

THE USE AND MISUSE OF MODELS FOR CLIMATE POLICY^{*}

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Abstract: In recent articles, I have argued that integrated assessment models (IAMs) have flaws that make them close to useless as tools for policy analysis. IAM-based analyses of climate policy create a perception of knowledge and precision that is illusory, and can fool policy-makers into thinking that the forecasts the models generate have some kind of scientific legitimacy. But some have claimed that we need some kind of model, and that IAMs can be structured and used in ways that correct for their shortcomings. For example, it has been argued that although we know little or nothing about key relationships in the model, we can get around this problem by attaching probability distributions to various parameters and then simulating the model using Monte Carlo methods. I argue that this would buy us nothing, and that a simpler and more transparent approach to the design of climate change policy is preferable. I briefly outline what that approach would look like.

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“Pay no attention to the man behind the curtain!”

L. Frank Baum, *The Wonderful Wizard of Oz*.

1. Introduction.

In a recent article, I argued that integrated assessment models (IAMs) “have crucial flaws that make them close to useless as tools for policy analysis.”¹ In fact, I would argue that calling these models “close to useless” is generous: IAM-based analyses of climate policy create a perception of knowledge and precision that is illusory, and can fool policy-makers into thinking that the forecasts the models generate have some kind of scientific legitimacy. IAMs can be misleading – and are inappropriate – as guides for policy, and yet they have been used by the government to estimate the social cost of carbon (SCC) and evaluate tax and abatement policies.²

What are the crucial flaws that make IAMs so unsuitable for policy analysis? They are discussed in detail in Pindyck (2013b), but the most important ones can be briefly summarized as follows:

1. Certain inputs – functional forms and parameter values – are arbitrary, but have huge effects on the results the models produce. An example is the discount rate. There is no consensus among economists as to the “correct” discount rate, but different rates will yield wildly different estimates of the SCC and the optimal amount of abatement that any IAM generates. For example, these differences in inputs largely explain why the IAM-based analyses of Nordhaus (2008) and Stern (2007) come to such strikingly different conclusions regarding optimal abatement. Because the modeler has so much freedom in choosing functional forms, parameter values, and other inputs, the model can be used to obtain almost any result one desires, and thereby legitimize what is essentially a subjective opinion about climate policy.
2. We know very little about *climate sensitivity*, i.e., the temperature increase that would eventually result from a doubling of the atmospheric CO₂ concentration, but this is a key input to any IAM. The problem is that the physical mechanisms that determine climate sensitivity involve crucial feedback loops, and the parameter values that determine the

¹ Pindyck (2013b). Some of these arguments are also discussed in Pindyck (2013a).

² See the technical reports from the Interagency Working Group on Social Cost of Carbon (2010, 2013), which used three IAMs to arrive at estimates of the SCC. Also, see Greenstone, Kopits and Wolverton (2013) for an illuminating explanation of the process used by the Interagency Working Group to estimate the SCC.

strength (and even the sign) of those feedback loops are largely unknown, and are likely to remain unknown for the foreseeable future.³ As Freeman, Wagner, and Zeckhauser (2015) have shown, over the past decade our uncertainty over climate sensitivity has increased.

3. One of the most important parts of an IAM is the *damage function*, i.e., the relationship between an increase in temperature and GDP (or the growth rate of GDP). When assessing climate sensitivity, we can at least draw on the underlying physical science and argue coherently about the relevant probability distributions. But when it comes to the damage function, we know virtually nothing – there is no theory and no data that we can draw from.⁴ As a result, developers of IAMs simply make up arbitrary functional forms and corresponding parameter values.
4. IAMs can tell us nothing about the likelihood or possible impact of a catastrophic climate outcome, e.g., a temperature increase above 5°C that has a very large impact on GDP. And yet it is the possibility of a climate catastrophe that is (or should be) the main driving force behind a stringent abatement policy.

Although many would agree that IAMs have their flaws, some (e.g., Metcalf and Stock (2015) and Weyant (2015) in their articles for this symposium) might argue that my assessment of their usefulness for policy analysis is too harsh, and that if used properly, the models could help us formulate and evaluate alternative climate policies. Arguments in support of the development and use of IAMs include the following:

1. All models have flaws – after all, any model is a simplification of reality – and yet economists build and use models all the time. A related argument is that the complexity of climate change and its economic impact makes it especially important to have some kind of model to account for the dynamic interactions among key variables, and to guide our thinking about policy design.

