

The degree to which the kind of "generalized precautionary principle" embodied in the dismal theorem is relevant for a particular application must be decided on a case-by-case "rule of reason" basis. It depends generally upon the extent to which prior λ-knowledge and prior k-knowledge combine with inductive-posterior n-knowledge in a particular case to fatten or to thin the bad tail. In the particular application to the economics of climate change, with so obviously limited data and limited experience about the catastrophic reach of climate extremes, to ignore or suppress the significance of rare fat-tailed disasters is to ignore or suppress what economic-statistical decision theory is telling us here loudly and clearly is potentially the most important part of the analysis.

Where does global warming stand in the portfolio of extreme risks currently facing us? There exist maybe half a dozen or so serious "nightmare scenarios" of environmental disasters perhaps comparable in conceivable worst-case

[13] Adler (2007) sketches out in some detail the many ways in which *de minimis* low-probability-threshold cutoffs are arbitrary and problematic in more ordinary regulatory settings.

impact to catastrophic climate change. These might include biotechnology, nanotechnology, asteroids, strangelets, pandemics, runaway computer systems, and nuclear proliferation.[14] It may well be that each of these possibilities of environmental catastrophe deserves its own CBA application of DT along with its own empirical assessment of how much probability measure is in the extreme tails around $D(\lambda)$. Even if this were true, however, it would not lessen the need to reckon with the strong potential implications of DT for CBA in the particular case of climate change.

Perhaps it is little more than raw intuition, but for what it is worth I do not feel that the handful of other conceivable environmental catastrophes are nearly as critical as climate change. I illustrate with two specific examples. The first is widespread cultivation of crops based on genetically modified organisms (GMOs). At casual glance, climate-change catastrophes and bioengineering disasters might look similar. In both cases, there is deep unease about artificial tinkering with the natural environment, which can generate frightening tales of a planet ruined by human hubris. Suppose for specificity that with GMOs the overarching fear of disaster is that widespread cultivation of so-called Frankenfood might somehow allow bioengineered genes to escape into the wild and wreak havoc on delicate ecosystems and native populations (including, perhaps, humans), which have been fine-tuned by millions of years of natural selection. At the end of the day I think that the potential for environmental disaster with Frankenfood is much less than the potential for environmental disaster with climate change—along the lines of the following loose and oversimplified reasoning.

In the case of Frankenfoods interfering with wild organisms that have evolved by natural selection, there is at least some basic underlying principle that plausibly dampens catastrophic jumping of artificial DNA from cultivars to landraces. After all, nature herself has already tried endless combinations of mutated DNA and genes over countless millions of years, and what has evolved in the fierce battle for survival is only an infinitesimal subset of the very fittest permutations. In this regard there exists at least some inkling of a prior high-k argument making it fundamentally implausible that Frankenfood artificially selected for traits that humans find desirable will compete with or genetically alter the wild types that nature has selected via Darwinian survival of the fittest. Wild types have already experienced innumerable small-step genetic mutations, which are perhaps comparable to large-step human-induced artificial modifications and which have not demonstrated survival value in the wild. Analogous arguments may also apply for invasive "superweeds," which so far represent a minor cultivation problem lacking ability to displace either landraces or cultivars. Besides all this, safeguards in the form of so-called terminator genes can be inserted into the DNA of GMOs, which directly prevent GMO genes from reproducing themselves.

A second possibly relevant example of comparing climate change with another potential catastrophe concerns the possibility of a large asteroid hitting Earth. In the asteroid case it seems plausible to presume there is much more high-n inductive knowledge (from knowing something about asteroid orbits and past collision frequencies) pinning down the probabilities to very small "almost known" values. If we use $P[\Delta T > 20°\text{C}] \approx 1\%$ as the very rough probability of a climate-change cataclysm occurring within the next two centuries, then this is roughly 10,000 times larger than the probability of a large asteroid impact (of a one-in-a-hundred-million-years size) occurring within the same time period.

