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Overflowing with uncertainty: controversies regarding epistemic wagers in climate-economy models

Jonathan Metzger

> But it seems that we have, without knowing it, made an immensely dangerous bet: namely, that we'll be able to use the power and knowledge we have gained in the past couple of centuries to cope with the climate risks we've unleashed over the same period. Will we win that bet? Time will tell.
>
> Unfortunately, if the bet goes bad, we won't get another chance to play. (Krugman, 2013)

This chapter relates questions of overflow to epistemic politic*s* – the social process of establishing what constitutes valid and robust knowledge within a specific community of practice.[1] The community of practice in this case pertains to the scientific field of climate economics,[2] a subfield of economics that deals with the potential effects of climate change understood in economic terms and the potential costs and benefits of various measures geared to mitigating that change. It focuses on various policy measures undertaken to reduce the emission of greenhouse gases (GHGs), and specifically the most common (albeit not most potent) of these: carbon dioxide (CO_2). The vast majority of climate economists agree that human-induced emissions of GHGs are currently leading to a process of global warming. Yet there is broad disagreement among climate economists about the precise economic effects of this geophysical

1 I wish to thank Per Wikman Svahn and my colleagues in the Managing Overflow project, particularly Barbara Czarniawska, for their helpful comments on previous versions of this chapter. Of course, I am personally fully responsible for any remaining omissions and errors.

2 Sometimes interchangeably referred to as the 'economics of climate change' or the 'economics of global warming'.

process and what is to be considered a well-designed policy response to these changes.

To be able to model various possible economic outcomes of global climatic changes and the potential strengths and weaknesses of various policy responses, climate economists have generally relied on so-called Integrated Assessment[3] Models (IAMs). IAMs are advanced computer programs that integrate models of various types of natural and social processes, to arrive at conclusions regarding the potential interplay between the modeled processes. Climate-economic models are one specific type of IAM that integrate all these effects into a single commensurable currency – generally USD GDP. IAM modelers argue that the use of such models enables them to compress an overflow of disparate pieces of information into a comprehensible, systematic whole, to allow for rigorous policy analysis that can contribute to the development of economically efficient, or even 'optimal', responses to the identified overflow of climate gases in the atmosphere.

However, an increasing number of climate economists are disputing claims about the usefulness of the IAMs in guiding climate policy. They argue that the framing of the problem provided by the IAMs is invalid because of the overflow of uncertainty produced by spurious and unwarranted assumptions that are integrated into the fundamental model structures. They further maintain that the identified cascade of uncertainties they find to be connected to the economic effects of global warming means that any results of models dependent upon these arbitrarily formulated assumptions have limited plausibility, to say the least. The IAM proponents tend to disagree with (or simply disregard) these accusations. The result is a controversy over whether there actually is an overflow of uncertainty within and because of the structure of the models – and if so, how serious it is.

In this chapter, I investigate how IAM proponents within the community of climate economists have framed the problem of how to perform an analysis of the economic impacts of global warming as fundamentally pertaining to the design of a correct cost–benefit analysis of this phenomenon; and how a group of IAM-skeptical scholars deemed the solutions proposed by the IAM advocates as unsatisfactory, maintaining that the IAM advocates were unable to contain the problem in question. No conclusive agreement has been

3 IA, Integrated Assessment, is a broader definition of practices for integrative analyses that are not necessarily mathematically formalized into a model and which can also contain qualitative and deliberative components.

reached between these two groups on whether the framing produces an overflow of uncertainties. This overflow controversy can thus be conceived of as a disagreement over what constitutes a tolerable *epistemic wager* – a helpful term introduced by Dan-Cohen (2016), who leaves the concept somewhat unspecified. To give this notion some substance and traction in the context of this chapter, I define epistemic wager as an identifiable leap of faith within otherwise rationalistic discourses.

According to Franck Cochoy (2013: 175), highlighting such leaps of faith and the wagers they entail constitutes one of the taboos of modern academic scholarship. Like most scholars, economists – particularly when producing predictions – must always permit creative and speculative inference to some extent when extrapolating more general conclusions from a limited set of empirical evidence. In other words, they work under conditions of imperfect information. Sometimes, however, questions arise regarding when these inferences are stretched to the degree that they can no longer be considered valid or plausible, or – differently put – when there is 'too much of too little' for a knowledge claim to be credible.

The tolerance for various magnitudes of epistemic wagers can be assumed to vary between various epistemic communities and among different individuals within the same community – which, in this case, comprises climate economists. Thus it can be assumed that at any given point it will be possible to find a variegated set of replies to the question 'How much is too much?' when it comes to epistemic wagers between different epistemic communities or within the same community. Following this reasoning, I suggest that the controversy depicted in the remainder of the chapter can be understood as directly pertaining to that disagreement.

