On Modeling and Interpreting the Economics of Catastrophic Climate Change

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Abstract

Using climate change as a prototype example, this paper analyzes the implications of structural uncertainty for the economics of low-probability high-impact catastrophes. The paper is an application of the idea that having an uncertain multiplicative parameter, which scales or amplifies exogenous shocks and is updated by Bayesian learning, induces a critical "tail fattening" of posterior-predictive distributions. These fattened tails can have very strong implications for situations (like climate change) where a catastrophe is theoretically possible because prior knowledge cannot place sufficiently narrow bounds on overall damages. The essence of the problem is the difficulty of learning extreme-impact tail behavior from finite data alone. At least potentially, the influence on cost-benefit analysis of fat-tailed uncertainty about climate change, coupled with extreme unsureness about high-temperature damages, can outweigh the influence of discounting or anything else.

1 Introduction

What is the abstract analytical essence of the economic problem posed by climate change? The uniqueness of climate change (as an application of economic theory) is not just that today's decisions have difficult-to-reverse impacts that will be felt far into the future, thereby

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straining the concept of time discounting and placing a heavy burden on the choice of an interest rate. Nor does uniqueness come from the pure risk of an objective-frequency climate-change stochastic process with *known structure*. Much more unsettling for an application of expected-present-discounted-utility analysis is the *structural uncertainty* surrounding what is clearly a non-zero subjective-Bayesian probability of a system-wide climate catastrophe. Motivated by the climate-change example, this paper presents a mathematically rigorous economic-statistical model of high-impact low-probability disasters. It also presents some less rigorous back-of-the-envelope numerical calculations to suggest 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, very necessary) thoughts about what this all means for climate-change policy.

In the next section I will argue that, as a very crude approximation which is roughly supported by the latest numbers in the latest scientific studies, if only relatively modest remedies are applied then mean global surface temperature change relative to pre-industrial-revolution levels will *eventually* be greater than 20°C with a ballpark probability estimate somewhere around .01. Societies and ecosystems in a world whose mean temperature has changed in the geologically-instantaneous time of a couple centuries or so by 20°C (for U.S. readers: 20°C = 36°F) are located in *terra incognita*, since such high average temperatures have not existed for at least hundreds of millions of years and such a rate of global temperature change would be unprecedented even on a time scale of billions of years. Standard conventional cost-benefit analysis (CBA) of climate change does not even come remotely close to grappling seriously with this kind of potential for disasters. When CBA is done correctly, by including reasonable probabilities of (and reasonable damages from) catastrophic climate change, the policy implications can be radically different from the conventional advice coming out of a standard economic analysis that (essentially) ignores this kind of potential for disasters.

However it is measured, the planetary welfare effects of climate change that might accompany a 1% chance of a mean temperature change greater than 20°C are sufficiently open-ended and fuzzy that it seems fair to say offhand there might be a non-negligible probability of a catastrophe. In his book *Catastrophe: Risk and Response*,[1] Richard A. 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 respond-

[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.

ing rationally to them." In this paper I address what a 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 highly-uncertain catastrophes with tiny but highly-unknown probabilities. I have four broad conclusions: (1) because of deep structural uncertainty about the prospects for disastrously large temperature changes, there is a strong presumption that probability density functions (PDFs) of climate change catastrophes have tails that are heavy with probability; (2) when these heavy tails are combined with extremely unsure high-temperature damages, this aspect dominates the discounting aspect in calculations of expected present discounted utility (even at empirically-plausible real-world interest rates); (3) this translates into placing severe limitations on the reliability of policy advice from standard CBAs of climate change; (4) removing these artificial limitations on conventional CBAs (that comes essentially from excluding high-impact disasters) can shift the climate-change policy implications of a more inclusive welfare analysis strongly away from relative complacency.

Modeling uncertain catastrophes (such as climate change disasters) presents some *very* strong challenges to economic analysis, the full implications of which have not been adequately confronted. CBA based on expected utility (EU) theory has been applied in practice primarily to cope with uncertainty in the form of known thin-tailed PDFs. 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 a whole lot of 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 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 can have some very strong implications that have neither been recognized in the literature nor incorporated into formal CBA modeling of disasters like climate-change

[2] More technically, as I am using the term in this paper a PDF has a "fat" tail when its moment generating function (MGF) is infinite – i.e., the tail probability approaches zero more slowly than exponentially. The standard example of a fat-tailed PDF is a power-law distribution, although, e.g., a lognormal PDF is also fat-tailed. By this definition a PDF whose MGF is finite has a "thin" tail. A normal PDF is thin-tailed as is *any* truncated PDF.

catastrophes. These implications raise many disturbing yet important questions, which will be dealt with somewhat speculatively in the concluding sections of this paper. Not-fully-answered questions and speculative thoughts aside, this paper will argue it is nevertheless undeniable that, at least in principle, fat-tailed CBA is capable of turning conventional thin-tail-based climate-change policy advice on its head. This paper will show 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 in the extreme fat tails are conceptualized and modeled, an issue that has received minimal treatment thus far in formal models. By implication, the policy advice coming out of conventional thin-tailed CBAs of climate change must be treated with (possibly severe) scepticism until this fat-tail 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 uncertain 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 sure unit of consumption from becoming infinite for any utility function with relative risk aversion everywhere bounded above zero. To close the model in the sense of making expected marginal utility be below $+\infty$ (or expected utility above $-\infty$), this 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 the natural world or civilization as we know these concepts. With this particular 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 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 risk for cost-benefit applications of EU theory to open-ended situations with potentially unlimited exposure. For such situations where there do not exist prior limits on damages (like climate change from greenhouse warming), expected present discounted utility analysis of costs and benefits is likely to be dominated by considerations and concepts related more to catastrophe insurance than to the consumption-smoothing consequences of long-term discounting at one or another particular interest rate.

2 Generalized Climate Sensitivity as a Scaling Factor

The key unknown structural parameter in the abstract model of this paper is a critical multiplier that amplifies (or scales) an uncertain exogenous shock or impulse to the system. The purpose of this section is to motivate heuristically (and to derive some crude ballpark numerical estimates for the tail PDF of) this kind of scaling 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 can be illustrated by the role of an uncertain "climate sensitivity" coefficient in climate-change models and discussions of global warming. Climate sensitivity is an uncertain parameter that serves as a very useful macro-indicator of the *eventual* aggregate response of temperature change to the aggregate level of greenhouse gases (GHGs). Let $\Delta \ln CO_2$ be sustained relative change in concentrations of atmospheric carbon dioxide while ΔT is equilibrium temperature response. Narrowly defined, climate sensitivity (here denoted S_1) is a benchmark amplifying or scaling multiplier for converting $\Delta \ln CO_2$ into ΔT by the (reasonably accurate) linear approximation $\Delta T \approx (S_1/\ln 2) \times \Delta \ln CO_2$. As the Intergovernmental Panel on Climate Change in its IPCC-AR4 (2007) Executive Summary phrases 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 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." (For IPCC-AR4, "likely" is essentially $P > 66\%$, while "very unlikely" is essentially $P < 10\%$.) Climate sensitivity is not nearly the same thing as temperature change, but for the benchmark-serving purposes of my simplistic example I assume that the shapes of both PDFs are very roughly similar because a doubling of anthropogenically-injected CO_2-equivalent (CO_2-e) greenhouse gases (GHGs) relative to pre-industrial-revolution levels is essentially unavoidable within about the next 40 years and is very likely to remain well above this level for at least the subsequent 50-100 years after first attaining it.