Contrast the above discussion about plausible magnitudes or probabilities of disaster for genetic engineering or asteroid collisions with possibly catastrophic climate change. The climate-change "experiment," whose eventual outcome we are trying to infer now, "tests" the planet's response to a geologically instantaneous exogenous injection of GHGs. An exogenous injection of this much GHGs this fast seems unprecedented in Earth's history stretching back perhaps billions of years. Can anyone honestly say now, from very limited low-k prior information and very limited low-n empirical experience, what are reasonable upper bounds on the eventual global warming or climate change that we are currently trying to infer will be the outcome of such a first-ever planetary experiment? What we do know about climate science and extreme tail probabilities is that planet Earth hovers in an unstable trigger-prone "whipsaw" ocean-atmosphere system,[15] chaotic dynamic responses to geologically instantaneous GHG shocks are quite possible, and all 22 recently published studies of climate sensitivity cited by IPCC-AR4 (2007), when mechanically aggregated together, estimate on average that $P[S_1 > 7°\text{C}] \approx 5\%$. To my mind this open-ended aspect with a way-too-high subjective probability of a catastrophe makes GHG-induced global climate change vastly more worrisome than cultivating Frankenfood or colliding with large asteroids.

These two examples hint at making a few meaningful distinctions among the handful of situations where DT might reasonably apply. My discussion here is hardly conclusive, so we cannot rule out a biotech or asteroid disaster. However, I would say on the basis of this line of argument that such disasters seem extremely unlikely, whereas a climate disaster seems "only" very unlikely. In the language of this paper, synthetic biology or large asteroids feel more like high-$(k + n)$ situations that we know a lot more about relative to climate change, which by comparison feels more like a low-$(k + n)$ situation about which we know relatively little. Regardless of whether my argument here is convincing, the overarching principle is this: the mere fact

[14] Many of these are discussed in Posner (2004), Sunstein (2007), and Parson (2007).

[15] On the nature of this unstable "whipsaw" climate equilibrium, see Hansen et al. (2007).

that DT might also apply to a few other environmental catastrophes does not constitute a valid reason for excluding DT from applying to climate change.

The simplistic two-period setup of this paper ignores or suppresses some important features of the climate-change problem. For instance, the really high values of ΔT are more likely to arrive (if they arrive at all) at further-distant future times. A more careful model of temperature dynamics[16] shows that the flavor of the two-period model survives this over-simplification via the following intuitive logic. If τ is the time of possible arrival of really high values of ΔT, then distant-future τ is associated with low β in formula (4), and once again we have the bedeviling (for CBA) existence of pointwise but nonuniform convergence—here in λ and τ (or λ and β). For any *given* $\lambda < \infty$, $\tau \rightarrow \infty$ implies $\beta \rightarrow 0$, which in equation (4) implies $E[M] \rightarrow 0$. But for any *given* $\beta > 0$, from DT $\lambda \rightarrow \infty$ implies $E[M] \rightarrow \infty$. Again here, this nonuniform-convergence aspect of the problem is what turns fat-tailed CBA into such an empirical-numerical nightmare for the economic evaluation of climate change.

A simplistic two-period setup also represses the real-option value of waiting and learning. Concerning this aspect, however, with climate change we are on the four horns of two dilemmas. The horns of the first dilemma are the twin facts that built-up stocks of GHGs might end up ex post representing a hugely expensive irreversible accumulation, but so too might massive investments in noncarbon technologies that are at least partly unnecessary.

The second dilemma is the following. Because climate-change catastrophes develop slower than some other potential catastrophes, there is ostensibly somewhat more chance for learning and mid-course corrections with global warming relative to, say, biotechnology (but not necessarily relative to asteroids when a good tracking system is in place). The possibility of "learning by doing" may well be a more distinctive feature of global-warming disasters than some other disasters, and in that sense deserves to be part of an optimal climate-change policy. The other horn of this second dilemma, however, is the nasty fact that the ultimate climate response to GHGs has tremendous inertial pipeline-commitment lags of several centuries (via the carbon cycle). When all is said and done, I don't think there is a smoking gun in the biotechnology, asteroid, or any other catastrophe scenario quite like the idea that a crude amalgamation of numbers from the most recent peer-reviewed published scientific articles is suggesting something like $P[S_2 > 10°\text{C}] \approx 5\%$ and $P[S_2 > 20°\text{C}] \approx 1\%$.