The investigation of scientific controversies is admittedly a strand of research with a distinct pedigree within the field of Science and Technology Studies (STS; see, e.g., Jasanoff, 2012). If there is any degree of novelty in this study, it is in bringing this established research topic together with the issue of enactment of overflows, summarized in the critical questions, 'How much is too much? Why? And according to whom?' (Löfgren and Czarniawska, 2012: 2).[4] This chapter is an attempt to explore the 'cultural dynamics of

4 Additionally, it is certainly not the first study relating questions of economics to discussions on the conceptualization of overflow, seeing that the whole theoretical debate on overflow to some extent has its roots in studies of economics. See particularly Callon (1998).

overflow dramatization', within climate-economic modeling, asking such questions as: 'Who is accusing whom of overflow, in what ways, in what social contexts and for what purposes? What types of overflow are defined as harmful, wasteful or morally bad?' (Löfgren and Czarniawska, 2013: 8).

It should be remembered that '[i]f something "flows over", it leaves one area and enters another: contaminating, enhancing, changing or perhaps having the opposite effect: reinforcing, diluting, and enriching' (Löfgren and Czarniawska, 2012: 4). In this case, the majority of critics argue that the overflow of assumptions in the modeling may be 'contaminating' climate policy by 'diluting' it or 'reinforcing' a business-as-usual (or approximate) approach. Although I do not directly address this more explicitly political aspect of the controversy in this chapter, it is nonetheless important to bear in mind that it forms a crucial backdrop to the disagreements investigated here.[5] Another important opening caveat is that this chapter focuses exclusively on debates on the topic that unfolded within the community of mutually recognizing climate economists. Beyond this narrow circle of colleagues, however, discussions concerning the validity and applicability of these methods for studying climate change have been discussed much longer. Still, it seems that such debates have had only marginal impact on the discipline of neoclassical economics, to which all of the climate economists presented in this chapter belong.[6]

The rest of the chapter is structured as follows. After a short presentation of the foundations and current state of the art, I present some of the major controversies concerning the validity of the mainstream approach to climate-economic modeling. The concluding discussion focuses on conflicting enactments of overflow and the resulting controversies. They coalesce, in one way or another, around disagreements over the issue of 'How much is too much?' in relation to some specific phenomenon or event. What is particularly salient in the controversy discussed here is not only the variegated acceptance of quite daring epistemic wagers within the community of climate economists, but also the extremely dramatic stakes that are dependent upon these wagers.

5 Regarding the latter debate more explicitly concerning the implication of climate-economic modeling with discussions about recommended policy responses, see specifically, e.g., Stern (2008, 2016); Weitzman (2009a, 2009b).

6 For reference, however, see Funtowicz and Ravetz (1994) for early critical voices emerging from outside the neoclassical community.

The basics of mainstream climate-economic modeling

Economists generally apprehend human-induced global warming as a market failure that results from an incorrect pricing of the welfare damage caused by the emission of GHGs. In this vein, leading climate economists have dubbed global warming as 'the biggest market failure the world has seen' (Stern, 2007) and 'the mother of all externalities' (Tol, 2009). The device preferred by economists for dealing with this type of problem is a so-called *Pigouvian tax* – a tax set to a level reflecting the damage that a specific externality causes to human welfare. Its purpose is thus to 'correctly price' the damage caused by some phenomenon and to force those who contribute to this negative phenomenon to carry this cost directly, instead of indirectly imposing it on society as a whole. In environmental policy, the application of Pigouvian taxes is often referred to as the 'polluter-pays' principle. As for GHGs, and particularly CO_2, many countries already apply Pigouvian taxes. Sweden, for example, has efficiently imposed a CO_2 levy since 1991, and the current tax rate for emitting a metric ton of CO_2 is roughly USD 120 (EUR 102). However, some of the largest global emitters of CO_2, such as Russia and the USA, have not yet implemented a general national carbon tax and do not at present plan to do so.[7]

Following this line of reasoning, establishing the optimal price of carbon emissions has become something of a Holy Grail of climate economics. The optimal pricing of CO_2 emissions, sometimes referred to as the social cost of carbon (SCC), is the calculated economic disutility of emitting a metric ton of CO_2 into the atmosphere, considering that this emission may have both positive and negative economic effects, which will emerge over different time scales. In simple terms, it is an attempt to place a value on the total economic damage done by an emitted metric ton of CO_2.