³ See, e.g., Roe and Baker (2007). Allen and Frame (2007) argue our lack of knowledge will not change in the coming years, and that climate sensitivity is essentially “unknowable.”

⁴ There is a large and growing literature on the use of weather data to estimate the impact of temperature (and other measures of climate), especially with respect to agriculture. For surveys of this literature, see Auffhammer et al. (2013) and Dell, Jones and Olken (2014). However, these studies are limited to short time periods and small fluctuations in temperature and other weather variables. They do not, for example, describe what has happened over 20 or 50 years following a 5°C increase in mean temperature, and thus cannot enable us to specify and calibrate IAM damage functions.

2. Yes, there is uncertainty over climate sensitivity, and we know very little about the damages likely to result from higher temperatures. But can't our uncertainty over climate sensitivity or the "correct" damage function be handled by assigning probability distributions to certain key parameters and then running Monte Carlo simulations?
3. We have no alternative. We must develop the best models possible in order to estimate the social cost of carbon and/or evaluate particular policies. In other words, working with even a highly imperfect model is better than having no model at all.
4. Finally, if we don't use IAMs, how can we possibly estimate the SCC and evaluate alternative GHG abatement policies? Should we rely instead on expert opinion? And don't experts have some kind of implicit mental models that drive their opinions? If so, isn't it better to make the model explicit?

Put simply, even with their faults, can't IAMs be useful as a tool to help inform policy? That is the question I address in this paper. In doing so, I respond to the arguments mentioned above. In the next section I briefly discuss the use and misuse of IAMs, i.e., how the models can indeed be helpful, versus how they can be misleading. I then address the question of whether our uncertainty over climate sensitivity and climate impacts can be handled by assigning probability distributions to various parameters and then running Monte Carlo simulations. (The answer is no.) Next I turn to the issue of scientific honesty. I will argue that the use of IAMs to estimate the SCC or evaluate alternative policies is in some ways dishonest, in that it creates a veneer of scientific legitimacy that is misleading. Finally, I address the question of what we can rely on to formulate climate policy if not IAMs. I argue that the best we can do is rely on "expert" opinion, perhaps combined with relatively simple, transparent, and easy-to-understand models. After all, the ad hoc equations that go into most IAMs are no more than reflections of the modeler's own "expert" opinion.

2. The Use and Misuse of Climate Change Models.

Economists find models useful because they provide a logically consistent way to organize our thinking about the relationships among variables of interest. They help us understand the implications of those relationships, and identify the roles of various functional forms and parameter values. That is what made the early efforts at climate change modeling so valuable. The models developed by Nordhaus (1991) and others over two decades ago were

early attempts to integrate climate science with the economic effects of greenhouse gas (GHG) emissions. Those models helped economists understand how GHG emissions accumulate in the atmosphere, how that accumulation can affect global mean temperatures, and how higher temperatures might affect GDP and consumption. By including a social welfare function that values the flow of consumption over time, the models can also be used to illustrate the possible welfare effects of different GHG abatement policies.

In effect, these early IAMs can be viewed as pedagogical devices. And indeed, Nordhaus (2013) uses his DICE (Dynamic Integrated Climate and Economy) model to help explain – at a textbook level – how unrestricted GHG emissions can cause climate change and lead to serious problems in the future. The book also utilizes the model to illustrate some of the uncertainties we face when thinking about the climate system and when trying to predict the changes to expect under different policies. The book thereby provides students (and others) with a good introduction to climate change policy.

So far, well and good. The problem comes up when we take these models so seriously that we use them to try to evaluate alternative policies and/or come up with an “optimal” (i.e., welfare-maximizing) policy. Yes, economists often build and use models, but usually they understand the limits of those models. They know that a model can help to tell a story in a logically coherent way, but the model might not be able to provide the numerical details of the story. In other words, the model might not be suitable for forecasting or quantitative policy analysis. That is the case for the various versions of the Nordhaus DICE model, as well as the plethora of IAMs (most much more complex than DICE) that have been developed over the past couple of decades. As I explained in my earlier article, the key relationships and parameter values in these models have no empirical (or even theoretical) grounding, and thus the models cannot be used to provide any kind of reliable quantitative policy guidance.