In this paper I am mostly concerned with the 17% of those S_1 "values substantially higher than 4.5°C" which "cannot be excluded." A grand total of twenty-two 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 studies cited by IPCC-AR4 are compiled in Table 9.3 and Box 10.5. The 22 studies are perhaps of uneven reliability, 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 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 $P[S_1 > 7°C] \approx 5\%$. From 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 sufficiently fat that one is allowed to presume from a simplistically-aggregated PDF of these 22 studies that $P[S_1 > 10°C] \approx 1\%$. 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.[4]

A very important supplementary component, which conceptually should be added on to climate sensitivity S_1, is the potentially extremely-powerful amplification of greenhouse warming due to heat-induced positive-feedback releases of the immense amounts of GHGs

[3] Details of this calculation are available from the author 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 averages here on 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 study with the single value of 6.4°C.

[4] A more technically-correct explanation might go roughly along the following lines. 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, etc. Attempts to identify S_1 in the 22 studies cited in IPCC-AR4 are roughly akin to measuring $\Delta T / \Delta R_f$ for various values of ΔR_f and subsequent ΔT. The problem is the presence of significant uncertainties in measurements and model parameterizations. This produces a long fat tail in the inferred posterior-predictive PDF of S_1 – for reasons which, while not identical, are structurally similar enough to the simpler generic inference problem of the model of this paper for the latter to stand in as a surrogate for the former (at the high level of abstraction appropriate to getting at the core essence of the welfare-inference problem). More details on "why is climate sensitivity so unpredictable?" are in the recent *Science* article with the same title by Roe and Baker (2007). See also the accompanying commentary by Allen and Frame (2007). Both of these articles about the core essence of the climate-sensitivity inference problem can be interpreted as being congruent with – and supportive of – the main thesis of this paper.

that are currently sequestered in arctic permafrost and other soils (mostly in the form of methane (CH_4), which is a far more potent GHG than CO_2). A yet-more-remote possibility that should also be included is high-temperature-induced releases of the even-much-vaster deposits of offshore methane hydrates (sometimes called clathrates), for which there is a decidedly non-zero probability of eventually escaping into the atmosphere at temperature increases around 7°C-10°C – and whose release could potentially precipitate a horrendously cataclysmic runaway-positive-feedback situation. The very real possibility of endogenous heat-triggered releases at high temperatures of the vast amounts of naturally-sequestered GHGs is a good example of supplementary 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 economic analysis of climate change is the reduced-form relationship between *anthropogenically-injected* CO_2-e (carbon-dioxide-equivalent) GHG levels 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 stands for a more abstract "generalized climate-sensitivity-like scaling parameter" that includes possible heat-induced releases of sequestered GHGs and other such carbon-source and carbon-sink feedbacks on the forcing. The linkage from $\Delta \ln$[anthropogenically-injected CO_2-e GHGs] to eventual ΔT is not linear (it is arguably more likely to be stronger, instead of weaker, than linear), but for the purposes of this highly-aggregated example the linear approximation is good enough. This implies that a doubling of anthropogenically-injected CO_2-e GHGs results in approximate equilibrium 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), which 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, can be used to give some 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 (2007) showed that heat-induced GHG releases add about .067 of gain to the conventional feedback factor, so that (expressed in my language) $S_2 = 1.2 / [1 - g_2]$, where $g_2 = g_1 + .067$. (The .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 .067-coefficient were considerably lower my point would remain.) Doing the calculations, $P[S_1 > 7°C] = 5\% = P[g_1 > .828] = P[g_2 > .896]$, which implies $P[S_2 > 11.5°C] = 5\%$. Likewise, $P[S_1 > 10°C] = 1\% = P[g_1 > .88] = P[g_2 > .947]$, which implies $P[S_2 > 22.6°C] = 1\%$ and presumably corresponds to a scenario where methane is released on a large scale from degraded soils and clathrates. 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]

Of course my calculations and the numbers above can be criticized, but I don't think a climate scientist would say either that they are wrong in principle or that there exists any other method for generating rough estimates of extreme-impact tail probabilities that is clearly superior. Without further ado I am just going to assume for the purposes of this simplistic example that $P[S_2 > 20°C] \approx 1\%$. This implies that a doubling of CO_2-e eventually causes $P[\Delta T > 20°C] \approx 1\%$, which I will take as my base-case tail estimate in what follows. This is a wildly-uncertain crude ballpark estimate of the tiny probability of what amounts to a huge climate impact occurring at some indefinite time in the remote future. However, the subject matter of this paper concerns just such kind of situations and my overly simplistic example here does not depend at all on very 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 hugely-significant part of the climate-change economic-analysis problem, whose strong implications have thus far largely been ignored.

Stabilizing anthropogenically-injected CO_2-e GHG stocks at anything like twice pre-industrial-revolution levels looks now like an extraordinarily ambitious goal. Given current trends in emissions, we will attain such a doubling of anthropogenically-injected CO_2-e GHG levels within about forty years and will then go far beyond that amount unless relatively 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 well within a century from now. 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 there are very long inertial lags in the transmission pipeline converting GHG emissions into temperature increases that might 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 discover some revolutionary new carbon-free energy source or carbon-fixing technological breakthrough. Maybe the future productivity of capital will change dramatically. Perhaps natural carbon-sink sequestration

[5] I am extremely grateful to John Harte for guiding me through these calculations, although he should not be blamed for how I am interpreting or using them in what follows. Although 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). For more details on the role of g_1 in creating a long-tailed PDF in S_1, see Roe and Baker (2007) or Allen and Frame (2007).

processes will turn out to be weaker (or stronger) than we thought. Then again, there is the unknown role of climate engineering. And 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 by sticking with the overly simplistic story that $P[S_2 > 20°C] \approx P[\Delta T > 20°C] \approx 1\%$. I can't know precisely what these numbers 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 $\Delta T > 20°C$ might mean for life on earth, but such high temperatures have not been seen for hundreds of millions of years. Global average warming of 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 this hypothetical temperature change of 20°C (=36°F) would be geologically instantaneous, it would effectively destroy planet Earth as we know it. At a minimum this would trigger mass species extinctions and biosphere ecosystem disintegration matching or exceeding the immense planetary die-offs associated with maybe two or three such previous geoclimate mega-catastrophes in Earth's history. There exist some truly terrifying consequences of mean temperature increases ≈20°C, such as: disintegration of the Greenland and at least the Western part of the Antarctic ice sheets with dramatic raising of sea level by perhaps 30 meters or so, dramatic changes in ocean heat transport associated, e.g., with thermohaline circulation and the Gulf Stream, complete disruption of large-scale weather and precipitation patterns like monsoons, highly consequential worldwide changes in regional freshwater availability and regional desertification – and so forth and so on.

All of the above-mentioned horrifying examples of climate-change mega-disasters are incontrovertibly possible on a time scale 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 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 zero 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 stands in sharp contrast with the conventional wisdom of *not* taking seriously extreme-temperature-change probabilities *because* such probability estimates are highly speculative and statistically indistinguishable from zero. The purpose of this paper is to prove that the exact opposite logic in fact holds by giving a rigorous Bayesian sense in which, other things being equal, the more speculative and fuzzy are the tiny tail probabilities of high-impact extreme events, the less ignorable and the more serious is the situation for a risk-averse agent whose welfare is measured by present discounted expected utility. The seriousness of such situations comes from combining relative risk aversion being strictly bounded above zero with a multiplicative factor (playing a role here analogous to S_2) that effectively has open-ended uncertainty. Such a structure is capable of amplifying impulses into very-bad welfare impacts with very-long and very-fat tails.