Establishing a robust optimal price of CO_2 emissions would, in turn, allow for the setting of a climate-economic policy (preferably, for most economists, the Pigouvian carbon-tax level) that would regulate the level of CO_2 emissions so they become Pareto-optimizing (i.e., allow for the largest possible total economic growth). Thus, this type of climate-economic work fundamentally entails a type of

7 China, the world's largest emitter of CO_2, has been considering a national carbon tax for a number of years, but it has not yet been implemented. The first-ever nation to abolish its carbon tax was Australia in 2014, which replaced it with a more limited so-called 'emissions reduction fund'.

cost–benefit analysis (CBA), trying to weight various utilities and disutilities against each other, to arrive at an optimal course of action (Weitzman, 2009a,b; Weyant, 2009: 318). Some of the overarching questions related to any CBA concern the factors assumed to affect and be affected by a specific proposed intervention (e.g., the introduction of carbon pricing), how these factors interplay, and how to estimate the dis/utility value of these factors correctly. Another key question concerns the issue of who will suffer disutilities or enjoy utilities, as it is not always evident who should be the target or stipulated consumer, for the benefit of whom the maximization is performed (the entire global human population, the inhabitants of a specific state, something or somebody else?).

As mentioned at the outset, to establish a figure for the optimal CO_2-emission price, climate economists generally utilize IAMs. IAMs function through the integration and co-calibration of various types of quantitative systems analyses of both natural and social phenomena, and their mutual interplay, into a coherent framework. Most current IAMs have their roots in energy-economic models developed in the 1970s, which were later complemented with fuel-specific CO_2 emission coefficients (Parson and Fisher-Vanden, 1997: 591, Weyant, 2009). Acid rain was the first global environmental issue to be analyzed with the help of an IAM. The Regional Air Pollution Information and Simulation (RAINS) models developed by the International Institute for Applied Systems Analysis (IIASA) in the 1980s have been credited with providing critical input into the discussions leading to international agreements to limit sulfur emissions (Parson and Fisher-Vanden, 1997: 592; Gough et al., 1998; van der Sluijs, 2002: 137).

What some authors have referred to as a 'rapid surge' of IAMs for climate change occurred from 1990 onwards (Parson and Fisher-Vanden, 1997: 592). In 2002, van der Sluijs (2002: 138) counted no fewer than 50 existing models, the structure and exact purpose of which he deemed to be 'very diverse'. Yet most of them had the goal of integrating 'knowledge that stems from a multitude of scientific disciplines in order to address interconnected aspects of one or more (global) environmental problems in an integrated fashion for the purpose of informing and supporting the policy debate' (van der Sluijs, 2002: 138). Although IAM modeling is currently an academic subfield in its own right, it also plays a significant role as the preferred methodology in most climate-economic research. Parson and Fisher-Vanden (1997: 593–594) stressed that climate-economy IAM modeling can have many purposes: to evaluate policies and responses, to structure knowledge and prioritize uncertainties, to contribute to basic knowledge of the entire climate system, and to assess broad comparative risk. Nonetheless, in one way or another, most of these models seek in some way to characterize and evaluate the social and economic impacts of climate change (Parson and Fisher-Vanden, 1997: 602).

As mentioned, the IAM models discussed in this chapter have a specific purpose: to utilize a marginalistic, Pareto-optimizing cost–benefit analysis to provide guidance on economically 'optimal' policy, to set an optimal price of CO_2 emissions. This type of climate-economic IAM was pioneered by Yale economist William Nordhaus in the beginning of the 1990s (Nordhaus, 1994). He based his climate-economy model upon his own energy-economy models from the 1970s, adding a carbon-cycle component and climate-change damage function to the overarching framework, dubbed the Dynamically Integrated Climate-Economy (DICE) model.

DICE is based on a neoclassical optimal growth model that rests upon the assumption that both climate damage and emission abatement interventions can lead to production losses. The key policy questions that the model answers concern the optimal balance between the costs of climate damage in relation to the costs of abatement activities and the marginal economic damage of an additional emitted metric ton of CO_2, under various modeled conditions. In the initial versions, DICE treated the entire world economy in an aggregated fashion, but the level of geographic differentiation of the model became increasingly detailed from the late 1990s onwards; consequently, the model was renamed the Regional Integrated Climate-Economy (RICE) model. The recommendations for a preferred-policy response to global warming based on various generations of DICE have been relatively robust. They amount to a calculated optimal path of modest initial carbon prices at early stages, later increased during the period of roughly a century, according to a 'climate-policy-ramp' approach. Throughout his work, Nordhaus has repeatedly conveyed a significant central message: setting an early carbon price too high will be extremely detrimental to global economic growth, and hence makes no economic sense at all.[8]

8 In his most recent book, however, Nordhaus (2013) goes to some lengths to argue that his central message should rather be read as pertaining to the necessity of at least taking some form of immediate action, even if it needs be carefully moderated to minimize the risk of economic damage. In December 2018, Nordhaus was awarded the Nobel Memorial Prize in Economic Sciences.

