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The Climate Policy Dilemma

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Introduction

Most environmental problems—for example, SOx and NOx emissions from coal-burning power plants—are amenable to standard benefit-cost analysis. There will always be uncertainties over the benefits and costs of any proposed abatement policy, but the characteristics and extent of those uncertainties will usually be well understood and comparable in nature to the uncertainties concerning many other public and private policy or investment decisions. Of course, economists can (and will) argue about the details of the analysis. But at a basic level, we're in well-charted territory and confident that we know what we're doing. If we come to the conclusion that a policy to reduce SOx emissions by a particular amount is warranted, that conclusion will be viewed, at least by most economists, as defensible and reasonable.

Not so with climate change. Climate policy poses a serious dilemma for environmental economists. In part because of declining economic growth and rising unemployment in much of the world, there has been waning political enthusiasm for implementing stringent greenhouse gas (GHG) abatement policies, and climate change is taking a back seat to other environmental—and nonenvironmental—policy problems. More importantly, the economic argument for stringent GHG abatement is far from clear. There is disagreement among both climate scientists and economists over the likelihood of alternative climate outcomes, as well as the nature and extent of the uncertainty of those outcomes. There is also disagreement about the framework that should be used to evaluate the potential benefits from an abatement policy, including the social welfare function and the discount rate to be used to put future welfare benefits from abatement in present value terms. These disagreements make climate policy both difficult to evaluate and hard to sell to the general public.

Given these disagreements and the limits to our current state of knowledge, should climate policy be a priority for environmental economists and policymakers? Many economists support policies that would result in gradual GHG abatement, and that could cost as much as 1 or 2 percent of gross domestic product (GDP). But can a case be made for the *early* adoption of a *stringent* GHG abatement policy that would sharply reduce emissions, thereby limiting the accumulation of GHGs in the atmosphere, and have an annual cost of more than 2 or 3 percent of GDP?[1] Put simply, is there a good economic argument for a stringent climate policy that is

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[1]To put these cost numbers in context, it has been estimated that the total direct cost of *all* environmental regulation in the United States has been about 2 percent of GDP on average. See Jaffe et al. (1995).

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likely to be costly to implement and that would yield highly uncertain benefits only fifty or a hundred years from now? This is what I call the climate policy dilemma.

Why is it so difficult to apply standard benefit-cost analysis to GHG abatement policy? The analysis is more complicated than for most other policy problems because of the very long time horizon, the very large uncertainties, and the difficulty of even characterizing those uncertainties. Even if there were no uncertainty, the time horizon alone is problematic because it makes the present value of future benefits extremely sensitive to the choice of discount rate—and (as noted earlier) there is considerable disagreement over what the "correct" discount rate should be. As for the uncertainties, they pertain to the extent of warming (and other aspects of climate change) under current and expected future GHG emissions, as well as the economic impact of any climate change that might occur. The economic impact of climate change is especially uncertain, in part because of the possibility of adaptation. We simply don't know much about how worse off the world would be if by the end of the century the global mean temperature increased by 3° or 5°C. In fact, we may never be able to resolve these uncertainties (at least not over the next fifty years). It may be that the impact of higher temperatures is not only unknown, but also unknowable.

Over the last twenty years we have seen a proliferation of quantitative studies of climate policy, including a variety of integrated assessment models (IAMs).[2] What conclusions can we draw from this large (and still growing) body of research? At the risk (or intent) of being provocative, I will argue in this article that the case for a *stringent* GHG abatement policy cannot be based on "most likely" scenarios (i.e., climate and impact outcomes that are within our 90 percent or even 95 percent confidence range), but rather on the possibility of a catastrophic outcome. Indeed, average estimates of the social cost of carbon (SCC) based on three widely cited IAMs range from \$5 to \$35 per ton in 2010, rising to \$16 to \$65 per ton in 2050—values consistent with, at most, moderate abatement.[3] Thus making the case for a stringent policy would require assumptions about costs, benefits, and economic parameters that are well outside the consensus range.[4]

Barring extreme assumptions about costs, benefits, and economic parameters, can an argument for stringent abatement be based on the tail of the climate outcome distribution, that is, on the possibility of a *catastrophic* climate outcome? The kind of outcome I am referring to is not simply a very large increase in temperature (or changes in other climate indicia), but rather a very large *economic impact*, in terms of a decline in human welfare, from whatever climate change occurs. If the likelihood and impact of such an outcome were sufficiently large, a net present value (NPV) calculation *might* support a stringent policy. Unfortunately, addressing

[2]Such models "integrate" a description of GHG emissions and their impact on temperature and other aspects of climate (a climate science model) with projections of current and future abatement costs and a description of how changes in climate affect output, consumption, and other economic variables (an economic model).
[3]The SCC is the cost to society from the environmental damage caused by an additional ton of carbon emissions. The three IAMs used to estimate the SCC are DICE (Dynamic Integrated Climate and Economy), PAGE (Policy Analysis of the Greenhouse Effect), and FUND (Climate Framework for Uncertainty, Distribution, and Negotiation), which are described in Nordhaus (2008) and Nordhaus and Boyer (2000), Hope (2006), and Tol (2002), respectively. For a survey of SCC estimates generated from these models, see Greenstone, Kopits, and Wolverton (2011) and the Interagency Working Group on Social Cost of Carbon (2010).
[4]The Stern Review (2007), which argues for the immediate adoption of very stringent GHG abatement, is an exception, but as Nordhaus (2007), Weitzman (2007), Mendelsohn (2008), and others point out, the Stern analysis makes extreme assumptions about costs, benefits, and economic parameters.

The Discount Rate

To understand the climate policy dilemma, a good place to start is the disagreement and debate over the correct rate at which to discount the future benefits from GHG abatement. To keep things simple, let's assume that everyone agrees (even though they don't) that the damage from global warming and climate change generally occurs via a reduction in consumption, C, and that a reduction in C directly reduces social welfare via the widely used constant relative risk aversion (CRRA) utility function $U(C)$:

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

Here η is the index of relative risk aversion (IRRA), which is also a measure of social aversion to consumption inequality across points in time. (The value of an extra unit of consumption, i.e., marginal utility, is $U'(C) = C^{-\eta}$, which declines as C grows.)

Now the question is how should we value the utility from some level of consumption fifty years from now relative to the *same amount of utility* enjoyed today? In other words, how should we discount future utility (*not* future consumption itself) so as to determine its present value? The discount rate used to do this, which I will denote by δ, is called the pure rate of time preference, but since we are looking at welfare for society as a whole, we will call it the social rate of time preference. But what is the "correct" value for this discount rate?