Oversimplifying enormously here, how warm the climate ultimately gets is approximately a product of two factors – (some monotone function of) the amount of anthropogenically-injected CO_2-e GHG concentrations and a critical climate-sensitivity-like scaling multiplier. Both of these factors are uncertain, but the scaling parameter is much more open-ended on the high side with a much longer and fatter upper tail. This critical scale parameter reflecting huge scientific tail 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. (Some of the scientific studies of climate sensitivity might approximately be examples of this because they rely on noisy temperature responses from past natural experiments of changed radiative forcings of unsure magnitude.) 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 extreme bad-tail probabilities from finite samples alone because, by definition, we don't get many data-point observations of such catastrophes. Therefore, rare disasters located in the stretched-out fattened tails of such posterior-predictive distributions must inherently contain an irreducibly-large component of deep structural uncertainty. The underlying sampling-theory principle is that the rarer is an event, the more unsure is our estimate of its probability of occurrence. In this spirit (from being constructed out of inductive knowledge), the empirical studies of climate sensitivity are perhaps preordained to find the fat-tailed power-law-like PDFs which they seem, approximately, to find in practice. The paper will show that a generalization of this form of interaction can be re-packaged and analyzed at a high level of abstraction as an aggregative macroeconomic model with essentially the same reduced form (structural uncertainty about some unknown open-ended scale-multiplying 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 easily drive applications of expected utility (EU) theory much more than anything else, including discounting.

When fed back 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 utility or welfare. There exists here a very long chain of tenuous inferences fraught with hugely-uncertain links every step of the way. Uncertainties about how available policy levers translate into GHG emissions are further compounded by uncertainties about how GHG-flow emissions transmit via the carbon cycle into GHG-stock concentrations, are further compounded by uncertainties about how GHG-stock concentrations transmit into global mean temperature changes, are further compounded by uncertainties about how global mean temperature changes translate into regional climate changes, are further compounded by uncertainties about how regional climate changes are translated into regional welfare changes, are further compounded by uncertainties about how regional welfare changes are aggregated into global welfare changes. The result of this immense cascading of uncertainties is a "reduced form" of enormous uncertainty about the overall aggregate-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 what might be called "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 zero* to temperature changes being above some critical physical constant of nature analogous to the speed of light. (This is not true incidentally, because the true situation is more like a case of the ever-tinier probabilities associated with ever-higher temperatures trailing off asymptotically to zero than it is like a case of hitting exactly zero probability at some empirically-usable *upper bound* physical constant analogous to the natural *lower bound* climate sensitivity given by zero.) *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 very high temperature changes makes the relevant reduced-form criterion of "welfare sensitivity" to a fat-tailed generalized scaling parameter seem almost unbelievably uncertain for very high temperatures – to the point of being essentially unbounded.

3 A Disturbing Example

Let C be consumption. Consider a representative agent with a standard familiar CRRA (constant relative risk aversion) utility function of the form

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

for $C > 0$, which implies that

$$U'(C) = C^{-\eta}. \tag{2}$$

Technically speaking, the model of this paper requires only that the coefficient of relative risk aversion η is positive. But the main application here involves a situation where the value of statistical life is extremely large, which, it will turn out, is essentially only consistent with $\eta > 1$. Although the condition $\eta > 1$ can be *derived* from the model (and later it will be), analytically it is easier just to assume it in the first place.

For analytical crispness, the model of this paper has only two periods – the present and the future. Applied to climate change, the future might very roughly be interpreted as about two centuries hence. Such a sharp formulation downplays the ability to learn and adapt gradually over time but I believe the key insights of this model would remain, *mutatis mutandus*, for most applications – including the economics of climate change with a realistically-long inertial time lag between emitted GHGs and eventual ΔT.

Instead of working directly with 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

$$Y \equiv \ln C, \tag{3}$$

where in this model Y is a random variable capturing *all* uncertainty that influences future values of $\ln C$. For the purposes of this paper, Y includes not just economic growth narrowly defined, but also the consumption-equivalent damages of adverse climate change. Actually, this paper is mostly concerned with the extraordinarily-small probability of an extraordinarily-large negative realization of Y that might accompany extreme climate change. Starting here and continuing throughout the rest of 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 welfare-equivalent C and uncertain ΔT in the background. Thus, the random variable Y is to be interpreted as implicitly being some monotone-decreasing function of the random variable ΔT of form $Y = F(\Delta T)$ with $F'(\Delta T) < 0$, so that (3) means $C = \exp(F(\Delta T))$. For this paper I take $F(\Delta T)$ to be of the linear form $F(\Delta T) = A - B\,\Delta T$ with positive constants A and B, but it could just as easily be of the quadratic form $F(\Delta T) = A - B\,(\Delta T)^2$ or of any form with $F'(\Delta T) < 0$. In order to cut sharply to the core essence of the economics-of-climate-change structural uncertainty problem, I treat the situation simplistically at a high level of abstraction *as if* we can make inferences about the indirect effect of ΔT on C via direct observations of realizations of Y, which are incorporated in a Bayesian-updated reduced-form posterior-predictive PDF of Y.

The "stochastic discount factor" or "pricing kernel" is an expression of the form

$$M(C) \;=\; \beta\,\frac{U'(C)}{U'(1)} \tag{4}$$

for time-preference parameter β $(0 < \beta \leq 1)$. When (2) holds, then from (4) the amount of present consumption that 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

$$E[M] \;=\; \beta E[\exp(-\eta Y)], \tag{5}$$

which is a kind of shadow price for discounting future costs and benefits in project analysis.

Throughout this paper I use the 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 – give similar results, but the required analysis in terms of mean-preserving spreads 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 cost-benefit or welfare analysis more generally in situations of potentially unlimited exposure to catastrophic impacts.

Using standard notation, let lower-case y denote a realization of the upper-case random variable Y. If Y has PDF $f(y)$, then the price of future consumption (5) can be written as

$$E[M] \;=\; \beta \int_{-\infty}^{\infty} e^{-\eta y} f(y)\, dy, \tag{6}$$

which means that $E[M]$ is "essentially" the Laplace transform or moment-generating function (MGF) of $f(y)$. This is helpful because the properties of the expected stochastic discount factor are the same as the properties of the MGF of a probability distribution, about which a great deal is already understood. For example, if $Y \sim N(\mu, s^2)$ then plugging the usual formula for the expectation of a lognormal random variable into (6) gives the familiar expression

$$E[M] \;=\; \exp\left(-\delta - \eta\mu + \frac{1}{2}\eta^2 s^2\right), \tag{7}$$

where $\delta = -\ln\beta$ is the instantaneous rate of pure time preference. Expression (7) shows up in innumerable asset-pricing Euler-equation applications as the expected value of the stochastic discount factor or pricing kernel when consumption is lognormally distributed. Equation (7) is also the basis of the well-known generalized-Ramsey formula for the riskfree interest rate

$$r^f \;=\; \delta + \eta\mu - \frac{1}{2}\eta^2 s^2, \tag{8}$$

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 (8) is theoretically overshadowed by the effect of an uncertain scaling parameter s.

Although not phrased in exactly this way, the existing literature already contains an example that can be interpreted as showing a sense in which EU-maximizing agents are significantly more averse to structural "uncertainty" (meaning here a situation where the structure of the data generating process is unknown and must be estimated statistically) than they are to pure "risk" (the structure of the data generating process is known, but future stochastic realizations are unknown – as in (8) where $Y \sim N(\mu, s^2)$ with both parameters known). The example used here to convey this basic idea is a relatively simple specification consisting of the workhorse isoelastic or CRRA utility function (1) along with familiar probability distributions: lognormal, Student-t, gamma.[6] One may then ask whether the

[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

insight that structural "uncertainty" has potentially more impact on EU analysis than pure "risk" is due here to the particular quirks of this relatively simple example or, alternatively, it represents a generic insight of broader scope. The answer, given in the next section, is that the result is generic (or at least much broader than the example). The relatively simple formulation of this section will then help to motivate the subsequent development of a more general theory of catastrophic change involving structural uncertainty about the true value of the relevant scale parameter.