To depict in greater detail how these models are constructed and expounded by their developers, I now take a closer look at the documentation of the highly influential DICE/RICE-99 version of Nordhaus's model, as presented by Nordhaus and Boyer (2000). The purpose of their work, they claim, is to 'help modelers and policymakers better understand the complex tradeoffs involved in climate-change policy', suggesting that '[i]n the end, good analysis cannot dictate policy, but it can help policymakers thread the needle between a ruinously expensive climate-change policy that today's citizens will find intolerable and a do-nothing policy that the future will curse us for' (Nordhaus and Boyer, 2000: 7).

Before they proceeded to discuss their analysis in detail, Nordhaus and Boyer made some crucial disclaimers. For instance, they made it clear that the model does not account for all GHGs – only CO_2 – and that it is difficult to make 'solid estimates of the impacts of climate change', due to the 'enormous uncertainties about the underlying physical and biological impacts and about the potential for adaptation', which is why some of the most tricky aspects of the expected damage, such as ecosystem damage valuation and catastrophic risks, were simply omitted from the analysis (Nordhaus and Boyer, 2000: 71). The authors also emphasized that the climate module of the IAM is 'highly simplified and is intended only to depict the broad features of climate change' (Nordhaus and Boyer, 2000: 64).

The model consists of an integrated framework built with modules modeling economic growth dynamics, the carbon cycle, the climate system, and a damage function for estimated economic losses resulting from various levels of global mean temperature rise. Speaking of the temperature, Nordhaus and Boyer (2000: 23) noted that '[e]stimating the damages from greenhouse warming has proven extremely elusive', and that their choice of assumption regarding the relationship between the damage from greenhouse warming and the extent of this warming is a simple quadratic function in which increased warming results in exponentially rising damage. It is also important to note that in their modeling of the economic damage of climate change (DICE/RICE-99) they focused only on direct damage, as it was assumed that indirect damage is too difficult to appreciate and calculate. This is why they also arrived at the conclusion that '90 percent of the economy, however, is not likely to be significantly affected by climate change', thus disregarding any potential knock-on effects, disruption costs, social unrest, increased security costs, or transaction costs in reallocation of resources, and so on.[9]

Given all their disclaimers, in their final analysis Nordhaus and Boyer landed on an initially mild optimal carbon price, somewhere in the region of USD 5–10 (EUR 4.2–8.4), which was then gradually 'ramped up' to levels somewhere below USD 70 (EUR 58.5) over the course of roughly a century. The level of assertiveness with which they communicated these findings is somewhat confusing, given their statement that 'attempts to estimate the impacts of climate change continue to be highly speculative' and that '[m]uch more work is needed to improve understanding of the impacts of climate change' (Nordhaus and Boyer, 2000: 98), and their later claim that their own model, in contrast to other IAMs, 'contains a complete evaluation of the societal impacts or damages from climate change while most other models stop short of incorporating damages' (Nordhaus and Boyer, 2000: 173).

In their concluding discussion, they presented as 'some unfinished business' the fact that the utilized climate damage function was still tentative and that catastrophic climate change could not be ruled out. But that statement did not hinder them from proclaiming a few pages later that '[i]f there is a single message, it is that climate change is a complex phenomenon, unlikely to be catastrophic in the near term, but potentially highly damaging in the long run' (Nordhaus and Boyer, 2000: 178). It is exactly this self-assured assertiveness that Harvard economist Martin Weitzman (2009) used as an entry point to his criticism of Nordhaus's modeling enterprise. I will soon return to that important intervention, but to describe the context in which it unfolds I must provide a closer look at the first great public controversy within climate economy: the debate about the Stern Review.

9 Discussions of this substantial claim lie somewhat outside the scope of this chapter. Other economists, such as Sterner and Persson (2008), note, however, that a complete collapse of agriculture on earth would in this and similar models result in only a 24% decline in global GDP, given that that is the economic size of this sector. Logic dictates, however, that the follow-on effects on global economic production would be rather more dramatic, to say the least – something that is not accounted for within these models. See also van den Bergh and Botzen (2014).

Some controversies concerning macro-economic approaches to climate change

As the goal of most climate-economy IAMs is to inform policy decisions, they are inevitably contentious devices; they become particularly controversial when it is taken into consideration that various ethical choices are hardwired into the models' underlying assumptions (even if the modelers do not always recognize that to be so – see discussion in Beck and Krueger, 2016). Because of the differences in these assumptions, various IAM-based climate-economy modeling teams have arrived at the optimal CO_2 as anywhere between USD -0.3 and 146 (EUR -0.26 and 124) per emitted metric ton (van den Bergh and Botzen, 2014).[10] Most of the criticism of the variegated assumptions hardwired into the models has come not from mainstream economists, but from other social scientists, and seems to have been largely ignored by the climate economists themselves. During the recent decade, however, the scholarly community of climate economists has been rocked by two major public scientific controversies in which serious doubts regarding the validity of the currently dominant climate-economy models have been expressed: the Stern/Nordhaus controversy and the debate following Martin Weitzman's 'Dismal Theorem'.