We know from a broad range of studies that most individuals would prefer to receive a unit of consumption now rather than receive that same unit a month, a year, or ten years from now. We also know that financial data reflecting investor behavior, as well as movements of macroeconomic aggregates reflecting consumer and firm behavior, suggest that δ is in the range of 2 to 5 percent.[5] While a rate in this range may reflect the preferences of investors and consumers, should it also reflect *intergenerational* preferences and thus apply to time horizons of fifty or a hundred years? In other words, should the welfare of our great-grandchildren be discounted relative to our own welfare, and if so, at what rate? The answer to this question is crucial for climate policy because even a rate as low as 2 percent would make the present value of future welfare gains from GHG abatement too small relative to the costs of abatement to justify almost any policy, even one that is quite moderate. (With a discount rate of 2 percent, a \$1 benefit a hundred years from now is worth less than 14 cents today.)

Unfortunately, economics has little to say about how we should make such intergenerational comparisons. Some economists (e.g., Stern [2007] and Heal [2009]) have argued that *on ethical grounds* the rate of time preference should be *zero* for intergenerational comparisons (i.e., that it is unethical to discount the welfare of future generations relative to our own welfare). But why is it unethical? Suppose John and Jane both have the same incomes. John saves 10 percent of his income every year in order to help finance the college educations of his (perhaps yet-to-be-born) grandchildren while Jane prefers to spend all of her disposable income on sports cars, boats, and expensive wines. Does John's concern for his grandchildren make him more ethical than Jane? Putting aside their personal views, I don't think economists have much to say about this question.

[5]For an excellent survey of research on the rate of time preference, see Frederick, Loewenstein, and O'Donoghue (2002).

It seems to me that the rate of time preference is a *policy parameter*, that is, it simply reflects the values of policymakers, who might or might not believe (or care) that their policy decisions reflect the values of voters or the general public. As a policy parameter, the rate of time preference might be positive, zero, or even negative. (Why negative? One could argue, perhaps based on simple altruism or a belief that human character is improving over time, that the welfare of our great-grandchildren should be valued *more* highly than our own.) The problem is that once we agree that the rate of time preference is a somewhat arbitrary parameter, it becomes hard to make a clear case for (or against) a stringent climate policy. Put another way, as in other areas of economic policy, the case for a stringent climate policy should be reasonably robust, not relying heavily on the value of any one parameter (in this case the rate of time preference).

The Index of Relative Risk Aversion

The IRRA, denoted by η, can also strongly affect the economic case for a climate policy. To see this, note that η affects expected future welfare in two ways. First, the larger the value of η, the faster the marginal utility of consumption will decline as consumption grows. Since (other things equal) consumption is expected to grow over time, the value of additional consumption in the future is smaller the larger is η. Second, η measures risk aversion; this means that if future consumption is uncertain, future welfare will be smaller the larger is η. Thus a higher value of η has two opposing effects on the expected benefits from an abatement policy that reduces climate-induced losses of future consumption: (a) A higher η means the marginal utility of consumption in the future will be smaller, implying a smaller benefit from avoiding a climate-induced loss of consumption; and (b) If there is uncertainty over the impact of GHG accumulation, a higher η means a larger loss of expected future welfare, implying a larger benefit from abatement. Most models show that unless risk aversion is extreme (e.g., η is above 4), the first effect dominates, which means an increase in η (say, from 1 to 4) will reduce the benefits from an abatement policy.

So, what is the "correct" value to use for η when evaluating a climate policy? Economists disagree. The answer depends in part on whether we view η as a *behavioral* parameter (i.e., reflecting the behavior of consumers, investors, and firms) or a *policy* parameter (i.e., reflecting the opinions and objectives of policymakers). If η is viewed as a behavioral parameter, the consensus range, based on the macroeconomics and finance literatures, extends from about 1.5 to at least 4. If we view η as a policy parameter, we can consider the fact that η also reflects aversion to consumption inequality (in this case across generations). This means that if a future generation is expected to have twice the income and consumption as the current generation, then the marginal utility of consumption is $1/2^{\eta}$ as large for the future generation as for the current generation, and it would be weighted accordingly in any welfare calculation. Since values of η above 3 or 4 imply a relatively very small weight for the future generation, a policymaker might view smaller values of η as more appropriate. In that case, the reasonable range might be from about 1 to 3.[6]

Whether η is viewed as a behavioral or policy parameter, we are left with a wide range of reasonable values, and thus a wide range of estimates of the benefits of climate change

[6]For discussions of this point, see Dasgupta (2008) and Nordhaus (2011).

mitigation. Clearly, it is much harder to justify a costly climate policy if one believes that η is closer to 3 than to 1, but at the same time it is hard to make a clear case that η should, in fact, be close to 1. As with the rate of time preference, our inability to pin down this crucial parameter makes it very difficult to argue clearly for (or against) a stringent climate policy.

Estimates of Willingness to Pay

It simplifies matters somewhat to focus on the "demand" side of climate policy, and, using a WTP framework, to ask how large a sacrifice society would be willing to make to achieve a policy objective. For example, we could ask what is the largest fraction of consumption that society would be willing to give up, now and throughout the future, to prevent the global mean temperature from increasing by more than, say, 3°C by the end of the century. Let's denote the increase in temperature by T, and suppose the WTP to ensure that $T \leq 3°C$ is 2 percent of consumption. It may or may not be feasible to limit the temperature increase to 3°C at a cost of 2 percent of consumption; the cost might be less than 2 percent of consumption (in which case the policy would yield a positive social surplus) or it might be more than 2 percent (in which case the policy could not be justified economically). The 2 percent is simply society's reservation price (i.e., the most society would pay) to achieve the policy objective.

The advantage of working with WTP is that we can ignore the cost side of policy and focus on the uncertainties involved in projecting temperature increases and their impact on GDP and consumption, as well as the implications of alternative values of the rate of time preference (δ) and the IRRA (η). Now let's suppose the policy objective is to reduce GHG emissions sufficiently so that any increase in the global mean temperature by the year 2100 is *at most* 3°C. Climate science studies surveyed by the IPCC (2007) suggest that the *expected* temperature increase in 2100 under BAU is 3°C, so this seems like a reasonable objective.[7] To make the case for a stringent abatement policy, we would need to show that the WTP for this objective is substantial (i.e., at least 2 or 3 percent of GDP) and robust to a range of reasonable values for key parameters and to a range of reasonable probability distributions for the increase in temperature and its impact.