Throughout this paper, the structural scale parameter controlling the tail spread of a probability distribution is the most critical unknown. For convenience in the super-simple example of this section, it is assumed that $Y \sim N(\mu, s^2)$ where the mean μ *is known* but the scale parameter s *is unknown.* In an extremely loose sense this unknown structural scale parameter is a highly-stylized abstraction of the effect that is embodied in an uncertain generalized climate-sensitivity-like amplifying multiplier like S_2, which was discussed in the previous section of the paper. With this rough analogy in this super-simple example, $Y \leftrightarrow A - B\Delta T$, where A and B are positive constants.

The structural uncertainty concerning s is modeled here *as if* this scale parameter is a subjectively-distributed random variable (denoted S) whose PDF must be inferred by inductive reasoning from n observed data points. At a very high level of abstraction, these data points might be interpreted as outcomes from various economic-scientific studies very roughly akin to the eighteen climate-sensitivity studies discussed in the introduction. Let $\mathbf{y} = (y_1, ..., y_n)$ be a sample of n i.i.d. random draws from the data-generating process of the normal distribution whose PDF is

$$h(y \mid s) = \frac{1}{\sqrt{2\pi}s} \exp\left(-\frac{(y-\mu)^2}{2s^2}\right). \tag{9}$$

The sample variance is

$$\nu_n \equiv \frac{1}{n}\sum_{j=1}^{n}(y_j - \mu)^2 \tag{10}$$

and the likelihood function for the random variable S here is

$$L(s; \mathbf{y}) \propto \frac{1}{s^n} \exp\left(-\frac{n\nu_n}{2s^2}\right). \tag{11}$$

Geweke (2001). Weitzman (2007a) extended this example to a nonergodic evolutionary stochastic process and developed the 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 sufficiently adequately conveyed by the simpler stationary case. The same comment applies to modeling the PDFs of S_1 or 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.

In a Bayesian framework, deriving the agent's posterior distribution of S requires that some prior distribution be imposed on S. The choice of a "noninformative" prior in the most *general* case represents a thorny issue within Bayesian statistics. However, for the *particular* case of a one-dimensional scale parameter, which is the case here, practically all definitions of a "noninformative" prior give the same PDF. This traditional reference prior forms the Bayesian mirror image of classical linear-normal regression analysis. This prior (called Jeffreys prior, which is explained in any textbook on Bayesian statistics) is a diffuse or uniform distribution of $\ln S$ on $(0, \infty)$, meaning that the (improper) prior PDF expressed in terms of s is

$$p_0(s) \propto \frac{1}{s}. \tag{12}$$

The "precision," θ, is commonly defined to be the reciprocal of the variance, so that

$$\theta \equiv \frac{1}{s^2}. \tag{13}$$

The posterior probability density is proportional to the product of the prior $p_0(s)$ times the likelihood $L(s; \mathbf{y})$. When the change of variables (13) is plugged into the product of (12) times (11), the posterior becomes conveniently expressed in terms of θ by the gamma distribution

$$\Gamma(\theta) \propto \theta^{a-1} \exp(-b\theta), \tag{14}$$

where

$$a = \frac{n}{2} \tag{15}$$

and

$$b = \frac{n\nu_n}{2}. \tag{16}$$

The mean of the gamma distribution (14) is a/b while its variance is a/b^2, so that from (15) and (16),

$$E[\theta \mid \mathbf{y}] = \frac{1}{\nu_n} \tag{17}$$

while

$$V[\theta \mid \mathbf{y}] = \left(\frac{2}{\nu_n}\right)\frac{1}{n}. \tag{18}$$

After integrating out the precision from the conditional-normal distribution (9), the unconditional or marginal posterior-predictive PDF of the growth rate Y is

$$f(y) \;\propto\; \int_0^\infty \sqrt{\theta}\,\exp(-\,\theta(y-\mu)^2/2)\;\Gamma(\theta)\,d\theta\,. \tag{19}$$

Straightforward brute-force integration of (19) (for (14), (15), (16)) shows that $f(y)$ is the Student-t distribution with n degrees of freedom:

$$f(y) \;\propto\; \left(1+\frac{(y-\mu)^2}{n\,\nu_n}\right)^{-(n+1)/2}. \tag{20}$$

(Any Bayesian textbook shows that a normal with gamma precision becomes a Student-t). Note that, asymptotically, the limiting tail behavior of (20) is a fat-tailed power-law PDF whose exponent is $n+1$. In the next section of the paper, this same kind of power-law-tail result will be shown to hold much more generally than the particular example of this section.

When the posterior-predictive distribution of Y is (20) (from s being unknown), then (6) becomes

$$E[M] \;=\; +\infty, \tag{21}$$

because the MGF of a Student-t distribution is infinite. What accounts technically for the economically-stunning counterintuitiveness of the finding (21) is a form of pointwise but nonuniform convergence. When $n\to\infty$ in (20), $f(y)$ becomes the familiar normal form $\exp(-(y-\mu)^2/2\nu_\infty^2)$, which then, as $y\to-\infty$, approaches zero faster than $\exp(-\eta y)$ approaches infinity, thereby leading to the well-known finite formula (7) for $E[M]$. Given any fixed n, on the other hand, as $y\to-\infty$ expression (20) tends to zero only as fast as the power-law polynomial $(-y)^{-n-1}$, so that now in formula (6) 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 (7) or (8). Yet when it is acknowledged that s is unknown (with a standard noninformative reference prior) and its value in formula (7) or (8) must instead be inferred as if from a data sample that can be *arbitrarily large* (but finite), expected marginal utility explodes in (21). 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?

I want to emphasize as emphatically as I can at this relatively early stage of the paper that the problem (both here *and throughout the rest of the paper*) is *not* a mathematically illegitimate use of the symbol ∞ in formula (21), 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, or 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 ∞ by an arbitrarily-large but finite number. Indeed, the model of this paper will be closed later 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 (or even marginalize) 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 ∞ symbol. One can easily remove the ∞ in formula (21), 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 by making what seem like 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 non-robustly upon artificial constraints or parameter settings that we really do not understand. The only legitimate way to avoid this potential problem is when there exists strong *a priori* knowledge that contains the damages (e.g., as with limited downside exposure from an independently distributed small component of a consumption-wealth portfolio, which is naturally bounded from below by zero).

What is happening in this example is a particular instance of a general idea. To repeat: *the* core underlying problem is the difficulty of learning limiting tail behavior inductively from finite data. Seemingly thin-tailed probability distributions (like here the normal), which are actually only thin-tailed *conditional on known structural parameters* of the model, become tail-fattened (like here the Student-t) after integrating out the uncertainty. This core issue cannot be eliminated in any clean way, and with unlimited downside exposure it must influence any utility function that is sensitive to low values of consumption. It is important to grasp that utility isoelasticity *per se* is inessential to the reasoning here (although it makes the argument easier to understand), because the expected stochastic discount factor $E[M]$ is $+\infty$ in this setup for *any* relatively-risk-averse utility function satisfying the curvature requirement: $\inf_{C>0}[-CU''(C)/U'(C)] > 0$.

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 zero. Intuitively, a 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 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 strictly positive relative risk aversion. One obvious technical way for the model to contain this scary "Student-t explosion" (or, as will later be shown more generally, "power-law-tail explosion") is to exclude it *a priori* by imposing some kind or another of an absolute lower bound $D > 0$ on allowed values of $C \geq D$.

The next issue to be investigated is the extent to which this particular example generalizes. It turns out that there is a rigorous sense in which this scary fattened-posterior-tail effect holds for (essentially) *any* probability distribution characterized by having an uncertain scale parameter. When indeterminateness is compounded by probability distributions themselves having probability distributions, then posterior-predictive tails must inevitably become fattened – with potentially strong consequences for some important applications of EU theory, including the economic analysis of climate change.