Is there any way around this problem? Can IAMs be salvaged as a tool for policy analysis if we somehow account for our lack of knowledge about key relationships and parameter values? I turn to that question next.

3. The Treatment of Uncertainty.

Some developers of IAMs understand that there is considerable uncertainty over climate

sensitivity and that we don't know what the "correct" damage function is. But they think they have a solution to this problem. They believe that the uncertainty can be handled by assigning probability distributions to certain parameters and then running Monte Carlo simulations. Unfortunately this won't help. The problem is that we don't know the correct probability distributions that should be applied to various parameters, and different distributions – even if they all have the same mean and variance – can yield very different results for expected outcomes, and thus for estimates of the SCC.⁵

To make matters worse, we don't even know the correct functional forms for some of the key relationships. This is particularly a problem when it comes to the damage function. The damage function used in the Nordhaus DICE model, for example, is a simple inverse-quadratic relationship:

$$L(T) = 1 / (1 + \pi_1 T + \pi_2 T^2) \quad (1)$$

Here T is the anthropomorphic increase in temperature, and $L(T)$ gives the reduction in GDP and consumption for any value of T . (Thus $\text{GDP} = L(T)\text{GDP}'$, where GDP' is what GDP would be if there were no warming.) But remember that this damage function is made up out of thin air. It isn't based on any economic (or other) theory, or any data. Furthermore, even if this inverse-quadratic function were somehow the true damage function, there is no theory or data that can tell us the values for the parameters π_1 or π_2 , or the correct probability distributions for those parameters, or even the correct means and variances.

For example, suppose we (somehow) chose probability distributions for π_1 and π_2 . A Monte Carlo simulation would then give us the expected loss $L(T)$ for any particular temperature increase T . But suppose that we then come to believe that damages are likely to rise very rapidly as T grows, more rapidly than eqn. (1) would indicate. This might lead us to conclude that the damage function should be an inverse-cubic, rather than quadratic. For example, we might decide that the following damage function is preferred:

$$L(T) = 1 / (1 + \pi_1 T + \pi_2 T^3) \quad (2)$$

The Monte Carlo simulation will now give us a very different expected loss. Likewise, one

⁵ In Pindyck (2013a), I took three different but plausible distributions for temperature change: a gamma distribution, a Frechet distribution (also called a Generalized Extreme Value, Type 2 distribution), and the distribution derived by Roe and Baker (2007). I calibrated all three distributions so they have the same mean and variance, and I demonstrated that they imply very different values for the willingness to pay (WTP) to avoid the temperature change. In Pindyck (2007), I discuss the implications of uncertainty for environmental policy more generally.

might argue that we are using the wrong probability distributions for π_1 and π_2 , or we have the correct distributions but the wrong means and/or variances. Changing the probability distributions or the means and variances of the distributions will also lead to a very different estimate of the expected loss.

The basic problem here is that the probability distributions are completely arbitrary, as is the damage function that we are applying them to. What can we learn from assigning arbitrary probability distributions to the parameters of an arbitrary function and running Monte Carlo simulations? Nothing. The bottom line here is quite simple: If we don't understand how A affects B, but we create some kind of arbitrary model of how A affects B, running Monte Carlo simulations of the model won't make up for our lack of understanding.

4. Scientific Honesty.

The argument is sometimes made that we have no choice; without a model we will end up relying on biased opinions, guesswork, or even worse. Thus we must develop the best models possible, and use them to evaluate alternative policies. In other words, working with even a highly imperfect model is better than having no model at all. That might be true if we were honest and upfront about the limitations of the model. But often we are not.

Models sometimes convey the impression that we know much more than we really do. They create a veneer of scientific legitimacy that can be used to bolster the argument for a particular policy. This is particularly the case for IAMs, which tend to be large and complicated, and are sometimes poorly documented. IAMs are typically made up of many equations, and the equations are hard to evaluate individually (especially given that they are often ad hoc and without a theoretical or empirical foundation), and even harder to understand in terms of their interactions as a complete system. In effect, the model is just a black box: we put in some assumptions about GHG emissions, discount rates, etc., and we get out some results about temperature change, damages, etc. And because the black box is "scientific," we are supposed to take those results seriously and use them for policy analysis.