The Stern Review is an over-700-page report commissioned by the UK government in 2005. It had a dual purpose: first, to investigate the economics of climate change; second, to provide a comprehensive depiction of the nature of the associated economic challenges, together with recommendations for ways of meeting them, both nationally and globally (Stern, 2007). Sir Nicholas Stern, then Second Permanent Secretary of HM Treasury and Professor of Economics at the London School of Economics, led the investigating team. The review was prepared by a team of economists at the Treasury and took a little more than a year to compile. Because of the routines of the British government, the final report was not subjected to scientific peer review before its release, something that other economists later criticized.

For many of its conclusions, the review relied on modeling results from the IAM PAGE2002, developed by Chris Hope and colleagues at the University of Cambridge. On the basis of these results, the Stern Review arrived at an optimal cost of CO_2 quite far above

10 It is noteworthy that any number below zero would indicate that CO_2 emissions would, when all supposedly relevant factors are considered, constitute a utility – and should, perhaps, even be subsidized rather than penalized.

that suggested by most preceding IAM models. This high estimation was largely due to the introduction of a subjectively low social discount rate (SDR) in comparison to most other models – SDR being the value attached to the welfare of future generations in relation to the present. On the basis of what Stern described as a 'moral principle', he set this rate (or, more technically, the 'pure rate of time preference') at 0.1%, which means, in effect, that the welfare of future generations will be valued as equal to the welfare of the present generation.[11] In addition to the moral argumentation for this choice, Stern presented technical arguments, suggesting that previous modeling of the optimal price of CO_2, such as those Nordhaus had depended upon, relied heavily upon historically documented expectations on short-term market returns on capital, and that using such data as revealed preferences for valuing the welfare of future generations in relation to a potentially existential threat such as global warming amounts to a category error and cannot be accepted as valid.

Stern's argumentation and design choices were harshly criticized by a number of leading climate economists (see, e.g., Mendelsohn, 2006; Tol and Yohe, 2006; Dasgupta, 2007; Nordhaus, 2007; Weitzman, 2007; Yale Symposium on the Stern Review, 2007).[12] The ensuing so-called Stern/Nordhaus controversy was based on a disagreement over what amounts to the most politically sound approach to climate policy: forceful action now, as advocated by Stern, or the postponement of harsher policy measures into the future, in accordance with Nordhaus's climate-policy ramp. But the controversy also contains a moral debate, albeit clad in technical terms, about the wellbeing of future generations. The controversy illustrates how seemingly technical choices in specific assumptions that are incorporated into the design of IAMs can have deep ethical and political implications. There is also a further dimension to this debate: the question of whether it is defensible to formulate recommendations for an optimal level of CO_2 with the help of the established toolkit of mainstream climate economics – a marginalistic CBA using IAM. This is the issue that Stern largely focused upon

11 That the pure rate of time preference (PTP) is not simply set to zero depends upon the introduction of a small correction which accounts for a risk of 1/1000 that humanity will be annihilated in any given future year.

12 As noted by Weitzman (2007), therefore, this controversy constitutes, to some degree, a re-rehearsal of the disagreement over how to set the same parameter between Cline (1992) and Nordhaus (1994).

in his reply to his critics – an issue that constituted a major part of in his 2008 Richard T. Ely lecture at the Annual Meeting of the American Economic Association.

In the opening of his presentation, Stern asserted that global warming is a special type of a particularly wicked problem in relation to economic analysis because: a) it is global in its origins and impact, b) some of its effects are extremely long-term and governed by a stock-flow process, c) there is a great deal of uncertainty in most steps of the scientific chain, and d) the effects are potentially massive and may be irreversible (Stern, 2008: 2). Stern's argument, based on these four specificities of global warming as an economic problem, led to a direct attack on the whole idea of using CBA to calculate an economically optimal path of CO_2 emissions which would then be used to guide policy decisions. He further argued that the relevance of marginalistic analysis, which forms the basis of the type of CBA that most climate economists use to calculate optimal carbon prices, is 'very limited' in relation to political decision-making concerning 'major strategic decisions for the world as a whole, with huge dynamic uncertainties and feedbacks' (Stern, 2008: 11). In such analyses, 'too much depends on assumptions about how decisions are made in a society and on how the participants [modelers] perceive the workings of the future economy' (Stern, 2008: 17). Consequently, he asserted, IAM-based quantitative analyses of the economic effects of global warming 'risk either confusing the issues or throwing out crucial features of the problem' (Stern, 2008: 17). Even worse, Stern claimed, when these models are used 'as vehicles for optimization analysis', they are 'still less credible' due to the models' sensitivity to 'simple structural assumptions', which concern moral issues and subjective judgments of risk that are characterized by a high degree of uncertainty (Stern, 2008: 17).