I used this approach in a recent study of climate change policy that examined the implications of uncertainty about both temperature change and its impact on the growth rate of GDP (Pindyck 2012).[8] Using information on distributions for temperature change and economic impact from studies assembled by the Intergovernmental Panel on Climate Change (IPCC) and others, I fit probability distributions for these variables, which I argued roughly reflect the "state of knowledge" regarding the nature and extent of uncertainty about global warming and its impact. Using these distributions, I calculated the WTP to limit the increase in temperature by the end of the century to 3°C. I found that for just about any values of η between 1 and 4, WTP

[7]According to the IPCC (2007), this 3°C is actually the expected value of *climate sensitivity*. Climate sensitivity is the temperature increase that would result from a doubling of the atmospheric concentration of CO_2-equivalent (CO_2e). But given the IPCC's estimated trajectory for atmospheric CO_2e and the time it takes for higher temperatures to result, 3°C is, at least roughly, the expected value of the temperature increase.

[8]Most IAMs and other economic studies of climate change posit a direct relationship between higher temperatures and the *level* of GDP, whereas (on theoretical and empirical grounds) I related higher temperatures to the *growth rate* of GDP. In Pindyck (2011b), I examined the extent to which my results might have been affected by relating temperature increases to the growth rate as opposed to the level of GDP, and found that it made little difference in the resulting estimates of WTP. For other studies using WTP, see, for example, Heal and Kriström (2002) and Newbold and Daigneault (2010).

was less than 2 percent of GDP if the rate of time preference, δ, was 1 percent or greater. When δ was set to zero (the so-called ethical value), a WTP greater than 2 percent of GDP could be obtained only if η was less than 1.5. For values of η above 2, the WTP was less than 1 percent. These results are inconsistent with the immediate adoption of a stringent abatement policy.

Of course one might take issue with the particular distributions I used (or the studies surveyed by the IPCC to which those distributions are calibrated) and with the range of parameters I considered. Stern (2007), for example, set $\eta = 1$, the lowest end of the credible range. Research by climate scientists and other economists may yield distributions for temperature change and its impact that are more pessimistic than those I (and others) have used, which might lead us to conclude that the "correct" distributions should have more weight in the tails (i.e., greater probabilities of extreme outcomes). I turn to this possibility next.

The Nature of Climate Change Uncertainty

As discussed earlier, there are two key areas of uncertainty and thus two distributions that we need to worry about: the extent of warming and the economic impact from whatever warming occurs.[9] However, much of the discussion of uncertainty that has appeared in the literature has focused solely on the extent of warming and related climate changes. I believe the main reason for this discrepancy is that we know much more about the climate science than we do about economic impacts. This means that we can look at means or confidence intervals that climate scientists have arrived at for, say, temperature, and then consider the probability distributions that are consistent with those statistics. The treatment of economic impacts in most IAMs has been largely ad hoc. Typically a loss function $L(T)$ is specified such that $L(0) = 1$ and $L(T)$ declines as T increases, and the parameters of that loss function are chosen to yield moderate losses (e.g., 3 percent of GDP, so $L = 0.97$) from moderate values of T (e.g., 3° or 4°C).[10] But there is little evidence on which to base the choice of parameters for the loss function, apart from yielding numbers (e.g., $L(3) = 0.97$) that seem "reasonable." Furthermore, once we consider temperatures of 6°C and higher, determining the economic loss, or a distribution for that loss, become a matter of guesswork. One can plug high temperatures into IAM loss functions—see Figure 1 in Greenstone, Kopits, and Wolverton (2011) for an illustration—but the results are just extrapolations with no empirical or theoretical grounding.

Given our inability to even characterize the uncertainty over economic impacts, I focus in this section on uncertainty with regard to T. That is, if we do little or nothing to abate GHG emissions, how much might the temperature increase by 2100? To address this question, we would like to know what probability distribution best represents the likelihood of various outcomes for T. In summarizing twenty-two studies of climate sensitivity, the IPCC (2007) translated the implied distributions from those studies into a standardized form and created graphs showing multiple distributions implied by groups of studies. I found that a gamma distribution for T provided a good fit to these distributions, and in Pindyck (2011b, 2012), I calibrated the parameters of the gamma distribution to match the mean and critical 66 percent

[9]I am assuming that all impacts of climate change, including health and social impacts, can be monetized and expressed in terms of lost GDP.

[10]The Nordhaus (2008) DICE model uses the inverse-quadratic loss function $L(T) = 1/(1+\pi_1 T+\pi_2 T^2)$. Weitzman (2008) introduced the exponential-quadratic loss function, $L(T) = \exp(-\beta T^2)$, which allows for greater losses at high temperatures.

and 95 percent points from the IPCC summary. Using the calibrated distribution, I then calculated the WTP to keep T in the year 2100 at or below various levels, and the values I obtained for WTP were generally small.[11]

A natural critique of this earlier work is that the distribution I used for T is overly optimistic. Some recent work suggests that the expected value for T is greater than 3°C, and/or that the 95 percent point should be at a higher temperature than the 7°C that I used. Recalibrating the distribution to a mean of 5°C indeed increases the WTP: I obtained a WTP above 3 percent if $\delta = 0$ and η is below 1.5. But this is hardly robust evidence for a high WTP because if $\delta = 0.01$ and/or η is above 1.5, the WTP again falls below 2 percent.

Another possible critique of my work is that the gamma distribution is thin-tailed, and that I should have used a fat-tailed distribution.[12] This is the argument raised by Weitzman (2009, 2011) as a potential justification for stringent abatement. In Pindyck (2011a), I showed that a fat-tailed distribution by itself need not lead to larger estimates of WTP if marginal utility is bounded.[13] Nonetheless, one could argue that alternative distributions for T, whether fat- or thin-tailed, that are also consistent with the IPCC (2007) mean and critical points might result in higher values of WTP. I explore the possibility of such alternative distributions next.

Alternative Distributions for Temperature

Figure 1 shows three distributions for increases in temperature (T) up to 10°C by 2100. The first is the (gamma) distribution I used in Pindyck (2011b, 2012), with parameters calibrated to match the mean and critical 66 percent and 95 percent points from the IPCC summary of climate sensitivity studies.[14] This distribution implies a 2.9 percent probability of a temperature *decline* as large as 1.1°C, which is consistent with the studies surveyed by the IPCC. In addition to an expected temperature change of 3°C, it implies a standard deviation of 2.1°C for the temperature change. Note that this distribution (as well as the two other distributions) applies to the temperature increase as of 2100.