To understand the problem, go back 40 years or so and recall the "Limits to Growth" controversy. It was based on a simple sequence of ideas that appeared quite reasonable to some environmentalists at the time: (1) The earth contains finite amounts of oil, coal, copper, iron, and other nonrenewable resources. (2) These resources are important inputs for the production of a

large fraction of GDP. (3) Because they are finite, we will eventually run out of these resources. In fact, partly because of population growth, we are likely to run out very soon. (4) When we run out, the world's developed economies will contract dramatically, greatly reducing our standard of living, and even causing wide-spread poverty. (5) Therefore, we should immediately and substantially reduce our use of natural resources (and slow or stop population growth). This will reduce our standard of living now, but will give us time to adapt and will push back (or even avoid) that day of reckoning when our resources run out and we are reduced to abject poverty.

Points (1) and (2) are indisputable. Points (3), (4) and (5), however, ignore basic economics. As reserves of oil, copper, and other resources are depleted, the costs of extraction and therefore the prices of these resources will rise, causing their use to decline. Higher prices also create the incentive to find substitutes. Thus we may never actually run out of these resources, although we will eventually stop using them. Most important, given the incentives created by rising prices and the likelihood of finding substitutes, there is no reason to expect the gradual depletion of natural resources to result in economic decline. Indeed, partly due to technological change and partly due to the discovery of new reserves, the real prices of most resources have gone down over the past 40 years, and there is no evidence that reserve depletion has been or is likely to be a drag on economic growth.

Although it made little economic sense, the "Limits to Growth" argument gained considerable traction in the press and in public discourse over environmental policy. This was due in part to a lack of understanding of basic economics on the part of the public (and many politicians). But it was also due to the publication and promotion of some simulation models that gave the "Limits" argument a veneer of scientific legitimacy. The most widely promoted and cited models were those of Forrester (1973) and Meadows et al (1974). These models were actually quite simple; as Nordhaus (1973, 1992) and others explained, they essentially boiled down to an elaboration of points (1) to (5) above, with some growth rates and other numbers attached. What mattered, however, was that these models required a *computer* to solve and simulate. The fact that some of the underlying relationships in the models were completely ad hoc and made little sense didn't matter – the fact that they were *computer models* made them "scientific" and inspired a certain degree of trust.

Still another example of an attempt to create a veneer of scientific legitimacy is the "technical analysis" used by stock market analysts to make buy/sell recommendations for

particular stocks and for the market as a whole. Sometimes this involves a formal computer model, and sometimes just an “analysis” of the up-and-down movements of what is essentially a random walk. By dressing up the “analysis” with terms like “resistance levels,” “support points,” potential or actual “breakouts,” etc., the buy/sell recommendations of technical analysts are given a scientific aura: Uninformed (or misinformed) investors think these recommendations are based on some kind of “science,” even though countless studies have shown that “technical analysis” is totally uninformative.

I do not mean to equate IAMs with the “Limits to Growth” models of the early 1970s, never mind the models used by those who promote the “technical analysis” of stock prices. Developers of IAMs generally try to base their models’ equations as much as possible on climate science and economic principles. Unfortunately, climate science and economic principles can’t tell us much about how these equations should be specified and parameterized, which is why IAMs cannot tell us much about the design of climate policy. The problem has been that the developers and users of IAMs have tended to oversell their validity, and have failed to be clear about their inadequacies. The result is that policy makers who rely on the projections of IAMs are being misled.

I believe that we need to be much more honest and upfront about the inherent limitations of IAMs. Claiming that IAMs can be used to evaluate policies and determine the SCC is misleading to say the least, and gives economics a bad name. If economics is indeed a science, scientific honesty is paramount.