Global climate change unfolds over a timescale of centuries and, through the power of compound interest, a standard CBA of what to do now to mitigate GHGs is hugely sensitive to the discount rate that is postulated. This has produced some sharp disagreements among economists about what is an "ethical" value of the rate of pure time preference δ (and the CRRA coefficient η) to use for intergenerational discounting in the deterministic version ($s = 0$) of the Ramsey equation (6) that forms the analytical backbone for most studies of the economics of climate change.[17] For the model of this paper, which is based on structural uncertainty, arguments about what values of δ to use in equations (5) or (6) translate into arguments about what values of β to use in the model's structural-uncertainty generalization of the Ramsey equation (4). (A zero rate of pure time preference $\delta = 0$ in equation [6] corresponds to $\beta = 1$ in equation [4].) In this connection, theorem 1 seems to be saying that no matter what values of β or η are selected, so long as $\eta > 0$ and $\beta > 0$ (equivalent to $\delta < \infty$), any big-λ CBA of GHG-mitigation policy should be presumed (until shown otherwise empirically) to be affected by fat-tailed structural uncertainty. The relevance of this presumption is brought home starkly by a simple numerical example based on equations (14) and (16) that if $\lambda \approx 1{,}000$ and the probability of a life-ending catastrophe is ≈ 0.005, then for $\eta \approx 2$ the (undiscounted) willingness to pay to avoid this catastrophe is $\approx 83\%$ of consumption.

Expected utility theory in the form of DT seems to be suggesting here that the debate about discounting may be secondary to a debate about the open-ended catastrophic reach of climate disasters. While it is always fair game to challenge the assumptions of a model, when theory provides a generic result (like "free trade is Pareto optimal" or "steady growth eventually outstrips one-time change") the burden of proof is commonly taken as being upon whoever wants to overrule the theorem in a particular application. The burden of proof in climate-change CBA is presumptively upon whoever calculates expected discounted utilities *without* considering that structural uncertainty might matter more than discounting or pure risk. Such a middle-of-the-distribution modeler should be prepared to explain why the bad fat tail of the posterior-predictive PDF does not play a significant role in climate-change CBA when it is combined with a specification that assigns high disutility to high temperatures.

VII. Possible Implications for Climate-Change Policy

A so-called integrated assessment model (hereafter "IAM") for climate change is a multiequation computerized model linking aggregate economic growth with simple climate dynamics to analyze the economic impacts of global

[16] Available upon request as Weitzman, "Some Dynamic Implications of the Climate-Sensitivity Inference Problem" (2008).

[17] While this contentious intergenerational-discounting issue has long existed (see, for example, the various essays in Portney & Weyant, 1999), it has been elevated to recent prominence by publication of the controversial *Stern Review of the Economics of Climate Change* (2007). The *Review* argues for a base case of preference-parameter values $\delta \approx 0$ and $\eta \approx 1$, on which its strong conclusions depend analytically. Alternative views of intergenerational discounting are provided in, for example, Dasgupta (2007), Nordhaus (2007), and Weitzman (2007b). The last of these also contains a heuristic exposition of the contents of this paper, as well as giving *Stern* some credit for emphasizing informally the great uncertainties associated with climate change.

warming. An IAM is essentially a dynamic model of an economy with a controllable GHG-driven externality of endogenous greenhouse warming. IAMs have proven themselves useful for understanding some aspects of the economics of climate change—especially in describing outcomes from a complicated interplay of the very long lags and huge inertias involved. Most existing IAMs treat central forecasts of damages as if they were certain and then do some sensitivity analysis on parameter values. In the rare cases where an IAM formally incorporates uncertainty, it uses thin-tailed PDFs including, especially, truncation of PDFs at arbitrary cutoffs. With the model of this paper, uncertainty about adaptation and mitigation shows up in the reduced form of a fat-tailed PDF of $Y \equiv \ln C$. In the IAM literature, this issue of very unsure adaptation and mitigation involves discussion or even debate about the appropriate choice of a deterministic "damages function" for high-temperature changes.

All existing IAMs treat high-temperature damages by an extremely casual extrapolation of whatever specification is arbitrarily assumed to be the low-temperature "damages function." High-temperature damages extrapolated from a low-temperature damages function are remarkably sensitive to assumed functional forms and parameter combinations because almost anything can be made to fit the low-temperature damages assumed by the modeler. Most IAM damages functions reduce welfare-equivalent consumption by a quadratic-polynomial multiplier equivalent to $1/[1 + \gamma(\Delta T)^2]$, with γ calibrated to some postulated loss for $\Delta T \approx 2°\text{C}–3°\text{C}$. There was never any more compelling rationale for this particular loss function than the comfort that economists feel from having worked with it before. In other words, the quadratic-polynomial specification is used to assess climate-change damages for no better reason than casual familiarity with this particular form from other cost-of-adjustment dynamic economic models, where it has been used primarily for analytical simplicity.