4 The General Model

In order to focus sharply on structural parameter uncertainty, the model of this section is patterned closely as a generalization of last section's example and is deliberately sparse. To create families of probability distributions that are simultaneously fairly general and analytically tractable, the following generating mechanism is employed. Suppose Z represents a random variable normalized to have mean zero and variance one. Let $\phi(z)$ be *any* piecewise-continuous PDF with

$$\int_{-\infty}^{\infty} z\,\phi(z)\,dz \;=\; 0 \tag{22}$$

and with

$$\int_{-\infty}^{\infty} z^2\,\phi(z)\,dz \;=\; 1. \tag{23}$$

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 extremely weak regularity or technical conditions that $\phi(z)$ is strictly positive within some neighborhood of $z = 0$, and that

$$\int_{-\infty}^{\infty} \exp(-\alpha z)\,\phi(z)\,dz \;<\; \infty \tag{24}$$

for all $\alpha > 0$, which is automatically satisfied if Z has finite lower support.

With $s > 0$ given, make the change of random variable of the linear form $y = sz + \mu$, which implies that the conditional PDF of y is

$$h(y \mid s) \;=\; \frac{1}{s}\,\phi\left(\frac{y-\mu}{s}\right), \tag{25}$$

where s and μ are structural parameters having the interpretations

$$\mu \;=\; E[Y \mid s] \;=\; \int_{-\infty}^{\infty} y\,h(y \mid s)\;dy \tag{26}$$

and

$$s^2 \;=\; V[Y \mid s] \;=\; \int_{-\infty}^{\infty} (y-\mu)^2\;h(y \mid s)\;dy. \tag{27}$$

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 conceptualized extremely loosely as a highly-stylized abstract generalization of a climate-sensitivity-like amplifying or scaling multiplier like S_2. (In this crude analogy, $Z \leftrightarrow \Delta \ln CO_2/\ln 2$, $SZ \leftrightarrow \Delta T$, $Y \leftrightarrow A - B\Delta T$.) Without significant loss of generality, it is assumed for ease of exposition that in (25) 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 $h(y \mid s)$ given by (25) is known to the agent 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 by some process of inductive reasoning. Suppose that the agent has observed the random sample $\mathbf{y} = (y_1, ..., y_n)$ of growth-rate data realizations from n independent draws of the distribution $h(y \mid s)$ defined by (25) 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 $\mathbf{y} = (y_1, ..., y_n)$ 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.

From (25) the relevant likelihood function of s is

$$L(s; \mathbf{y}) \;\propto\; \prod_{j=1}^{n} h(y_j \mid s). \tag{28}$$

The prior PDF of S is taken to be a generalization of (12) of the form

$$p_0(s) \;\propto\; s^{-k} \tag{29}$$

for any number k. (The number k is crudely identifiable with the strength of prior knowledge in the same spirit as n is crudely identifiable with the strength of inductive knowledge.) Since k can be chosen to be *arbitrarily large*, the non-dogmatic prior distribution (29) can be made to place *arbitrarily small* prior probability weight on big values of s. It should be appreciated that *any invariant prior must be of the form* (29). Invariance (discussed in the Bayesian-statistical literature) is considered desirable as a description of a "noninformative" reference prior that favors no particular value of the scaling parameter s over any other. For such a noninformative reference prior, it seems reasonable to impose a condition of scale invariance that might be justified by the following kind of reasoning. If the action taken in a decision problem should not depend upon the unit of measurement, then a plausible principle of scale-parameter invariance might ask of a prior representing a state of ignorance that

$$p_0(s) \ \propto \ p_0(\alpha s), \tag{30}$$

and the only way that (30) can then hold for all $\alpha > 0$ and all $s > 0$ is when the (necessarily improper) PDF is of the form (29).

The posterior probability density $p_n(s \mid y)$ is proportional to the product of the prior $p_0(s)$ from (29) multiplied by the likelihood $L(s; \mathbf{y})$ from (28), which here yields

$$p_n(s \mid \mathbf{y}) \ \propto \ p_0(s) \prod_{j=1}^{n} h(y_j \mid s). \tag{31}$$

Integrating out the agent's uncertainty about s described by the probability density (31), the unconditional or marginal posterior-predictive density of the growth-rate random variable Y is

$$f(y) \ = \ \int_0^\infty h(y \mid s) \ p_n(s \mid \mathbf{y}) \ ds, \tag{32}$$

and (6) then becomes

$$E[M] \ = \ \beta \int_{-\infty}^{\infty} e^{-\eta y} \, f(y) \, dy. \tag{33}$$

5 The Key Role of a "VSL-like Parameter"

To jump ahead of the story just a bit, the general model of Section 4 has essentially the same unsettling property as the disturbing example of Section 3 – namely that $E[M]$ is unbounded. 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 mechanism. In this paper I close the model by placing an *ad hoc* positive lower bound on consumption, which is denoted D (for "death"). This lower bound D is not *completely* arbitrary, however, because it can be related conceptually to a kind of "value of statistical life" (VSL) parameter. This has the advantage of tying conclusions to a familiar economic concept whose ballpark estimates can at least convey some very 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 life on earth as we know it, or perhaps the value of statistical civilization as we know it. In this paper I am 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. I am not trying to argue that a VSL-like parameter naturally pops up as a great candidate for closing this model – I am just saying that it is better than the alternatives.

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 here is essentially identical (but with a different purpose in a different context) to a specification used recently by Hall and Jones (2007), which is supported by being broadly consistent with a wide array of stylized facts about health spending and empirical VSL estimates.

The basic idea in terms of an application to my model here 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 people might make private trade-offs between decreased consumption as against a lower probability of their own personally-catastrophic end – which they do all the time. However fanciful 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 fanciful, more arbitrary, and more unnatural. 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 who is trading off increased consumption against a slightly increased probability of death.

Suppose D is some disastrously low value of consumption, conceptualized as representing some kind of an analogue of a starvation level, below which level the individual dies. Let the utility associated with death be normalized at zero. The utility function $U(C;D)$ is chosen to be of the analytically convenient form

$$U(C;D) \;=\; \frac{C^{1-\eta}-D^{1-\eta}}{1-\eta} \tag{34}$$

for $C \geq D$, and

$$U(C;D) \;=\; 0 \tag{35}$$

for $0 \leq C < D$.

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

$$(1-q)\, U(1+A(q);D) \;=\; U(1;D). \tag{36}$$

Differentiating (36) with respect to q and evaluating at q=0 gives

$$-U(1;D) + U'(1;D)A'(0) \;=\; 0. \tag{37}$$

The "value of statistical life" (or, in the terminology here, the "VSL-like parameter") is defined as the rate of substitution between consumption and mortality risk, here being

$$\lambda \;\equiv\; A'(0). \tag{38}$$

Equations (37), (38) can be inverted to give the implied lower bound on consumption D as an implicit function of the VSL-like parameter λ. Proceeding this way for the isoelastic utility function (1) yields, after manipulation, the equation

$$D(\lambda) \;=\; [1+(\eta-1)\lambda]^{-1/(\eta-1)}. \tag{39}$$

In this paper I am interested in analyzing what happens for very large values of λ. In order for equation (39) to make sense in such situations by ensuring $D(\lambda) > 0$ requires here that

$$1+(\eta-1)\lambda \;>\; 0, \tag{40}$$

which can "essentially" (up to limiting measure zero) hold for arbitrarily large values of λ

only if

$$\eta > 1. \tag{41}$$

In other words, for what it is worth the theory here is telling us that the large empirical values of statistical life (relative to consumption), which seem to characterize our world, are "essentially" compatible only with relative risk aversion implied by (41) being above one.