As an alternative approach, Stern suggested that economists should focus more on developing their thinking in the direction of 'the risk-management economics of climate change', and that they should take seriously the enormous uncertainties in all current appreciations of climate risks and the very real risk of catastrophic consequences of climate change, particularly at higher temperatures (Stern, 2008: 7).[13] Developing this latter line of argumentation, Stern gave credit

13 Curiously, in his more recent (2016) *Science* commentary, Stern appears to have all but forgotten his harsh fundamental critique of marginalistic optimization IAMs (Stern, 2008), and instead calls for 'better' IAMs which, in his view, manage to incorporate risks and costs in a more satisfactory manner.

to Harvard economist Martin Weitzman for having begun to explore such an approach to climate economics.

Weitzman was one of the most prominent of the economists who partially sided with Nordhaus in the critique of the Stern Review, and he particularly focused his criticism on what he saw to be the elitist and paternalistic stance that Stern had assumed in setting the SDR arbitrarily low. According to Weitzman, Stern's stance predetermined the outcome of the analysis in favor of strong immediate action in a somewhat dishonest way (Weitzman, 2007). In the remainder of his review, Weitzman proceeded to raise a more fundamental question: was the form of modeling that underpins the Stern Review and most other mainstream climate-economy research actually of any help in grappling with such a complex problem as global warming?

In a later paper, Weitzman (2009a) formulated what he considered to be a mathematically rigorous specification of the 'Dismal Theorem', which claims to illustrate that the marginal utility of GHG abatement is unlimited, or at least close to unlimited. The reasons for his conclusion are two-fold: a) the probability of extreme climate change is relatively high, and b) the potential economic cost of extreme climate change is practically unbounded, as it approaches the limits of human survival as a species and contains global economic collapse and the risk of death to hundreds of millions of people. In the face of such profound risks, Weitzman contended, marginalistic cost-benefit analyses (CBAs) attempting to fine-tune an optimal pricing of carbon, such as those offered by Nordhaus and other IAM modelers, are completely meaningless. What is crucial, instead, is the development of strategies against worst-case scenarios, as a form of 'insurance thinking', rather than trying to fine-tune according to 'most likely' cases and then attempting to maximize utility (i.e., economic growth) based on these cases.

The gist of Weitzman's argument, therefore, was that it is irresponsible for any economic analyst to overlook this real risk of catastrophe, even if it is currently difficult to calculate its exact probability. The problem with current optimization approaches is that they build upon existing knowledge regarding prior conditions, primarily based on historical records of the effects of relatively small variations in temperature on such factors as agriculture and human health. Weitzman notes, however, that nobody knows the degree to which the projections that can be extrapolated from such historical data will actually hold in a potential globally warmed world, given that humanity has never experienced the level of concentration of GHGs that it may have to confront. Weitzman concluded that given these 'deep structural uncertainties' that allow for potentially unlimited economic damage, one cannot focus on optimization and a supposedly 'correct' setting of the SDR. Rather, it is necessary to start calculating the potential economic costs of climate 'insurance' against worst-case scenarios, by stabilizing GHG concentrations in the atmosphere at a level that qualified experts believe would be within reasonably safe boundaries.

Nordhaus's (2009) comment on the Dismal Theorem, which in turn can be read as a direct criticism of his own work, gives some credit to the novel insights it offers to economic modeling, but downplays its relevance for the economics of climate change. According to Nordhaus, Weitzman's conclusions are unnecessarily alarmistic and overstated, and the dominant IAM-based CBA methods for analyzing optimal CO_2 pricing should be considered generally sound and reliable.[14]

Weitzman further developed his line of reasoning in a direct rejoinder to Nordhaus (Weitzman, 2009b), suggesting that Nordhaus's response was off the mark, in that it failed to grapple with the more fundamental implications of Weitzman's argument. After all, it is common knowledge that computer-driven simulations, such as climate-economy IAMs, are 'dependent upon the core assumptions of the model inside the computer'. The aim of his idea of the Dismal Theorem was to sow 'a few seeds of doubt that the "standard" CBA of climate change is fairly representing structural uncertainties, and therefore its conclusions might be more shaky than is commonly acknowledged' (Weitzman, 2009b: 2). After having reviewed all these uncertainties, and how the models – according to Weitzman – fail to account for them, he finally concluded that

> the economics of climate change consists of a very long chain of tenuous inferences fraught with big uncertainties in every link: beginning with unknown base-case GHG emissions; then compounded by big uncertainties about how available policies and policy levers will transfer into actual GHG emissions; compounded by big uncertainties about how GHG-flow emissions accumulate via the carbon cycle into GHG stock concentrations; compounded by big uncertainties about how and when GHG stock concentrations translate into global mean temperature changes; compounded by big uncertainties about

14 Notably, Nordhaus turned Weitzman's critique of 'standard' CBA analyses of climate change around, and in turn accused Weitzman of positing unwarranted, unrealistic assumptions.