The two other distributions for T shown in Figure 1 are a Frechet (also called a generalized extreme value, type 2) distribution[15] and a nonstandard probability distribution called

[11]Given a value for T_{2100}, the temperature increase at each point in time, T_t, increases from zero to this value and then continues increasing, asymptotically approaching $2T_{2100}$. To be precise, the temperature change at time t is $T_t = 2T_{2100}[1 - (1/2)^{.01t}]$. See Pindyck (2012) for details.

[12]A thin-tailed (fat-tailed) distribution for T declines to zero faster (more slowly) than exponentially as T increases.

[13]A constant relative risk aversion utility function implies that marginal utility becomes infinite as consumption goes to zero. As a result, a fat-tailed distribution for T can yield a WTP of 100 percent because the high probability of a sufficiently high T and the resulting high probability of a sufficiently low C can imply an expected marginal utility of consumption that is infinite. This result disappears if we bound marginal utility at some maximum value.

[14]The distribution is

$$f(T; r, \lambda, \theta) = \frac{\lambda^r}{\Gamma(r)}(T - \theta)^{r-1} e^{-\lambda(T-\theta)}, \quad T \geq \theta$$

where $\Gamma(r)$ is the gamma function, $r = 3.8$, $\lambda = 0.92$, and $\theta = -1.13$.

[15]The Frechet distribution is given by

$$f(T; k, \mu, \sigma) = (1/\sigma)\exp[-(1+kz)^{-1/k}](1+kz)^{-1-1/k}$$

where $z = (T - \mu)/\sigma$, $k > 0$, and $T \geq \mu - \sigma/k$.

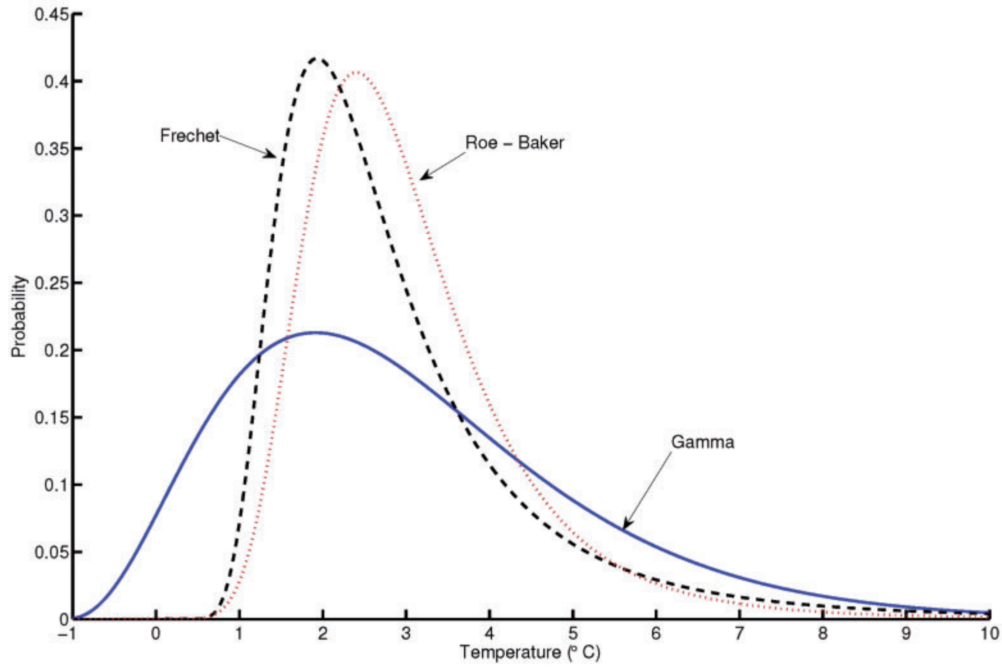

Figure 1 Three Temperature Distributions
Notes: All three distributions were calibrated to have the same mean (3°), standard deviation (2.1°), and minimum possible temperature change (−1.1°).

Roe-Baker,[16] which was developed by Roe and Baker (2007) from a simple climate model with uncertain feedback effects. I chose parameter values for these two distributions to match the mean, standard deviation, and minimum point of the gamma distribution so that all three distributions are roughly consistent with the studies surveyed by the IPCC.[17] Although it appears from the graphs that the Frechet and Roe-Baker distributions begin at values of T above 0, in fact the distributions are positive, but extremely small, for values of T as low as $-1°$. Note that the Frechet distribution is "somewhat" fat-tailed (i.e., for the parameter values I have chosen, the distribution has a mean and variance but no higher moments) and the Roe-Baker distribution is extremely fat-tailed (i.e., none of the moments exist).[18]

All three of these distributions seem like credible descriptions of the likelihood of future temperature changes, especially since we know very little about the likelihood of temperatures above 6 or 8°C. Nevertheless, depending on one's prior assumptions, one distribution may

[16]The Roe-Baker distribution is given by

$$g(T;\bar{f},\sigma_f,\theta) = \frac{1}{\sigma_f\sqrt{2\pi z^2}}\exp\left[-\frac{1}{2}\left(\frac{1-\bar{f}-1/z}{\sigma_f}\right)^2\right]$$

where $z = T+\theta$. The feedback parameter in the model is normally distributed with mean and standard deviation $\bar{f}$ and σ_f, respectively.

[17]The parameter values for the Frechet distribution are $k = 0.28$, $\mu = 2.15$, and $\sigma = 0.915$. The parameter values for the Roe-Baker distribution are $\bar{f} = 0.797$, $\sigma_f = .0441$, and $\theta = 2.13$.

[18]I calibrated the Roe-Baker distribution to have a *calculated* mean of 3° and standard deviation of 2.1° for range of T up to 50°.