5. Isn’t the Use of IAMs the Best We Can Do?

Suppose our job is to come up with an estimate of the SCC, which will be used as a basis for determining the size of a carbon tax. We know that IAMs are deeply flawed, but aren’t they still the best game in town? If we acknowledge these flaws and explain that the projections and SCC estimates that are generated have large standard errors, isn’t the use of one or more IAMs better than having no model? Not necessarily. Putting aside the question of scientific honesty, there are three additional problems with the use of IAMs.

5.1. The Modeler Has Too Much Flexibility.

Put simply, it is much too easy to use a model to generate, and thus seemingly validate, the results one wants. Take any one of the three IAMs that were used by the Interagency

Working Group (2010, 2013) to estimate the SCC.⁶ With a judicious choice of parameter values (varying the discount rate is probably sufficient), the model will yield an SCC estimate as low as a few dollars per ton, as high as several hundred dollars per ton, or anything in between. Thus a modeler who, for whatever reason, believes that a stringent abatement policy is (or is not) needed, can choose a low (or high) discount rate, or choose other inputs that will yield the desired results.

The Interagency Working Group did not try to determine the “correct” values for the discount rate. Instead, they used middle of the road assumptions about the discount rate as well as other parameters, and arrived at an estimate of around \$33 per ton for the SCC (recently updated to \$39 per ton). But other well-known studies have deviated from using these middle-of-the-road assumptions and arrived at very different estimates of the SCC. For example, using a version of his DICE model (one of the three models used by the Interagency Working Group), Nordhaus (2011) obtained an estimate of \$11 per ton for the SCC. On the other hand, Stern (2007), using the PAGE model, obtained optimal abatement policies consistent with an SCC of over \$200 per ton. Although the models differed, the main reason for these wildly different SCC estimates is that Nordhaus used a relatively high discount rate, and Stern a relatively low rate.

The problem here is that there is no consensus regarding the “correct” discount rate. (The Interagency Working Group simply chose a mid-range number – 3 percent – that the members of the Group could all live with; the Group’s reports never claimed that this number was in any sense “correct.”) Because reasonable arguments can be made for a low discount rate or for a high rate, the modeler simply has too much flexibility. If the modeler is at all biased towards a more or less stringent abatement policy, he/she can choose the discount rate accordingly. And while I have focused on the discount rate, IAMs have other parameters whose choice can lead to a higher or lower SCC estimate, as I discuss in Pindyck (2013a).

5.2. The Choice of Model is Largely Irrelevant.

Suppose we could take away the flexibility that the modeler has in choosing parameters. Perhaps some government agency tells the developer of each model to use a specific set of parameter values, including the discount rate. Are we then home free?

⁶ The three IAMs were DICE (Dynamic Integrated Climate and Economy), PAGE (Policy Analysis of the Greenhouse Effect), and FUND (Climate Framework for Uncertainty, Distribution, and Negotiation). For descriptions of the models, see Nordhaus (2008), Hope (2006), and Tol (2002).

You might say that first we need to decide which model to rely on. But the choice of model illustrates a second problem. Let's go back to the wildly different SCC estimates of Nordhaus (\$11) and Stern (\$200 plus). Which one should we rely on? The answer boils down almost entirely to our belief about the discount rate. The choice of model – DICE versus PAGE versus some other IAM – doesn't matter all that much. Yes, for a fixed set of parameter values DICE will give a different SCC estimate than PAGE, but the difference will be small compared to the effect of changing the discount rate for any one model.

If one believes that we should use market-based discount rates (i.e., the rates we actually observe in financial markets), then \$11 is roughly the right number for the SCC. But if instead, one believes (perhaps based on some kind of “ethical” argument regarding intergenerational welfare comparisons) that we should use a very low discount rate, then \$200 or so is the right number. The point here is that there is hardly any need for a model; decide on the discount rate, and you pretty much have an estimate of the SCC. The model itself is almost a distraction.

Why is the SCC determined almost entirely by the discount rate, rather than by the specific IAM used in the analysis? The reason is that the impact of GHG emissions on climate is a very slow and gradual process. Even with no abatement, most studies indicate that any significant warming will not occur for several decades. Thus the costs of a GHG abatement policy are incurred starting now, but most of the benefits come in the distant future. If those future benefits are discounted at a market-based rate (say around 5%), their present value will be very small, and the implied SCC will be very small. To get a large SCC, we need to discount future benefits at a very low rate (say around 1%). So, is the SCC small or large? To answer that, we only have to agree on the discount rate.⁷ We don't have to agree on which model to use.