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.[7] From a wide variety of empirical studies in disparate contexts, a plausible value of the coefficient of relative risk aversion might be two.[8] For η=2, the value $\lambda \approx 100$ plugged into formula (39) gives $D(100) \approx .01$. An interpretation of λ as a parameter representing the per-capita value of statistical civilization or the value of statistical life on earth (as we currently know or understand these concepts) may very well involve much higher values of λ than ≈ 100 and therefore much lower values of D than $\approx .01$. In any event, I note here for later reference that a Monte Carlo simulation assessing the EU impacts of losing up to 99% or more of welfare-equivalent consumption in the far-distant dark reaches of a bad fat tail is very different from any simulations now being done with any existing empirical model or study of the economics of climate change.

Let $E[M \mid \lambda]$ represent the expected value of the stochastic discount factor for $M(C)$ given by formula (4) when $C \geq D(\lambda)$ and with $M(C)$=0 when $0 \leq C < D(\lambda)$. This corresponds to $M(C) \equiv 0$ throughout the interval $0 \leq Y < \ln D(\lambda)$. The next section explores what happens to $E[M \mid \lambda]$ for extremely large values of λ.

6 The Dismal Theorem

The following "Dismal Theorem" (hereafter sometimes abbreviated as "DT") shows that $E[M] \to +\infty$ under quite general circumstances when there is structural uncertainty concerning the unknown scaling parameter s – *and* when λ might be *very* big.

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

$$\lim_{\lambda\to\infty} E[M \mid \lambda] \;=\; +\infty. \tag{42}$$

[7] 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, Dione, and Lebeau (2007) or the survey of Viscusi and Aldi (2003).

[8] This is the point estimate 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.

Proof. [9]Combining the interpretation of $D(\lambda)$ with the appropriate modifications of (33), (32) – and tracing the link of equations all the way back to (25) – implies that

$$E[M \mid \lambda] \propto \int_0^\infty \frac{1}{s^{k+n+1}} \prod_{j=1}^{n} \phi\left(\frac{y_j - \mu}{s}\right) \left[\int_{\ln D(\lambda)}^{\infty} e^{-\eta y}\, \phi\left(\frac{y-\mu}{s}\right) dy \right] ds. \tag{43}$$

Make the change of variable $z = (y - \mu)/s$, use the fact from (39) that $D(\infty) = 0$, and reverse the order of integration to rewrite (43) as

$$\lim_{\lambda \to \infty} E[M \mid \lambda] \propto \int_{-\infty}^{\infty} \phi(z) \left[\int_0^\infty e^{-\eta z s} \frac{1}{s^{k+n}} \prod_{j=1}^{n} \phi\left(\frac{y_j - \mu}{s}\right) ds \right] dz. \tag{44}$$

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

$$\lim_{s \to \infty} \left\{ e^{-\eta z' s} \frac{1}{s^{k+n}} \right\} = +\infty, \tag{45}$$

implying that expression (44) also approaches $+\infty$ as $\lambda \to \infty$, which concludes this basic sketch of a proof. ■

The underlying logic behind the strong result of Theorem 1 is described by the limiting behavior of (45) 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 (45), 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 random variable Y essentially behaves like the tail of the random variable 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 whose power coefficient from (45) is $n+k$. In this sense, power-law tails need not be postulated, because they are essentially unavoidable in posterior-predictive distributions. No matter the number of observations, the race to the bottom of the bad tail between a polynomially-contracting power-law probability times an exponentially-expanding marginal utility impact is won in the limit every time by the marginal utility impact – with *any* utility function for which relative

[9]This is only a loose sketch of the structure of a proof. It is being included here primarily to provide *some* motivation for the formulas in the analysis that comes next and depend upon equation (45). In this spirit, the purpose of this "proof sketch" is to give at least a minimal quick-and-dirty indication of where (45) is coming from. A rigorous proof can be built around the very significant (perhaps even seminal) contribution of Michael Shwarz to decision making under extreme uncertainty. An important result proved in Schwarz (1999) is that the tails of $f(y)$ defined by (32) are power-law of order $n+k$. From this fact, a rigorous proof of Theorem 1 then proceeds along the lines sketched here.

risk aversion is positive in the limit.[10] However, it is also true that 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.

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 $p_0(s)$ defined by (29) assigns positive probability to an open interval around s^*, the imposed specification has sufficient regularity for large-sample likelihood dominance to cause strong (i.e., almost sure) convergence of the posterior distribution (31) of S to its true data-generating value $s = s^*$. This in turn means that the posterior-predictive distribution of growth rates (32) converges strongly to its true data-generating distribution $h(y \mid s^*)$ and (for *any given* $\lambda<\infty$) $E[M \mid \lambda]$ converges strongly to its true value:

$$n \to \infty \quad \Longrightarrow \quad E[M \mid \lambda] \underset{a.s.}{\longrightarrow} \beta \int_{-\infty}^{\infty} e^{-\eta y} \frac{1}{s^*} \phi\left(\frac{y-\mu}{s^*}\right) dy. \tag{46}$$

Condition (46) signifies that for *any given* $\lambda < \infty$ (which via (39) puts a positive lower bound $D(\lambda)$ on C, and thereby a finite upper bound on M), in the limit as full structural knowledge is approached (because $n \to \infty$), $E[M \mid \lambda]$ 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-tail EU theory essentially corresponds to a situation where there is enough 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 the conventional parameters β and η, we have at least some very rough idea of what kinds of numbers might be empirically relevant. (For example, reasonable point estimates might involve values of β consistent with $\approx 99\%$ per year and η consistent with ≈ 2.) In complete contrast, any discussion about climate change concerning the empirically-

[10] As stated here, DT depends upon an invariant prior of the polynomial form (29), but this is not much of a limitation because k can be *any number*. To undo the infinite limit in (42) requires a *noninvariant* prior that additionally approaches zero faster than *any polynomial in* $1/s$ (as $s \to \infty$). In such a case the limit in (42) 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.

relevant value of the nonconventional VSL-like parameter λ (relative to $n+k$) belongs to a much more abstract realm of discourse. It is therefore understandable to want climate-change CBA to be restricted to dealing with worst-conceivable damages of, say for example, $\approx$20% of consumption (equivalent to picking $D \approx 4/5$) – with anything worse than this being disregarded (as being "too speculative" or "not based on hard science") by chopping off the rest of the bad tail, discarding it, and then forgetting about it. This is actually 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 uncertainty concerning 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 (relative to $n+k$) 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 the 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.

In the spirit of the above discussion, it is critical here to note that with (46) the a.s. convergence of $E[M \mid \lambda]$ 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 $E[M \mid \lambda]$ is always exceedingly sensitive to very large values of λ. If "risk" means that the data generating process is known exactly (only the outcome is random), while "uncertainty" means that (as well as the outcome being random) the parameters of the data generating process (like S) are unknown and must be estimated statistically, then DT can be interpreted as saying that structural "uncertainty" always has the potential to trump pure "risk" for situations of potentially-unlimited downside exposure when no plausible bound $D(\lambda)>0$ can confidently be imposed by prior knowledge. The Dismal Theorem 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 catastrophes coexisting with a 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 has both a general point and 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 the expected marginal utility of an extra sure unit of consumption become independent of the VSL-like parameter – thereby potentially allowing this VSL-like-parameter aspect to dominate cost-benefit applications of EU theory. To drive home the main theme of this paper yet again, the underlying general problem that DT is illustrating concerns a fundamental limitation on the ability of empirical learning or inductive knowledge to shed light on extreme events. Even in a stationary world, it is not possible to learn enough about the frequency of tail events from finite samples alone to make expected stochastic discount factors be independent of artificially-imposed bounds on the extent of possibly-ruinous disasters.[11]

The part of the distribution of possible outcomes that can most readily be learned (from inductive information that comes in 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 perhaps even 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 PDF. As we move towards probabilities in the periphery of the distribution, however, we are increasingly moving into the unknown territory of subjective uncertainty where our probability 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. Climate change generally and climate sensitivity specifically are prototype examples of this general principle, because we are trying to extrapolate inductive knowledge far outside the range of limited past experience from natural forcing experiments or computer runs of general circulation climate models with point-calibrated parameterizations. Since the unknown scale parameter S (whose uncertainty drives the economic analysis) is an abstract generalized version of an amplifying multiplier whose role is very crudely analogous to the role of climate sensitivity, it is then perhaps no accident or surprise that the empirical PDF of climate sensitivity (which is, after all, based ultimately on inductive information from data and studies) seemingly has a thick power-law-type tail.