> how global mean temperature changes decompose into regional climate changes; compounded by big uncertainties about how adaptations to, and mitigations of, climate-change damages are translated into utility changes at a regional level; compounded by big uncertainties about how future regional utility changes are aggregated – and then how they are discounted – to convert everything into expected-present-value global welfare changes. The result of this lengthy cascading of big uncertainties is a reduced form of truly extraordinary uncertainty about the aggregate welfare impacts of catastrophic climate change. (Weitzman, 2009b: 7–8)

Weitzman asserted that none of the uncertainties outlined in this quotation are adequately dealt with by the 'standard CBA' of climate change. According to him, this issue needs to be recognized as a serious problem. In rounding off his argument, he noted that his skepticism regarding the established methods of climate economics might come across as threateningly 'anti-scientific' and 'anti-economic' to mainstream economists, and that his conclusions may be 'frustrating for economists', given that 'we make a living from plugging rough numbers into simple models and reaching specific conclusions (more or less) on the basis of these numbers' (Weitzman, 2009b: 13). From such a broad 'economist common-sense' perspective, it may well be that '[t]he "standard" CBA appears to offer a constructive ongoing scientific-economic research program for generating ever more precise outputs from ever more precise inputs' (Weitzman, 2009b: 13). In light of this, Weitzman conceded, '[i]t is threatening for us economists to admit that constructive "can do" climate-change CBA may be up against some limitations on the ability of quantitative analysis to give robust advice' (Weitzman, 2009b: 13). Even if it is 'uncomfortable for economists', Weitzman concluded,

> if this is the way things are with the economics of climate change, then this is the way things are – and non-robustness to subjective assumptions is an inconvenient truth to be lived with rather than a fact to be denied or evaded just because it looks less scientifically objective in CBA. (Weitzman, 2009b: 13)

As a consequence of these accounted-for circumstances, Weitzman argued, economists should be more upfront with policymakers, politicians, and the public, telling them that there is a real danger of overconfidence in the objective status of climate-change CBA results, seeing how fundamentally influenced they are by incorporated subjective judgments. Therefore, economists 'should not pursue a narrow, superficially crisp, analysis by blowing away the low-probability high-impact catastrophic scenarios as if this is a necessary price we must pay for the worthy goal of giving answers and advice to policy makers', and therefore need to avoid succumbing to '[a]n artificial infatuation with crispness [that] is likely to make our analyses go seriously askew and undermine the credibility of what we say by effectively marginalizing the very possibilities that make climate change so grave in the first place' (Weitzman, 2009b: 14). Rather, economists should better explain the enormity of the unprecedented structural uncertainties, and must also, given the very real possibility of catastrophic effects of global warming upon human wellbeing, recognize that Nordhaus's climate-policy ramp constitutes an unacceptably risky wager, which amounts to 'gambling on a midcourse-correction learning mechanism […] relying more on an article of faith than on an evidence-based scientific argument' (Weitzman, 2009b: 14).

The debate between Weitzman and Nordhaus was rehearsed in a symposium published a couple of years later in the *Review of Environmental Economics and Policy* (Nordhaus, 2011; Weitzman, 2011). The third participant in this symposium was the celebrated MIT micro-economist Robert Pindyck (2011). In the following years, Pindyck came to launch what amounted to a full-frontal attack and scathing dismissal of the entire mainstream approach to the economics of climate change. In a later text (Pindyck, 2013a) he presented an argument similar in spirit, albeit not in tone, to Weitzman's – suggesting that many of the critical parameters that crucially determine the optimal prices of carbon coming out of IAMs are not only at present 'unknown' but belong within 'the realm of the unknowable' in every practical sense.

In another paper, Pindyck (2013b) asked the question that was already in his title: 'Climate change policy: what do the models tell us?' and answered it with the first two words of the abstract: 'Very little'. He went on to declare that the IAMs employed for climate-economic analysis have crucial flaws that make them 'close to useless' as tools for policy analysis, because certain inputs (e.g., the discount rate) are 'arbitrary', but have huge effects on the SCC estimates the models produce; that the models' descriptions of the impact of climate change are 'completely ad hoc, with no theoretical or empirical foundation'; and that the models 'can tell us nothing about the most important driver of the SCC, the possibility of a catastrophic climate outcome' – after which he concludes that 'IAM-based analyses of climate policy create a perception of knowledge and precision, but that perception is illusory and misleading' (Pindyck 2013b: 860).