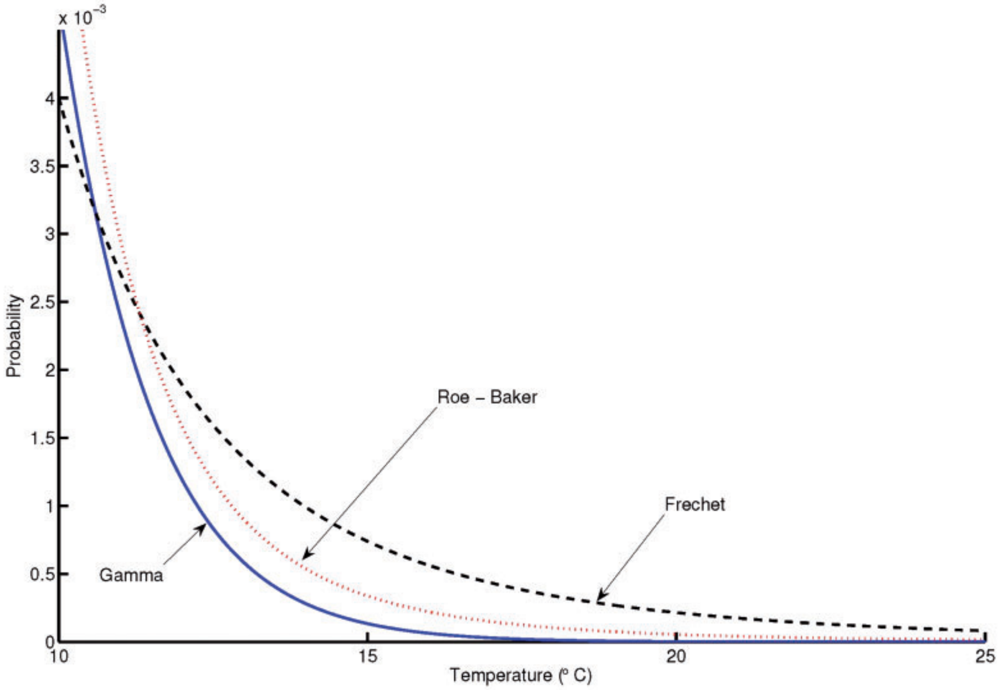

Figure 2 Upper Tails of Temperature Distributions

seem more credible than the others. For example, the Frechet and Roe-Baker distributions have much more mass than the gamma distribution between 1° and 4°, and the Roe-Baker distribution is grounded in a theoretical model relating GHG concentrations to temperature change. However, as shown in Figure 2, the tails of the three distributions look quite different, with the Frechet and Roe-Baker distributions having much more mass than the gamma distribution at temperatures above 10°. But again, given how little we know about the likelihood of such high temperatures, it is hard to distinguish among these distributions and claim that one is more "correct" than the others.

Implications for Policy

Might these three temperature distributions have very different policy implications? I address this question using the framework in Pindyck (2012), but taking the parameter that relates T to the rate of GDP growth as fixed and equal to its mean value.[19] This implies a 3 percent loss of GDP in 2100 if T reaches 4°C, which is consistent with IPCC (2007). Next, for each of the three temperature distributions, and for a range of values for the index of risk aversion, η, I calculate the WTP to ensure that T in 2100 will not exceed its mean value of 3°C.[20] The results are shown in Figure 3.

[19]In Pindyck (2012), the real per capita GDP growth rate is $g_t = g_0 - \gamma T_t$, where γ is uncertain and follows a gamma distribution. In this exercise I focus on uncertainty over T and set γ equal to its mean value.
[20]Of course, I could have instead calculated the WTP to ensure T will remain zero or not exceed some other value. I chose 3°C for this exercise because a variety of studies focus on limiting temperature increases to 2°C or 3°C, and 3°C is the expected value for T in 2100.

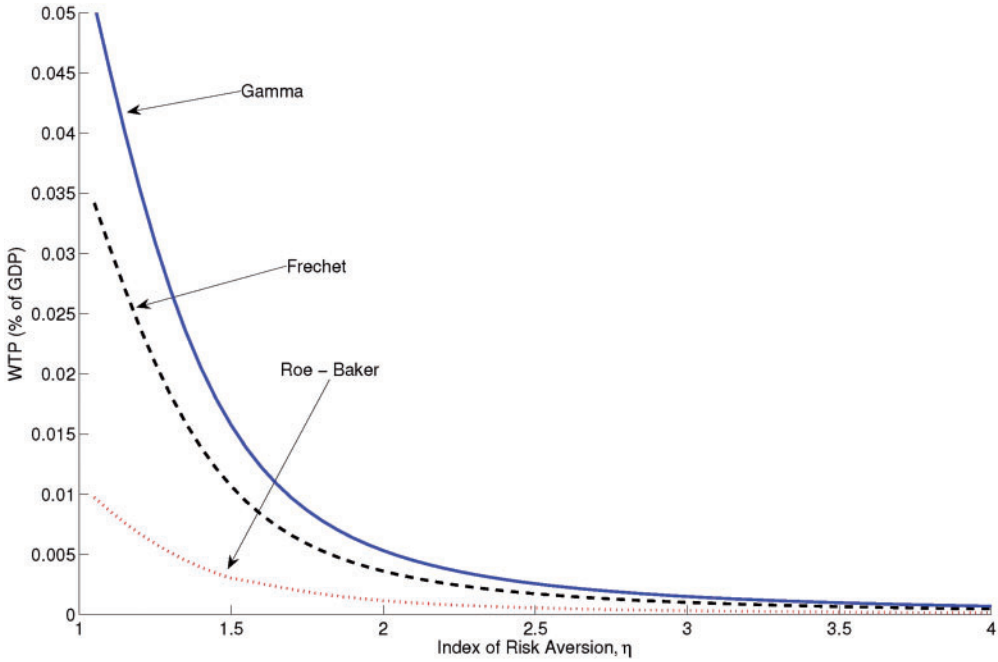

Figure 3 WTP to Keep *T* in 2100 Below 3°C

Notes: The three distributions (gamma, Frechet, and Roe-Baker) are calibrated to have the same mean (3°), standard deviation (2.1°), and minimum possible temperature change (−1.1°). Initial GDP growth rate, g_0, is .02, the rate of time preference, δ, is zero, and the growth impact parameter, γ, equals its mean value of .000136.

Observe that the highest values of WTP are obtained when T is assumed to follow the (thin-tailed) gamma distribution. The Roe-Baker distribution, which is extremely fat-tailed, leads to a very low WTP for the entire range of η. The reason is evident from Figures 1 and 2, which show that compared to the gamma distribution, the Roe-Baker and Frechet distributions are "bunched up" in the range of 1° to 4°, with less mass in the higher temperature range of 5° to 8°. The standard deviations are the same for all three distributions because they have more mass at very high temperatures. Nevertheless, the *probabilities* for those very high temperatures are still very low. Thus fat tails need not imply a high WTP, a point discussed in detail in Pindyck (2011a).