I would argue that if, for some unfathomable reason, climate-change economists want dependence of damages to be a function of $(\Delta T)^2$, then a far better function at high temperatures for a consumption-reducing, welfare-equivalent, quadratic-based multiplier is the exponential form $\exp(-\gamma(\Delta T)^2)$. Why? Look at the specification choice abstractly. What might be called the "temperature harm" to welfare is arriving here as the arbitrarily imposed quadratic form $H(\Delta T) = (\Delta T)^2$, around which some further structure is built to convert into utility units. With isoelastic utility, the exponential specification is equivalent to $dU/U \propto dH$, while for high H the polynomial specification is equivalent to $dU/U \propto dH/H$. For me it is obvious that, between the two, the former is much superior to the latter. When temperatures are already high in the latter case, why should the impact of dH on dU/U be artificially and unaccountably diluted via dividing dH by high values of H? The same argument applies to any polynomial in ΔT. I cannot prove that my favored choice is the more reasonable of the two functional forms for high ΔT (although I truly believe that it is), but no one can disprove it either—and this is the point here.

The value of γ required for calibrating welfare-equivalent consumption at $\Delta T \approx 2°\text{C}–3°\text{C}$ to be (say) $\approx 97\%–98\%$ of consumption at $\Delta T = 0°\text{C}$ is so miniscule that both the polynomial-quadratic multiplier $1/[1 + \gamma(\Delta T)^2]$ and the exponential-quadratic multiplier $\exp(-\gamma(\Delta T)^2)$ give virtually identical outcomes for relatively small values of $\Delta T \leq 5°\text{C}$, but at ever higher temperatures they gradually, yet ever increasingly, diverge. With a fat-tailed PDF of ΔT and a very large value of the VSL-like parameter λ, there can be a big difference between these two functional forms in the implied willingness to pay (WTP) to avoid or reduce uncertainty in ΔT. When the consumption-reducing, welfare-equivalent damages multiplier has the exponential form $\exp(-\gamma(\Delta T)^2)$, then as the VSL-like parameter $\lambda \to \infty$, a DT-type argument for $\eta > 1$ implies in the limit that the WTP to avoid (or even reduce) fat-tailed uncertainty approaches 100% of consumption. This does not mean, of course, that we should be spending 100% of consumption to eliminate the climate-change problem.

But this limiting example does highlight how a damages specification more reactive to high temperatures (than the standard multiplicative-in-consumption polynomial-quadratic specification) can dominate climate-change CBA when it is combined with fat tails.

A further issue with IAMs is that samplings based upon conventional Monte Carlo simulations of the economics of climate change may give a very misleading picture of the EU consequences of alternative GHG-mitigation policies.[18] The core problem is that while it might be true in *expectations* that utility-equivalent damages of climate change are enormous, when chasing a fat tail this will not be true for the overwhelming bulk of Monte Carlo *realizations.* DT can be approached by a Monte Carlo simulation only as a double limit where the grid-range and the number of runs both go to infinity simultaneously. To see this in a crisp thought experiment, imagine what would happen to the simple stripped-down model of this paper in the hands of a Monte Carlo IAM simulator.

A finite grid may not reveal the true expected stochastic discount factor or true expected discounted utility in simulations of this model (even in the limit of an infinite number of runs) because the most extreme negative impacts in the fattened tails will have been truncated and evaluated at but a single point representing an artificially imposed lower bound on the set of all possible bad outcomes from all conceivable negative impacts. Such arbitrarily imposed *de*

[18] Tol (2003) showed the empirical relevance of this issue in some actual IAM simulations. I am grateful to Richard Carson for suggesting the inclusion of an explicit discussion of why a Monte Carlo simulation may fail to account fully for the implications of uncertain large impacts with small probabilities.

minimis threshold-cutoff truncations are typically justified (when anyone bothers to justify them at all) on the thin-tailed frequentist logic that probabilities of extremely rare events are statistically insignificantly different from 0—and hence can be ignored. This logic might conceivably suffice for known thin tails, but the conclusion is highly erroneous for the rare and unusual class of fat-tailed potentially high-impact economic problems to which climate change seemingly belongs. Back-of-the-envelope calculations cited earlier in this paper appear to indicate that a Monte Carlo simulation of the economics of climate change requires seriously probing into the implications of disastrous temperatures and catastrophic impacts in incremental steps that might conceivably cause up to a 99% (or maybe even much greater) decline of welfare-equivalent consumption before the modeler is allowed to cut off the rest of the bad fat tail in good conscience and discard it. This paper says that any climate-change IAM that does not go out on a limb by explicitly committing to a Monte Carlo simulation that includes the ultra-miniscule but fat-tailed probabilities of super-catastrophic impacts (down to 1%, or even considerably less, of current welfare-equivalent consumption) is in possible violation of best-practice economic analysis because (by ignoring the extreme tails) it could constitute a serious misapplication of EU theory. The policy relevance of any CBA coming out of such a thin-tail-based model might then remain under a very dark cloud until this fat-tail issue is addressed seriously and resolved empirically.[19]