In a later paper, he sharpened his critique even further, claiming that

> [b]ecause 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. (Pindyck, 2015: 1)

Consequently, he contended, 'calling these models "close to useless" is generous', and he challenged the entire IAM-modeling enterprise as lacking in scientific honesty, 'in that it creates a veneer of scientific legitimacy that is misleading' (Pindyck, 2015: 3). He concluded that IAM modeling must not guide policy decisions on climate change and that 'environmental economists should be ashamed to claim that IAMs can forecast climate change and its impact, or tell us what the SCC is' (Pindyck, 2015: 13).

Epistemic wagers and enactments of overflow in the economics of climate change

Franck Cochoy once suggested that '[r]eality will always overflow (or escape) the webs of reference that are supposed to account for it', which means that the data on which researchers base their representations of said reality 'are often not enough to support the results fully, yet they must present them as such', given that established research norms render any other position effectively 'taboo' (Cochoy, 2013: 275). Observing that the scientific emperor is usually only half-dressed – for example, by bringing attention to the epistemic wagers performed by fellow scientists – is, understandably, a sensitive act that is often avoided. Nevertheless, open debates regarding the acceptability of such wagers sometimes erupt within a scientific community, as the case described in this chapter illustrates.

The debates regarding the applicability of CBA and IAMs in climate-economic modeling highlight the more general question of how every conscious decision to ignore a known source of uncertainty in a modeling enterprise in itself constitutes an epistemic wager to the effect that the model will nonetheless be helpful in representing the phenomenon it aims at depicting in a productively simplified form by 'cutting through the clutter' (see also Dan-Cohen, 2016). Although all parties to these debates admit that it is necessary to posit some assumptions as a basis for any economic analysis, a number of prominent economists argue that in the case of the 'standard CBA' of mainstream climate economics, the assumptions employed are simply too uncertain and speculative to be considered scientifically sound. Relating to the use of IAMs in climate-economic modeling, one can paraphrase the earlier quotation from Martin Weitzman and conclude that it appears as if Pareto-optimizing CBA of the economic effects of climate change is built upon a number of huge epistemic wagers regarding the natural-science characteristics of global warming. These are further compounded by huge epistemic wagers regarding the social and economic effects of this warming. Taken together, they amount to a cascading enormous epistemic wager to the effect that the models can productively represent the economic costs of future climate change under various conditions; and further, that they can be relied upon to any degree whatsoever as relevant input in establishing an optimal price for CO_2 emissions.

This is a wager that economists such as Martin Weitzman and Robert Pindyck appear unable to subscribe to, as they have argued that these models are based upon excessive assumptions – especially those that uphold the relevance of historical experience of levels of climate damage for the calculation of damage incurred by potentially dramatic and historically unprecedented global temperature increases. They argue that when the enormous scientific uncertainties regarding the effects of climate change are compounded by the potentially existential stakes of global warming, it becomes scientifically irresponsible and morally reprehensible to claim to be able to calculate an optimal level of carbon pricing. Economists such as William Nordhaus, who employ 'standard' CBA of climate change, tend to disagree with the critics, often pleading for a necessary, methodologically induced 'strategic ignorance' of complexities in the modeled processes (see also McGoey, 2012; Dan-Cohen, 2016), according to a show-must-go-on logic that invites further analyses of an interesting 'microphysics of selective knowledge' (Löfgren, 2013).

From an overflow perspective, it appears as if much of the controversy regarding the usefulness of IAMs for climate policy can be understood in terms of enacted *overflows of uncertainty*. It is quite obvious that in the case of assumptions in climate-economy modeling, the inevitable question of 'How much is too much?' highlights an irreducible, albeit not always explicitly addressed, ethical or even moral dimension to each epistemic wager. In this case, the situation is perhaps particularly delicate, given that this is a wager which, if it falls out unfavorably – as Paul Krugman pointed out in the opening quotation – may actually jeopardize the future of life on Earth as we know it.

The case has a further interesting aspect: It clearly illustrates how enactments of overflow depend on simultaneous enactments of frames, such as what is considered to be a beneficial or acceptable level of something – in this case, an acceptable level of uncertainty-induced ‘strong’ assumptions, simplifications, and related epistemic wagers. The described controversies help reveal otherwise often implicit – and, obviously, conflicting – norms of what constitutes an acceptable level of speculative inference in the practice of mainstream economic scholarship. In summary, I hope that this case will invite readers to pay attention to what could be called a ‘politics of overflow’: the processes by which overflows are enacted, the potential controversies that can arise as a result of claims of overflows, and the invoked rationales and consequences claimed by conflicting parties. One can safely assume that such debates will have a deeply moral dimension, whether immediately obvious or not.