Perhaps the limited set of economic studies reviewed by the IPCC (2007) were overly optimistic regarding the impact of higher temperatures. To explore this possibility, I recalculated WTP, but this time I doubled the parameter that relates T to the rate of GDP growth (so that $T = 4$°C would result in a 6 percent rather than a 3 percent loss of GDP in 2100). The results are shown in Figure 4. Not surprisingly, the WTP values are roughly double those in Figure 3. In fact, for the gamma distribution, WTP is close to 10 percent of GDP if the index of risk aversion, η, is close to 1.

What do the results in Figure 4 tell us? First, if we assume a large enough economic impact, we can obtain a WTP that is large enough to be consistent with stringent abatement. But note that the parameter values needed to get this result are a bit extreme: a value of η below 1.5 and a rate of time preference, δ, equal to zero. (Although not shown in the figures, WTP falls

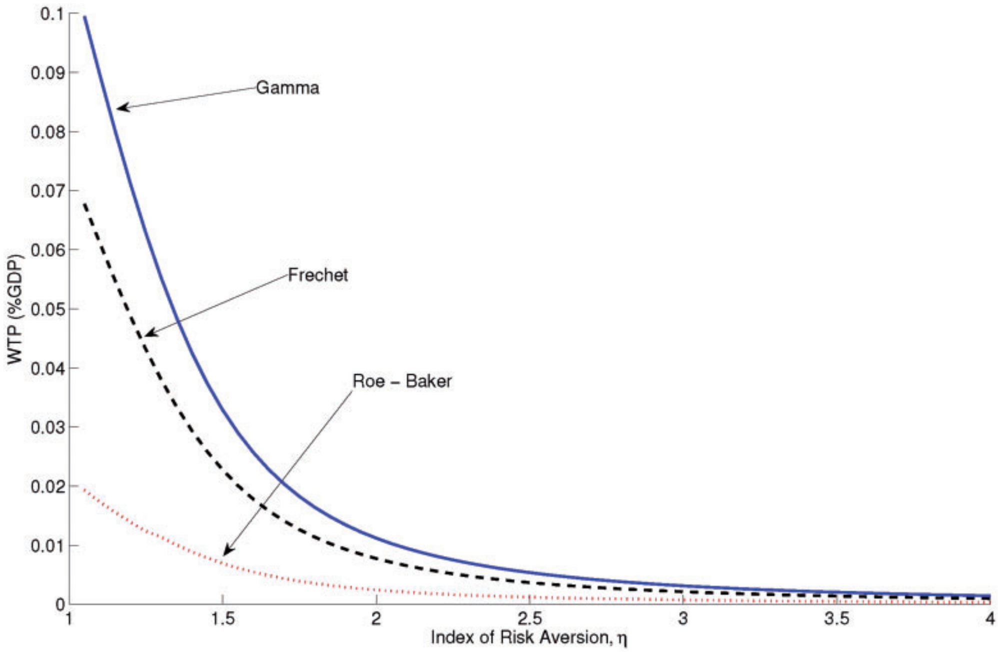

Figure 4 WTP to Keep *T* in 2100 Below 3°C, with Growth Impact Doubled
Notes: The three distributions have the same mean (3°), standard deviation (2.1°), and minimum possible temperature change (−1.1°). Initial GDP growth rate, g_0, is .02, and rate of time preference, δ, is zero. The growth impact parameter, γ, equals twice its mean value, i.e., $\gamma = .000272$.

dramatically if $\delta = 0.01$ or 0.02.) Unless one can make a strong argument for $\delta = 0$ and $\eta < 1.5$, these results do not provide a strong case for stringent abatement.

Perhaps we should consider the possibility of even larger economic impacts, especially for temperature increases above 5°C, which, although unlikely, are certainly possible. The problem with this approach is that we know so little about the possible economic impacts of this degree of global warming. In contrast to temperature, it is difficult to even come up with probability distributions for economic impacts.

Impact of Climate Change: The Unknown and the Unknowable

Why is it so difficult to estimate how climate change will affect the economy? One problem is that we have very little data on which to base empirical work. True, there have been a few empirical studies that made use of temperature and rainfall data for a large panel of countries over fifty or more years (e.g., Dell, Jones, and Olken [2009, 2012], and Bansal and Ochoa [2011a, 2011b]). These studies have helped to confirm that the impact of higher temperatures is largely on the *growth rate* of GDP, as opposed to the *level* of GDP. But the size of the impact is imprecisely measured and applies only to small changes in temperature, not the 5° or more of warming that many people worry about. The same is true for studies of the effects of temperature on agricultural output.

A second problem is that there is little or nothing in the way of economic theory to help us understand the potential impact of higher temperatures. We have some sense of how higher temperatures might affect agriculture, but we also know that losses of agricultural output in some regions of the world (e.g., near the equator) might be offset by increased output in other regions (e.g., northern Canada and Russia). Furthermore, agriculture is a small fraction of total economic output: 1 to 2 percent of GDP for industrialized countries, 3 to 20 percent of GDP for developing countries. Beyond agriculture, it is difficult to explain, even at a heuristic level, how higher temperatures will affect economic activity. That is why the loss functions in IAMs that relate temperature to GDP are essentially ad hoc, with little or no theoretical grounding.

A third problem is that climate change will occur slowly, which means there is considerable potential for adaptation. In the case of agriculture, we already saw substantial adaptation in the United States during the nineteenth century as settlers moved west and had to adapt crops to new and very different climatic conditions.[21] Flooding is a potential hazard of climate change if sea levels rise substantially, but here, too, we have seen adaptation in the past (with the dikes of Holland perhaps the best known example). This does not mean, however, that adaptation will eliminate the impact of climate change—it is simply another complicating factor that makes it very difficult to estimate any kind of loss function.

It may be that the relationship between temperature and the economy is not just something we don't know, but something that we *cannot* know, at least for the time horizon relevant to the design and evaluation of climate policy. Some researchers have come to the conclusion that climate sensitivity is in this category of the "unknowable."[22] Yet, for the reasons just discussed, the impact of climate change is even less "knowable" than climate sensitivity. This suggests that we will never (or at least not over any relevant time period) agree on an answer to the question posed at the beginning of this article: Should environmental economists push for the early adoption of a stringent GHG abatement policy?

Note that this does not mean the answer to this question is no, and that there is no case to be made for stringent GHG abatement. But I don't think the case can be made by applying Monte Carlo (i.e., random sample) simulation methods to one or more IAMs, or by calculating WTP based on "consensus" probability distributions as I have done earlier. Rather, the case for stringent abatement will have to be based on the possibility of a *catastrophic outcome*, something that is far outside the realm of these models and probability distributions.