Additionally, a finite sample of Monte Carlo simulations may not reveal true expected utility in this model (even in the limit of an infinite grid) because the restricted sample may not be able to go deep enough into the fat tails where the most extreme damages are. Nor will typical sensitivity analysis necessarily penetrate sufficiently far into the fat-tail region to represent accurately the EU consequences of disastrous damages. For any IAM (which presumably has a core structure resembling the model of this paper), special precautions are required to ensure that Monte Carlo simulations represent accurately the low-utility impacts of fat-tailed PDFs by having the grid-range and the number of runs both be very large.

Instead of the existing IAM emphasis on estimating or simulating economic impacts of the more plausible climate-change scenarios, to at least compensate partially for finite-sample bias the model of this paper calls for a dramatic *over*sampling of those stratified climate-change scenarios associated with the most adverse imaginable economic impacts in the bad fat tail. With limited sampling resources for the big IAMs, Monte Carlo analysis could be used much more creatively—not necessarily to defend a specific policy result, but to experiment seriously in order to find out more about what happens with fat-tailed uncertainty and significant high-temperature damages in the limit as the grid size and number of runs increase simultaneously. Of course this emphasis on sampling climate-change scenarios in proportion to utility-weighted probabilities of occurrence forces us to estimate subjective probabilities down to extraordinarily tiny levels and also to put degree-of-devastation weights on disasters with damage impacts up to perhaps being welfare-equivalent to losing 99% (or possibly even more) of consumption—but that is the price we must be willing to pay for having a genuine economic analysis of potentially catastrophic climate change.

In situations of potentially unlimited damage exposure like climate change, it might be appropriate to emphasize a slightly better treatment of the worst-case fat-tail extremes—and what might be done about them, at what cost—relative to refining the calibration of most-likely outcomes or rehashing point estimates of discount rates (or climate sensitivity). A clear implication of this paper is that greater research effort is relatively ineffectual when targeted at estimating central tendencies of what we already know relatively well about the economics of climate change in the more plausible scenarios. A much more fruitful goal of research might be to aim at understanding even slightly better the deep uncertainty (which potentially permeates the economic analysis) concerning the less plausible scenarios located in the bad fat tail. I also believe that an important complementary research agenda, which stems naturally from the analysis of this paper, is the desperate need to comprehend much better all of the options for dealing with high-impact climate-change extremes. This should include undertaking well-funded detailed studies and experiments about the feasibility, deleterious environmental side effects, and cost-effectiveness of geoengineering options to slim down the bad fat tail quickly as part of emergency preparedness for runaway climate situations if things are beginning to slip out of hand—even while acknowledging that geoengineering might not be appropriate as a first-line defense against greenhouse warming.[20]

[19] Several back-of-the-envelope numerical examples, available upon request, indicate to my own satisfaction that the fat-tail effect is likely to be significant for at least some reasonable parameter values and functional forms. However, serious IAM-based numerical simulations of fat-tail effects on the economics of climate change have not yet been done and are more properly the subject of another more empirical study and paper.

[20] With the unfortunately limited information we currently possess, geoengineering via injection into the stratosphere of sulfate aerosol precursors or other artificially constructed particulates looks superficially like it may be a cheap and effective way to slim down the bad fat tail of high temperatures quickly as an emergency response—although with largely unknown and conceivably nasty unintended consequences that we need to understand much better. For more on the economics and politics of geoengineering (with further references), see, for example, Barrett (2007). In my opinion there is an acute, even desperate, need for a more pragmatic, more open-minded approach to the prospect of climate engineering—along with much more extensive research on (and experimentation with) various geoengineering options for dealing with potential runaway climate change. This research should include studying more seriously and open-mindedly the possible bad side effects on the environment of geoengineering and everything else, as part of a cost-benefit-effectiveness assessment of climate-change strategies that honestly includes the pluses and minuses of all actual policy alternatives and tradeoffs that we realistically face on climate-change options.