5.3. Catastrophic Outcomes.

As I stated in the Introduction and explained in detail in my 2013 articles, what really matters for the SCC is the likelihood and possible impact of a catastrophic climate outcome: a much larger-than-expected temperature increase and/or a much larger-than-expected reduction in GDP caused by even a moderate temperature increase. IAMs, however, simply cannot account

⁷ Economists are sharply divided on the discount rate that should be used for the analysis of climate change policy. There is a large and growing literature on the discount rates (plural, because some argue that the rate should decline over time) that should be used for very long time horizons. For an overview, see Gollier (2013).

for catastrophic outcomes. It is easy to see why by looking at eqns. (1) and (2), which are alternative representations for the damage function.

Eqn. (1) is the damage function used in the DICE model. The parameters π_1 and π_2 are chosen to be roughly consistent with the common wisdom regarding the loss of GDP likely to result from T in the range of 1° to 4°C. That common wisdom, which might be totally wrong, puts the loss for these kinds of temperature increases at a few percent. The problem is that these damage functions tell us nothing about what to expect if the temperature increases are larger, e.g., 5° C or more. Given the arbitrary nature of eqn. (1), putting in $T = 5$ or 7 is a meaningless exercise, and will tell us nothing about the damages we should expect if the temperature were indeed to increase this much. Because of the cubic term, eqn. (2) will yield much higher damage numbers for $T = 5^\circ$ or more, but eqn. (2) is just as arbitrary, and the damage numbers will be just as meaningless.

How do we know that the possibility of a catastrophic outcome is what matters for the SCC? Because unless we are ready to accept a discount rate that is very small, the “most likely” scenarios for climate change simply don’t generate enough damages – in present value terms – to matter.⁸ That is why the Interagency Working Group, which used a 3 percent discount rate, obtained the rather low estimate of \$33 per ton for the SCC.

6. So What to Do?

Focusing on catastrophic outcomes actually simplifies the problem somewhat. First, it is only *economic* outcomes that matter, not the causes of the outcomes. In other words, it doesn’t matter whether a large drop in GDP is the result of a dramatic increase in temperature (but a moderate effect of temperature on output) or a moderate increase in temperature (but a dramatic effect of temperature on output). What we have to worry about is the possibility of a drop in GDP so large as to be considered catastrophic. (Of course climate change could also cause non-economic damages, such as greater morbidity and mortality, the extinction of species, and social disruptions. I am assuming – as is typically done in the estimation of the SCC – that these non-economic damages could all be monetized and included as part of the drop in GDP.)

⁸ I show this formally in Pindyck (2011, 2012).

Starting with some scenario for GHG emissions (e.g., no abatement), we could therefore begin by considering a plausible range of catastrophic outcomes, as measured by percentage declines in GDP broadly defined. Next, what are plausible probabilities that we can attach to these possible outcomes? Here, “plausible” would mean acceptable to a range of economists and climate scientists. Given these plausible outcomes and probabilities, one can calculate the present value of the benefits from averting those outcomes, or reducing the probabilities of their occurrence. In present value terms, the benefits will depend on the discount rate and perhaps other parameters, but if those benefits are sufficiently large and robust to reasonable ranges for those parameters, it would support a stringent abatement policy. Let’s denote this present value of benefits by B .

The second step would be to ask how great the reduction in annual CO₂ emissions would have to be to avoid these catastrophic outcomes. Sum these annual reductions over some time horizon (say 50 or more years), and denote the total reduction by ΔE . Given B and ΔE , a rough estimate of the SCC is just $B/\Delta E$.

Determining plausible outcomes and probabilities, and the emission reductions needed to avert these outcomes, would mean relying on “expert” opinion. For an economist, this is not very satisfying. Economists often build models to *avoid* relying on subjective (expert or otherwise) opinions. But remember that the inputs to IAMs (equations and parameter values) are already the result of “expert” opinion; in this case the modeler is the “expert.” And of course experts are likely to disagree, particularly when it comes to climate change, where our knowledge is so limited. On the other hand, focusing on the extreme tail (i.e., catastrophic outcomes), and the emission reductions needed to eliminate that tail, may reduce the extent of disagreement, and will center the debate on what really matters as the driver of policy. Compared to agreeing on the details of some IAM, it should be relatively easy for climate scientists and economists to reach a consensus on the answers to the questions raised above, or at least agree on a range of answers.