Catastrophic Climate Change

Some environmentalists argue that without stringent abatement, a climate catastrophe is not just a possibility, but almost a sure thing. In a recent *New York Times* opinion piece, for example, James Hansen (2012) claims that if Canada extracts the oil from its reserves of tar sands "and we do nothing, it will be game over for the climate." Exactly what is meant by "game over" is unclear, but it certainly sounds catastrophic, and it is certainly not something embodied in any IAM that I am aware of. While a catastrophic climate outcome is indeed a possibility (with or without Canadian tar sands oil), it is something we know very little about.

For climate scientists, catastrophic outcomes almost always take the form of high temperature outcomes (e.g., a 7° or 8° increase by 2100). Putting aside the difficulty of estimating the

[21] Libecap and Steckel (2011) provide several detailed examples of this kind of adaptation.
[22] See, for example, Allen and Frame (2007), who show how the uncertain feedback effects that are central to the Roe-Baker model imply that we will never be able to determine an upper bound for climate sensitivity.

probability of such a large temperature increase, what matters in the end is not the temperature increase itself but rather the impact it would have. Would that impact be "catastrophic," and if so, in what sense? And might a smaller (and more likely) temperature increase also have a catastrophic impact? Is there some way to bring economic analysis to bear on the policy implications of possible catastrophic outcomes?

Although my calculations of WTP in the preceding section were based on full distributions of temperature outcomes, for all three distributions I considered, the probabilities for very high temperatures are low. However, as we saw in Figure 4, doubling the assumed impact of temperature on GDP growth results in a higher WTP. This is no surprise; we could increase the impact even more (e.g., so that 4° of warming would result in a 12 percent loss of GDP in 2100) and get a still higher WTP. The problem, as explained earlier, is that we have very little basis, empirical or theoretical, for determining the actual impact or even the overall functional relationship between temperature and GDP or GDP growth.

Furthermore, it is difficult to see how our understanding of the economic impact of rising temperatures is likely to improve in the coming years. Even more so than temperature change itself, economic impact may simply be in the realm of the "unknowable." If so, it would make little sense to try to use an IAM-based analysis to make the case for stringent abatement. Rather, the case would have to be based on the likelihood, even if it is small, of a catastrophic outcome in which climate change is sufficiently extreme to cause a very substantial drop in welfare.

How Likely? How Extreme?

How should we think about catastrophic climate outcomes? Given how little we know, it seems to me that a very detailed and complex modeling exercise is unlikely to be helpful. (Even if we really believed the model accurately represented the relevant physical and economic relationships, which is unlikely, we would have to come to agreement on key parameters, such as the rate of time preference.) Something simpler is probably needed, although not something as simple as the claims made by Hansen (2012) and others that a catastrophe is inevitable. Perhaps the best we can do is come up with rough subjective estimates of the probability of a climate change sufficiently large to have a catastrophic impact, and then some distribution for the size of that impact (in terms, say, of a reduction in GDP, or a reduction in the effective capital stock or the productivity of the capital stock).

This is the approach that has been used in recent studies of "consumption disasters," defined as events that caused consumption to decline by some substantial amount (say, more than 10 percent). In Barro (2009) and related work, disasters are modeled as random arrivals, the impact of which is a random percentage reduction in the capital stock (and thus in ongoing consumption), with the loss fraction given by a simple distribution such as a one-parameter power distribution. The mean arrival rate of disasters and the parameter of the impact distribution might be estimated from consumption data for a sample of countries over a century or more of time, or inferred from the behavior of macroeconomic and financial aggregates.[23] For

[23]Barro (2009) and Barro and Jin (2009) are examples of studies in which arrival rates and impact distributions are estimated from panel data. Pindyck and Wang (forthcoming) estimate the relevant parameters (including the IRRA) as calibration outputs from a general equilibrium model.

climate change, however, a catastrophic outcome has not yet occurred, so estimating impact parameters from panel data or from macroeconomic aggregates is not an option.[24]

The problem of assessing catastrophic climate outcomes is analogous to assessing the world's greatest catastrophic risk during the Cold War—the possibility of a thermonuclear exchange between the United States and the Soviet Union. What was the likelihood of such an event? There were no data or reliable models that could yield good estimates. Instead, analyses had to be based on the *plausible*, that is, on events that could reasonably be expected to play out, even if with low probability. The same approach had to be taken with respect to assessing the range of potential impacts of a thermonuclear exchange. Such analyses were useful because they helped evaluate the potential benefits (and risks) of arms control agreements.

It seems to me that the same approach could be used to assess climate change catastrophes. We could begin by asking what is a plausible range of catastrophic outcomes (under, for example, BAU), as measured by a percentage declines in the stock of productive capital (thereby reducing future GDP over time). That range could be discrete (e.g., three or more potential outcomes) or continuous. Next, what are plausible probabilities? Here, "plausible" would mean acceptable to a range of economists and climate scientists. Given these plausible outcomes and probabilities, one could calculate the WTP to avert those outcomes, or to reduce the probabilities of their occurrence. That WTP would once again depend on preference parameters (i.e., the rate of time preference and index of risk aversion). But if the WTP were sufficiently large and robust to reasonable ranges for those parameters, it might provide support for a stringent policy.

This approach does not carry the perceived precision that is part of an IAM-based analysis. But that perceived precision is likely to be illusory. To the extent that we are dealing with unknowable quantities, it may be that the best we can do is rely on the "plausible."

Multiple Potential Catastrophes

So suppose an analysis of climate change catastrophes based on "plausible" outcomes and probabilities indicates a high WTP, something around 10 percent of GDP, for a reasonable range of preference parameters. Are we home yet? Would we then have what we need to make a strong case for a stringent abatement policy?

Maybe not. The problem is that we must consider other potential catastrophic events. A climate catastrophe is only one of a number of potential catastrophes that could cause major damage on a global scale. Different people have different nightmares, but my list would include a nuclear or biological terrorist attack (far worse than 9/11), a highly contagious "mega-virus" that spreads uncontrollably, or an environmental catastrophe unrelated to GHG emissions and climate change (perhaps related to toxic waste or severe water shortages).[25] These other

[24]Although Dell, Jones, and Olken (2009, 2012), and Bansal and Ochoa (2011a, 2011b) estimated the impact of temperature change on economic growth using historical data for a panel of countries, both the temperature variations and changes in growth rates were small and nothing close to what might be considered "catastrophic."