When analyzing the economics of climate change, perhaps it might be possible to make back-of-the-envelope comparisons with empirical probabilities and mitigation costs for extreme events in the insurance industry. One might try to compare numbers on, say, a homeowner buying fire insurance (or buying fire-protection devices, or a young adult purchasing life insurance, or others purchasing flood-insurance plans) with cost-benefit estimates of the world buying an insurance policy going some way toward mitigating the extreme high-temperature possibilities. On a U.S. national level, rough comparisons could perhaps be made with the potentially huge payoffs, small probabilities, and significant costs involved in countering terrorism, building antiballistic missile shields, or neutralizing hostile dictatorships possibly harboring weapons of mass destruction. A crude natural metric for calibrating cost estimates of climate-change environmental-insurance policies might be that the U.S. already spends approximately 3% of national income on the cost of a clean environment.[21] All of this having been said, the bind we find ourselves in now on climate change starts from a high-λ, low-k prior situation to begin with, and is characterized by extremely slow convergence in n of inductive knowledge toward resolving the deep uncertainties—relative to the lags and irreversibilities from not acting before structure is more fully identified.

The point of all of this is that economic analysis is not completely helpless in the presence of deep structural uncertainty and potentially unlimited exposure. We can say a few important things about the relevance of thick-tailed CBA to the economics of climate change. The analysis is much more frustrating and much more subjective—and it looks much less conclusive—because it requires some form of speculation (masquerading as an "assessment") about the extreme bad-fat-tail probabilities and utilities. Compared with the thin-tailed case, CBA of fat-tailed potential catastrophes is inclined to favor paying a lot more attention to learning how fat the bad tail might be and—if the tail is discovered to be too heavy for comfort after the learning process—is a *lot* more open to at least considering undertaking serious mitigation measures (including, perhaps, geoengineering in the case of climate change) to slim it down fast. This paying attention to the feasibility of slimming down overweight tails is likely to be a perennial theme in the economic analysis of catastrophes. The key economic questions here are, what is the overall cost of such a tail-slimming weight-loss program and how much of the bad fat does it remove from the overweight tail?

VIII. Conclusion

Last section's heroic attempts at constructive suggestions notwithstanding, it is painfully apparent that the dismal theorem makes economic analysis trickier and more open-ended in the presence of deep structural uncertainty. The economics of fat-tailed catastrophes raises difficult conceptual issues that cause the analysis to appear less scientifically conclusive and more contentiously subjective than what comes out of an empirical CBA of more usual thin-tailed situations. But if this is the way things are with fat tails, then this is the way things are, and it is an inconvenient truth to be lived with rather than a fact to be evaded just because it looks less scientifically objective in cost-benefit applications.

Perhaps in the end the climate-change economist can help most by *not* presenting a cost-benefit estimate for what is inherently a fat-tailed situation with potentially unlimited downside exposure as if it is accurate and objective—and perhaps not even presenting the analysis as if it is an approximation to something that is accurate and objective—but instead by stressing somewhat more openly the fact that such an estimate might conceivably be arbitrarily inaccurate depending upon what is subjectively assumed about the high-temperature damages function along with assumptions about the fatness of the tails and/or where they have been cut off. Even just acknowledging more openly the incredible magnitude of the deep structural uncertainties that are involved in climate-change analysis—and explaining better to policymakers that the artificial crispness conveyed by conventional IAM-based CBAs here is especially and unusually misleading compared with more ordinary non-climate-change CBA situations—might go a long way toward elevating the level of public discourse concerning what to do about global warming. All of this is naturally unsatisfying and not what economists are used to doing, but in rare situations like climate change where DT applies we may be deluding ourselves and others with misplaced concreteness if we think that we are able to deliver anything much more precise than this with even the biggest and most detailed climate-change IAMs as currently constructed and deployed.

The contribution of this paper is to phrase exactly and to present rigorously a basic theoretical principle that holds under positive relative risk aversion and potentially unlimited exposure. In principle, what might be called the catastrophe-insurance aspect of such a fat-tailed unlimited-exposure situation, which can never be fully learned away, can dominate the social-discounting aspect, the pure-risk aspect, and the consumption-smoothing aspect. Even if this principle in and of itself does not provide an easy answer to questions about how much catastrophe insurance to buy (or even an easy answer in practical terms to the question of what exactly *is* catastrophe insurance buying for climate change or other applications), I believe it still might provide a useful way of framing the economic analysis of catastrophes.

[21] U.S. Environmental Protection Agency (1990), executive summary projections for 2000, which I updated and extrapolated to 2007.

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