In effect, we would use expert opinion to determine the inputs to a simple, transparent, and easy-to-understand model (and I stress the importance of easy-to-understand). As an example of how this might be done, start with three or four potential catastrophic outcomes that, under BAU, might occur at, say, 50 years in the future. Those outcomes might be a 10%, 30%, or 50% drop in GDP and consumption (or something worse). Now attach probabilities to those

outcomes, say .2, .1, and .05 respectively (so the probability of no catastrophe is .65). Given these outcomes and probabilities, and given a discount rate, we can calculate the present value of the expected benefits from avoiding these outcomes. Next, come up with an estimate (or set of estimates and associated probabilities) of the reduction in CO₂ emissions needed to eliminate the catastrophic scenarios. A simple ratio then gives us an estimate of the SCC. Of course the result will still depend on the discount rate that is used, so we might use a range of discount rates.

Yes, the calculations I have just described constitute a “model,” but one that is exceedingly simple and straightforward, and involves no pretense that we know the damage function, the feedback parameters that affect climate sensitivity, or other details of the climate-economy system. And yes, some experts might base their opinions on one or more IAMs, on a more limited climate science model, or simply on their research experience and/or general knowledge of climate change and its impact. That’s fine, because we are using a range of expert opinions to summarize our current understanding of catastrophic climate outcomes, and the range of disagreement over those outcomes.

Some might argue that the approach I have outlined above is insufficiently precise. But I believe we have no choice. Building and using elaborate models might let us think that we are approaching the climate policy problem more scientifically, but like the Wizard of Oz, we would just be drawing a curtain around our lack of knowledge.

7. Conclusions.

I have stressed that as economists, we need to be honest about what we know and don’t know about climate change and its impact. Just as financial economists would (or should) be ashamed to sell “technical analysis” to investors, environmental economists should be ashamed to claim that IAMs can forecast climate change and its impact, or tell us what the SCC is.

Atmospheric scientists have made great progress in understanding how weather patterns develop and change, but they don’t claim to be able to forecast next month’s weather or when the next hurricane will arrive. There has also been great progress in our understanding of the drivers of climate, how GHG emissions can affect climate, and (to a lesser extent) how changes in climate can affect GDP and other economic variables. But that progress does not enable us to build and use IAMs as tools for forecasting and policy analysis, and we would be deluding ourselves if we thought otherwise.

It would be nice if the problem were simply imprecise knowledge of certain parameters, so that uncertainty could be handled by assigning probability distributions to those parameters and then running Monte Carlo simulations. Unfortunately, not only don't we know the correct probability distributions that should be applied to these parameters – we can't even write down the correct equations to which those parameters apply.

This does not mean we have to throw up our hands and give up on the estimation of the SCC and the analysis of climate change policy more generally. I have argued that the problem is somewhat simplified by the fact that what matters for policy is the possibility of a catastrophic climate outcome. How probable is such an outcome (or set of outcomes), and how bad would they be? And by how much would emissions have to be reduced to avoid these outcomes? I have argued that the best we can do at this point is come up with plausible answers to these questions, perhaps relying at least in part on consensus numbers supplied by climate scientists and environmental economists. This kind of analysis would be simple, transparent, and easy-to-understand. It might not inspire the kind of awe and sense of scientific legitimacy conveyed by a large-scale IAM, but that is exactly the point. It would draw back the curtain and clarify our beliefs about climate change and its impact.