7 What is the Dismal Theorem Trying to Tell Us?

A common reaction to the conundrum for CBA implied by the Dismal Theorem is to acknowledge its mathematical logic but to wonder how it is to be used constructively for deciding what to do in practice. After all, horror stories about theoretically-possible hypothetical disasters can be told for many situations without this aspect necessarily paralyzing decision-making (or freezing the status quo by effectively blocking all progress whenever there exists

[11] This point 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).

a theoretical possibility of severe downside risks).

The Dismal Theorem says that in the limit as the VSL-like parameter becomes *very* large, agents are willing to pay a *very* high price to eliminate or reduce deep uncertainty – a somewhat vexing idea that is difficult to wrap one's mind around. Is this an incompleteness result showing that the familiar ordinary form of CBA based on EU theory with known thin tails is somehow nullified by a severe restriction on what inductive information can teach us about basic structural uncertainty? Phrased differently, is DT an economics version of an impossibility theorem which signifies that there are fat-tailed situations where economic analysis is up against a 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 on *some* explicit numerical estimates of the miniscule probabilities of all levels of catastrophic impacts up to absolute disaster? And if ordinary thin-tailed CBA is thereby constrained, then what are we supposed to use in its place? 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? (Should we have foregone the industrial revolution because of the GHGs it generated?) Considering the enormous inertias that are involved in the buildup of GHGs and its warming consequences, is the possibility of learning and mid-course corrections a plausible counterweight to DT? How should the fat tail of climate uncertainty be compared with the fat tails of various proposed solutions such as nuclear power, geoengineering, or carbon sequestration at the bottom of the ocean? With such a severe barrier to what we are able to say about extreme events in a situation of deep structural uncertainty (when it is combined with practically unlimited potential for downside exposure), what should we do when *some* kind of a policy response to climate change is required *now*? *Ceterus paribus*, DT suggests a more cautious approach to GHG emissions, but *how much* more caution is warranted?

I simply do not know the full answers to the above wide range of legitimate questions that DT raises. I don't think anyone does. But I also don't think that such questions can just be brushed aside in good conscience. When uncertainty is formally considered at all in climate change, the artificial practice of using thin-tailed PDFs – especially the widespread practice of imposing *de mimimis* low-probability-threshold cutoffs that casually dictate what part of the high-impact bad tail is to be truncated and discarded from CBA – seems (to me) arbitrary and problematic.[12] In the spirit of the idea that the unsettling issues raised by fat-tailed CBA for the economics of climate change *must* be addressed seriously, even while

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

admitting that we do not now know all the answers, I tentatively offer here some speculative thoughts on what it all means. Even if the quantitative magnitude of what DT implies for climate-change policy is as yet 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 unusual (for economics) nonuniform convergence of $E[M]$ or $E[U]$ in all of 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 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 DT might apply. In ordinary run-of-the-mill limited-exposure 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 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 the Dismal Theorem might apply, CBA based upon empirically-reasonable functional forms and parameter values (including λ) might reveal useful information. At the same time, 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 in any event just another parameter whose value is 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 some sort of 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-tail disasters of the order of magnitude of anything remotely resembling $P[\Delta T > 20°\text{C}] \approx 1\%$ 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.

There exist perhaps half a dozen or so serious "nightmare scenarios" of environmental disasters perhaps comparable in conceivable worst-case impact to catastrophic climate change. These might include: biotechnology, nanotechnology, asteroids, strangelets, pandemics, runaway computer systems, nuclear proliferation.[13] 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. Let me illustrate some of what I have in mind with two specific examples. The first example is the widespread cultivation of crops based upon bioengineered genetically-modified organisms (GMOs). At first glance, climate-change catastrophes and bioengineering disasters might casually appear to be similar. In both cases, there is deep unease about artificial tinkering with the natural environment, which can generate frightening tales of a world turned upside down and a planet that has been ruined by human activity. Suppose for the sake of specificity that in the case of GMOs the overarching fear of disaster concerns the possibility that widespread cultivation of so-called "Frankenfood" could 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.

I think that 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 the extent of catastrophic jumping of artificial DNA from cultivars to landraces. After all, nature herself has already tried endless combinations of mutated DNA and genes over

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

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 perhaps comparable to large-step human-induced artificial modifications – and these natural small-step modifications have already been shown not to have survival value in the wild. I think that analogous arguments may also apply for invasive "superweeds," which (at least so far) represent a minor cultivation problem and have not shown any ability to displace landraces, much less cultivars. Besides all this, and importantly in this context, safeguards in the form of so-called "terminator genes" can be inserted into the DNA of GMOs, which directly prevents 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 values. If we use $P[\Delta T > 20°\mathrm{C}] \approx 1\%$ as the very rough probability of a climate-change disaster occurring within the next two centuries, then this is roughly four orders of magnitude 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 the plausible limits of the magnitude or probability of a disaster in the case of genetic engineering or asteroid collisions with the situation of possibly-catastrophic climate change. The climate-change "experiment," whose eventual outcome we are trying to infer now, concerns the planet's response to a geologically-instantaneous exogenous injection of GHGs. Such a "planetary experiment" of an exogenous injection of *this much GHGs this fast* seems unprecedented in Earth's history stretching back perhaps even 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 climate equilibrium[14], chaotic dynamics (including thresholds, bifurcations, and other discontinuities) cannot by any means be ruled out, and all twenty-two current studies of climate sensitivity (peer-reviewed and published in reputable scientific journals) cited by IPCC-AR4 (2007)

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

mechanically aggregated together are estimating on average that $P[S_1 > 7°C] \approx 5\%$. 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 global cultivation of Frankenfood or the possibility of colliding with a large asteroid.

I think these two examples perhaps suggest that it may (barely) be possible to make a few meaningful distinctions among the handful of situations where the Dismal Theorem might conceivably apply seriously. My discussion here is hardly conclusive, so that we surely cannot rule out a biotech or asteroid disaster (and therefore we should undertake appropriate precautions in both cases). However, I would say on the basis of this line of argument that such disasters seem (to me) *very very* unlikely, whereas a climate disaster seems (to me) "only" *very* unlikely. In the language of this paper, synthetic biology or large asteroids feel (to me) more like high-($k+n$) situations that we know a lot more about *relative to* climate change, which by comparison feels (to me) more like a low-($k+n$) situation about which we know relatively little. It is true that a climate-change catastrophe develops more slowly than some other potential catastrophes and there is perhaps somewhat more chance for learning and mid-course corrections relative to, say, biotechnology (but not necessarily relative to asteroids when a good tracking system is in place). So the possibility of "learning by doing" may be a more distinctive feature of climate change disasters than some other disasters and in that sense deserves to be part of an optimal climate-change policy. This being said, the climate response to GHGs has tremendous built-in inertial lags and 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 that $P[S_2 > 20°C] \approx 1\%$.

I have tried to give two examples contrasting climate change with synthetic biology and large asteroids. I have further tried to argue that climate change looks far more dangerous than either of these other two examples of catastrophic possibilities. However, these examples are intended more to illustrate a possible thought process than to render a final judgement. The applicability of DT to each catastrophe scenario must be decided on a case by case basis. To repeat, the overarching principle here is that the mere fact that DT might also apply to a few other environmental catastrophes, like maybe biotechnology or asteroids, does *not* constitute a valid reason for excluding it from applying to climate change.