[25]For those readers lacking imagination, see Posner (2004) and Bostrom and Ćirković (2008). For a sobering discussion of the likelihood and possible impact of nuclear terrorism, see Allison (2004).

potential catastrophes may be just as likely, or even more likely, to occur than a climate catastrophe, and they could occur much sooner and with much less warning (and thus less time to adapt). And as with climate, the likelihood and/or impact of these catastrophes could be reduced by taking costly action now.

The "plausible" outcomes and probabilities approach just outlined could also be used to assess these other possible catastrophes and evaluate policies to avert them. Let's consider nuclear terrorism. Various studies have assessed the likelihood and potential impact of the detonation of one or more nuclear weapons (with the yield of the Hiroshima bomb) in major cities. "Plausible" estimates of the probability of this occurring in the next ten years vary. Allison (2004) puts the probability at 50 percent; others put it at 1 to 5 percent. What are plausible estimates of the impact? A million or more deaths, along with a shock to the capital stock and GDP resulting from reduced trade and economic activity worldwide, as vast resources are devoted to averting further events.[26]

Suppose the WTP to avert or reduce the probability of a nuclear catastrophe of this sort is 10 percent of GDP. How would this affect the analysis of a climate catastrophe? Suppose that with no other potential catastrophes, the WTP to avoid a climate catastrophe is also 10 percent of GDP. But suppose there are also five or six other potential catastrophes (a mega-virus, a severe water shortage; you can come up with your own list), each with a WTP of 10 percent. How will the WTP for averting a climate catastrophe change once we take into account the other potential catastrophes? It is likely to go down, as will the individual WTPs for the other catastrophes. The reason is that the WTPs are not additive; society would probably be unwilling to spend 60 or 70 percent of GDP to avert all of these catastrophes. Thus, when taken as a group, the WTP for each potential catastrophe (including climate) would fall.

It is important to understand that the inclusion of other potential catastrophes would affect the WTP for climate in two opposing ways. On the one hand, the nonclimate potential catastrophes would reduce the expected growth rate of GDP, thereby reducing expected future GDP and increasing expected future marginal utility *before* a climate catastrophe occurs. This in turn increases the benefit of avoiding the climate catastrophe. On the other hand, assuming all of these potential catastrophes are equally threatening, a large fraction of GDP would be needed to keep us safe. This "income effect" would reduce the WTP for climate. Unless the number of potential catastrophes is small, this "income effect" will dominate, so that the WTP for climate will fall.

Unfortunately, society faces a number of potentially catastrophic threats, and opinions will differ about the size of that number and which threats are worse than others. Climate change is probably one of those catastrophic threats, but it is unclear where it would rank in a list ordered by likelihood and potential severity. The point is that when evaluating the expenditure of scarce resources to reduce the likelihood of a climate catastrophe, these other catastrophic threats should not be ignored.

Conclusions

I began this article by asking whether environmental economists should push for the early adoption of a stringent GHG abatement policy. I have argued that the *economic* case for

[26]For a discussion and survey of studies of nuclear terrorism, see Ackerman and Potter (2008).

stringent GHG abatement cannot be made based on "most likely outcomes" (i.e., distributions for temperature change consistent with the IPCC [2007]) and the economic impact functions used in most IAMs (which were also surveyed by the IPCC). I have also argued that it doesn't matter whether the temperature distributions are fat- or thin-tailed because even if we assume that moderate temperature change (say 4°C) would have a large economic impact (say a 6 percent loss of GDP), the WTP for a stringent policy will be small for most "reasonable" values of key parameters. For example, it is hard to justify even a moderate abatement policy if the rate of time preference is 1 or 2 percent rather than zero, or if the index of risk aversion is 2 or greater. Some have argued that "ethics" should dictate these parameter values. However, while a rate of time preference of zero might reflect the ethical values of some economists, I am aware of no evidence that it reflects the preferences of society as a whole.

I have also argued that the greatest area of uncertainty concerns the economic impact (including health and social impacts) of climate change. The economic loss functions that are part of most IAMs are essentially ad hoc. This is not surprising given how little we know—in terms of both theory and data—about the ways and extent to which changes in temperature and other climate variables are likely to affect the economy. In fact, the economic impact of climate change may well be in the realm of the "unknowable." This in turn means that IAM-based analyses of climate change may not take us very far, and the models may be of very limited use as a policy tool.

By utilizing WTP, I have avoided having to deal with the cost (or "supply") side of climate policy, and have focused instead on the "demand" side (in that WTP is society's reservation price for achieving a policy objective). Clearly, the cost side is relevant when evaluating policy. If it can be demonstrated that a policy objective (e.g., limiting T in 2100 to 3°C or less) can be achieved at low cost—say 1 percent of GDP—then a WTP of only 2 percent is more than sufficient to justify the policy. At this point, estimates of cost are not that small, but that may change as better technologies for reducing emissions or sequestering carbon become available.

I have argued here that any case for stringent abatement must be based on the possibility of a *catastrophic* climate outcome. And a "catastrophic climate outcome" does not simply mean a very high increase in temperature and rising sea levels; what matters is whether the *economic* impact of those physical changes would be catastrophic. Furthermore, simply stating that such an outcome is possible is not enough. We need "plausible" estimates of probabilities of various temperature outcomes, and "plausible" estimates of large economic impacts from those temperature outcomes. Thus far, the claims of likely catastrophic outcomes that I have seen (made largely by climate scientists, not economists) relate to temperature and other physical processes (such as climate variability and changes in sea levels). Generally absent is a clear analysis showing that the economic impact of these physical changes would be so large as to be catastrophic. Completely absent (to my knowledge) is any analysis that puts a climate catastrophe in the context of a set of potential global catastrophes.

So should environmental economists push for stringent GHG abatement? In the end, I have not answered this question. Instead I have tried to explain why the question is so inherently difficult, why an answer won't come from the kinds of modeling exercises that have permeated the literature, and why the case for stringent abatement—if that case is to be made at all—must be based on an analysis of potential catastrophic outcomes. This, I would argue, should be the focus of future climate policy research.

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Abstract

Climate policy poses a dilemma for environmental economists. The economic argument for stringent greenhouse gas (GHG) abatement is far from clear. There is disagreement among both climate scientists and economists concerning the likelihood of alternative climate outcomes, the nature and extent of the uncertainty of those outcomes, and the framework that should be used to evaluate potential benefits from GHG abatement, including key policy parameters. I argue that the case for stringent abatement—if it can be made at all—cannot be based on the kinds of modeling exercises that have permeated the literature thus far, but instead must be based on the possibility of a catastrophic outcome. I discuss how an analysis that incorporates such an outcome might be conducted. (*JEL*: Q54; D81, Q51)