References

- Allen, Myles R., and David J. Frame, “Call Off the Quest,” *Science*, October 26, 2007, **318**, 582—583.
- Auffhammer, Maximilian, Solomon M. Hsiang, Wolfram Schlenker, and Adam Sobel, “Using Weather Data and Climate Model Output in Economic Analysis of Climate Change.” National Bureau of Economic Research Working Paper 19087, 2013.
- Dell, Melissa, Benjamin F. Jones, and Benjamin A. Olken, “What Do We Learn from the Weather? The New Climate-Economy Literature,” *Journal of Economic Literature*, Sept. 2014, 52(3), 740—798.
- Forrester, Jay W., *World Dynamics*, Wright-Allen Press, 1973.
- Freeman, Mark C., Gernot Wagner, and Richard J. Zeckhauser, “Climate Sensitivity Uncertainty: When is Good News Bad?” National Bureau of Economic Research Working Paper 20900, January 2015.
- Gollier, Christian, *Pricing the Planet’s Future*, Princeton University Press, 2013.
- Greenstone, Michael, Elizabeth Kopits, and Ann Wolverton, “Developing a Social Cost of Carbon for U.S. Regulatory Analysis: A Methodology and Interpretation,” *Review of Environmental Economics and Policy*, 2013, 7(1), 23—46.
- Hope, Chris. 2006. “The Marginal Impact of CO₂ from PAGE2002: An Integrated Assessment Model Incorporating the IPCC’s Five Reasons for Concern.” *Integrated Assessment* 6 (1): 19–56.
- Interagency Working Group on Social Cost of Carbon, “Technical Support Document: Social Cost of Carbon for Regulatory Impact Analysis,” U.S. Government, February 2010.
- Interagency Working Group on Social Cost of Carbon, “Technical Support Document: Technical Update of the Social Cost of Carbon for Regulatory Impact Analysis,” U.S. Government, 2013.
- Meadows, Donella H., Dennis L. Meadows, Jorgen Randers, and William W. Behrens III, *The Limits to Growth: A Report for the Club of Rome’s Project on the Predicament of Mankind*, Universe Books, 1974.
- Metcalf, Gilbert E., and James Stock, “The Role of Integrated Assessment Models in Climate Policy: A User’s Guide and Assessment,” *Review of Environmental Economics and Policy*, 2015 (this issue).

- Nordhaus, William D., "World Dynamics: Measurement Without Data," *The Economic Journal*, Dec. 1973, Vol 83, No. 332 .
- Nordhaus, William D., "To Slow or Not to Slow: The Economics of the Greenhouse Effect," *The Economic Journal*, 1991, **101**, 920—937.
- Nordhaus, William D., "Lethal Model 2: The Limits to Growth Revisited," *Brookings Papers on Economic Activity*, 1992:2.
- Nordhaus, William D., *A Question of Balance: Weighing the Options on Global Warming Policies*, Yale University Press, 2008.
- Nordhaus, William D., "Estimates of the Social Cost of Carbon: Background and Results from the RICE-2011 Model," National Bureau of Economic Research Working Paper 17540, 2011.
- Nordhaus, William D., *The Climate Casino*, Yale University Press, 2013.
- Pindyck, Robert S., "Uncertainty in Environmental Economics," *Review of Environmental Economics and Policy*, Winter 2007.
- Pindyck, Robert S., "Modeling the Impact of Warming in Climate Change Economics," Chapter 2 in G. Libecap and R. Steckel (Eds.), *The Economics of Climate Change*, University of Chicago Press, 2011.
- Pindyck, Robert S., "Uncertain Outcomes and Climate Change Policy," *Journal of Environmental Economics and Management*, February 2012.
- Pindyck, Robert S., "The Climate Policy Dilemma" *Review of Environmental Economics and Policy*, Summer 2013a, 7(2), 219—237.
- Pindyck, Robert S., "Climate Change Policy: What Do the Models Tell Us?" *Journal of Economic Literature*, September 2013b, 51(3), 860—872.
- Roe, Gerard H., and Marcia B. Baker, "Why is Climate Sensitivity So Unpredictable?" *Science*, October 26, 2007, **318**, 629—632.
- Stern, Nicholas, *The Economics of Climate Change: The Stern Review*, Cambridge University Press, 2007.
- Tol, R., "Estimates of the Damage Costs of Climate Change, Part I: Benchmark Estimates," *Environmental and Resource Economics*, 2002, **22**, 47—73.
- Weyant, John, "Contributions of Integrated Assessment Models," *Review of Environmental Economics and Policy*, 2015 (this issue).