Global climate change unfolds over a time scale 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 (8) that forms the analytical backbone for most studies of the economics of climate change.[15] For the model of this paper, which is based on structural uncertainty, arguments about what values of δ to use in equation (7) or (8) translate into arguments about what values of β to use in the model's structural-uncertainty generalization of the Ramsey equation, which is the expectation formula (33). (A zero rate of pure time preference $\delta = 0$ in (8) corresponds to $\beta = 1$ in (33).) 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 deep structural fat-tailed uncertainty.

Expected utility theory in the form of the Dismal Theorem is telling us here analytically 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 residing with whoever wants to over-rule the theorem in a particular application. The take-away message here is that the burden of proof in the economics of climate change is presumptively upon whoever wants to model or conceptualize the expected present discounted utility of feasible trajectories under greenhouse warming *without* considering that structural uncertainty might matter more than discounting or pure risk *per se*. Such a middle-of-the-distribution modeler should be prepared to explain why the fat bad tail of the posterior-predictive distribution is *not* empirically relevant and does *not* play a significant role in the analysis.

8 Possible Implications for Climate-Change Policy

Many situations have natural limited-exposure-like bounds on the possible damages that might materialize, for which the theory of this paper may be less relevant or perhaps even have no relevance. When 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 tail fatness is not such a paramount concern. But a few

[15] While this contentious intergenerational-discounting issue has long existed (see, e.g., the various essays in Portney and 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, e.g., 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.

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. This paper shows that the EU analysis of those relatively few deep-uncertainty situations with potentially catastrophic reach – like climate change – can give very different conclusions than what might emerge from a typical CBA of a more ordinary limited-exposure situation.

A so-called "Integrated Assessment Model" (hereafter "IAM") for climate change is a multiple-equation computer-assisted model that combines dynamic economics with geophysical and geochemical circulation dynamics for purposes of analyzing the economic impacts of global climate change. An IAM is essentially a model of economic growth 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 long lags and big inertias that are involved. Most existing economic analyses of climate change treat central forecasts of damages as if they were certain and then do some casual sensitivity analysis on parameter values. In the rare cases where an IAM formally incorporates uncertainty at all into its structure, it uses thin-tailed PDFs including, especially, truncation of PDFs at arbitrary cutoffs.

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." The high-temperature "damages function" extrapolated from the low-temperature "damages function" is remarkably sensitive to assumed functional forms and parameter combinations because an extraordinarily wide variety of functional forms and parameter combinations can be made to fit virtually identically the low-temperature damages that have been assumed by the modeler. In the literature, most "damages functions" reduce welfare-equivalent consumption by the quadratic-polynomial multiplier $1/[1 + \gamma(\Delta T)^2]$, for which there was never any more compelling rationale in the first place than the comfortable feeling from long acquaintance that economists have for this particular type of loss function. In other words, the quadratic-polynomial specification is being used to assess climate-change damages for no better reason than casual familiarity from other cost-of-adjustment dynamic economic models, where this particular form was used primarily for analytical simplicity.

I would argue strongly on *a priori* grounds 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 "harm" to welfare is arriving here as the arbitrarily imposed quadratic form $H(\Delta T) = (\Delta T)^2$, around which some further structure is imposed 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. 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. To repeat a theme that by now is woven into the fabric of this paper: I cannot prove that my favored choice here 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.

The value of γ required for calibrating welfare-equivalent consumption at $\Delta T = 3°\text{C}$ to be, say, 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 $\Delta T \leq 6°\text{C}$, 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 profound 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 multiplier has the exponential form $\exp(-\gamma(\Delta T)^2)$, as the VSL-like parameter $\lambda \to \infty$ the Dismal Theorem implies in the limit that the WTP to avoid (or even reduce) this 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 example does highlight the remarkable ability of miniscule refinements of the "damages function" (when combined with fat tails) to dominate climate-change CBA – and the remarkable fragility of policy advice coming out of conventional thin-tailed IAMs with polynomial damages.

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.[16] The core underlying 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*. The dismal outcome embodied in Theorem 1 of this paper 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 most clearly in a crisp thought experiment, imagine what would happen to the simple stripped-down model of this

[16] 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.

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 values of consumption-equivalent outcomes from all conceivable negative impacts. Such arbitrarily-imposed *de 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 zero – 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 the validity of 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 ultra-catastrophic impacts (down to at least one percent or so of current welfare-equivalent consumption, which is equivalent to damages of 99% or more) 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.[17]

An additional consideration is that 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 fattened tails where the most extreme damages are. Nor will typical sensitivity analysis (of either deterministic models or formal Monte Carlo simulations) necessarily penetrate sufficiently far into the fattened-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

[17] 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.

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 the economic impacts of what are effectively 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 (relative to probability of occurrence) 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 more-realistic high-temperature damages in the limit as the grid size and number of runs simultaneously increase. Of course this emphasis on sampling climate-change scenarios in proportion to marginal-utility-weighted probabilities of occurrence forces us to estimate subjective probabilities down to extraordinarily tiny levels and also to put utility weights on disasters with damage impacts up to perhaps being welfare-equivalent to losing 99% (or possibly even much 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 – a reframing of the focus of economic analysis might be appropriate, with more emphasis on even 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 merely-likely outcomes or arguing over such things as appropriate discount rates and most-likely values of climate sensitivity. A clear implication of this paper is that greater research effort is relatively ineffectual when it is targeted at describing and estimating the 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 in my opinion is 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 goal, 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, which should include undertaking well-funded detailed studies and experiments about the feasibility, possible environmental side effects, and cost-effectiveness of climate-engineering options to slim down the bad fat tail quickly in emergency-runaway cases where things might be beginning to get out of hand.[18]

[18] It is a secret thus far well kept within the climate-change community of scholars that (with the unfortunately-limited information we currently possess) geoengineering via injection of stratospheric sul-

When analyzing the economics of climate change, perhaps it might be possible to reason somewhat by analogy with insurance for extreme events and to compare, say, a homeowner's cost of buying fire insurance (and/or buying fire-protection devices), or a young adult's cost of purchasing life insurance, with the cost to the world economy of buying an insurance policy going some way towards 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 anti-ballistic missile shields, or neutralizing hostile dictatorships that might harbor 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.[19] 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 towards resolving the deep uncertainties – relative to the lags and irreversibilities from *not* acting before structure is more fully identified.[20]

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. This paying attention to the feasibility of slimming down overweight tails is

fate aerosol precursors looks at first glance now like an incredibly cheap and effective way to slim down the bad fat tail quickly in case of emergency – although with largely unknown and conceivably nasty unintended consequences that we need to understand much better. For more on the economics of geoengineering, see 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 as part of an overall cost-benefit-effectiveness assessment of climate-change strategies that faces honestly the question: what in our *realpolitik* world are the actual policy alternatives and trade-offs that we realistically face on actually-available (as opposed to "ideal") climate change options?

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

[20] For more details about slow convergence in n as viewed from a climate-science perspective, see Roe and Baker (2007) or Allen and Frame (2007).

likely to be a perennial theme in the economic analysis of catastrophes. The key economic issues 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?

9 Conclusion

Last section's heroic (if speculative) attempts at doling out 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 – and there is no sense pretending otherwise. The economics of fat-tailed catastrophes raises difficult conceptual issues which cause the analysis to appear less scientifically conclusive and to look 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 a fact 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 not even presenting the analysis as if it is an *approximation* to something that is accurate and objective – but instead by stressing more 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 uncertainties that are involved in climate-change analysis (and explaining better to policy makers that the artificial crispness conveyed by conventional IAM-based CBAs is especially and unusually misleading in this particular context) would in my opinion go a long way towards elevating the level of a reality-based 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 the Dismal Theorem 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 prove 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, may dominate the social-discounting aspect, the pure-risk aspect, or 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.

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