S.Haneman
https://www.edockets.state.mn.us/EFiling/edockets/searchDocuments.do?method=showPoup&documentId={EE3FA843-36EB-41E1-8412-E805FBA4CCC6}&documentTitle=20159-113909-02
pp.32-33 has interesting comments on the serious difficulties for others in using FUND software, unlike DICE, for example.
pp.44-45 He agrees with some of Martin's critiques.
RESPONSES TO: Tol, Martin, Smith (agreement with Polasky)

BEFORE THE MINNESOTA OFFICE OF ADMINISTRATIVE HEARINGS
600 North Robert Street
St. Paul, MN 55101

FOR THE MINNESOTA PUBLIC UTILITIES COMMISSION
121 Seventh Place East, Suite 350
St Paul, MN 55101-2147

IN THE MATTER OF THE FURTHER INVESTIGATION INTO ENVIRONMENTAL AND SOCIOECONOMIC COSTS UNDER MINNESOTA STATUTE 216B.2422, SUBDIVISION 3	MPUC Docket No. E999/CI-14-643 OAH Docket No. 80-2500-31888

SURREBUTTAL TESTIMONY OF DR. MICHAEL HANEMANN

ON BEHALF OF

THE DIVISION OF ENERGY RESOURCES OF
THE MINNESOTA DEPARTMENT OF COMMERCE
AND
THE MINNESOTA POLLUTION CONTROL AGENCY

SEPTEMBER 10, 2015

SURREBUTTAL TESTIMONY OF DR. MICHAEL HANEMANN
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I. INTRODUCTION AND QUALIFICATIONS

Q. Please state your name and affiliation.

A. My name is Michael Hanemann. I am a consultant providing testimony at the request of the Minnesota Department of Commerce (Department or DOC), and Minnesota Pollution Control Agency (MPCA) (together, the Agencies).

Q. Have you previously filed testimony in this case?

A. Yes. I filed Direct Testimony on June 1, 2015 and Rebuttal Testimony on August 12, 2015.

Q. What is the purpose of your Surrebuttal Testimony?

A. I respond to the Rebuttal Testimonies of the following witnesses:

- Peabody Energy Corporation (Peabody) witnesses, Professor Robert Mendelsohn and Professor Richard S.J. Tol;
- Xcel Energy Corporation (Xcel) witness Mr. Nicholas F. Martin;
- Ottertail Power (OTP) Great River Energy Corporation (GRE), Minnesota Power (MP) and the Minnesota Large Industrial Group (MLIG) witness, Dr. Anne E. Smith.
- Clean Energy Organizations (CEO) witness, Dr. Stephen Polasky

In Section II, I address the following topics raised by Peabody witness Professor Mendelsohn:

A. My expertise

B. Global emissions in the Interagency Working Group's Social Cost of Carbon (IWG SCC or SCC)

C. Statement regarding benefits to Minnesota

D. Modifications of the Integrated Assessment Models (IAMs) by the IWG

E. Averaging of the IAMs by the IWG

F. Understatement of damages in the IAMs

G. The IWG's 2013 update

H. Discounting in the IWG SCC.

In Section III, I respond to the following issues raised in the Rebuttal Testimony of Peabody witness Professor Tol:

A. My expertise

B. The availability of the FUND model

C. The IWG's use of the FUND model

D. Discounting in the IWG SCC

In Section IV, I respond to the following issues raised by the Rebuttal Testimony Mr. Martin:

A. Use of the IWG SCC for resource planning

B. Creation of a new IAM

C. Adaptation and technological change

D. Use of the median versus the mean and trimming the SCC values

E. Areas of agreement

In Section V, I respond to an issue raised in the Rebuttal Testimony of Dr. Smith:

A. Climate damages in the IWG SCC

Last, in Section VI, I identify several issues discussed in the Rebuttal Testimony of Dr. Polasky, with which I agree:

A. Uncertainty in the IWG SCC

B. Projections of future emissions

C. Measurement of damages

D. Discount rates in the IWG SCC

E. Credibility of Dr. Smith's approach

F. IWG SCC use of mean value

G. Projections of Future Emissions

H. Inclusion of PAGE, FUND, and DICE IAMs in the IWG SCC

II. RESPONSE TO PROFESSOR MENDELSOHN

A. MY EXPERTISE

Q. Professor Mendelsohn stated:

> **It appears that Professor Michael Hanemann and Professor Stephen Polasky have been asked to give opinions outside their areas of expertise. Neither appears to be very familiar with Integrated Assessment Models ("IAMs") and the calculation of the social cost of carbon.[1]**

What is your opinion on Professor Mendelsohn's position?

A. I disagree. I am very familiar with the literature on IAMs, the damages from climate change and the social cost of carbon. I first became aware of these issues around 1978 when I came across the multi-volume report on "Economic and Social

[1] Peabody Ex.___ at RM-1, page 2 (Mendelsohn Rebuttal).

Measures of Biologic and Climate Change" edited by Professor Ralph D'Arge.[2] I was subsequently a participant in perhaps the first conference on the economics of climate change in the United States, organized in 1980 by Professor Kerry Smith with funding from NOAA.

Q. Can you restate your qualifications with regard to these topics?

A. Yes. During the 1980s I published papers on the economics of irreversible environmental change, of which global warming is the ultimate example. Because of this research, my colleague Professor Tony Fisher and I received funding to work on economic issues related to climate change impacts. We were also invited to participate in a research project organized by the National Institute for Global Environmental Change at the University of California, Davis, which led to a number of publications in 1993 on the economic impacts of climate change on fisheries, water supply and hydropower production in California. We also had been asked to contribute a paper on the valuation of climate impacts for a conference in Washington DC on the economics of global warming, subsequently published as Fisher and Hanemann (1993).[3] We followed this up with a 1994 working paper on "What's Wrong with Current Estimates of Climate Change Damages." This was a critique of the damage function used by Professor Nordhaus in the first version of the DICE model (Nordhaus, 1992).

In 1994, I was invited to the Stanford EMF Snowmass Conference to discuss damages from climate change. At that meeting, I heard Professor Nordhaus present

[2] Panel on Economic and Social Measures of Biologic and Climatic Change, Climate Impact Assessment Program Monograph 6, prepared for the US Department of Transportation, Washington, DC 1975.
[3] Anthony C. Fisher and W. Michael Hanemann, "Assessing Climate Change Risks: Valuation of Effects." In *Assessing Surprises and Nonlinearities in Greenhouse Warming* edited by Joel Darmstadter and Michael A. Toman, Washington, DC: Resources for the Future, 1993.

results on the economic impact of climate change on US agriculture, subsequently published in Mendelsohn, Nordhaus and Shaw (1994). The message was that the economic harm to US agriculture would be quite small. My initial skepticism and subsequent analysis and research into this topic led eventually to Schlenker, Hanemann and Fisher (2005, 2006) and Fisher, Hanemann, Roberts and Schlenker (2012).[4]

Starting in 2003, I became very actively engaged with the broader set of economic issues associated with climate change when I was asked by the California Energy Commission to establish and direct the California Climate Change Center at UC Berkeley which focused on the economic and policy issues associated with mitigation, damage assessment and adaptation in California. I have continued to engage with those issues ever since.

Since approximately 2010, I have been serving as an advisor to the economics group in the European Union's Joint Research Center which conducts the EU's economic assessment of the impacts of climate change on EU member countries.

From 2011 to 2014, I was a lead author for Chapter 3 of Working Group III for the Fifth IPCC Assessment Report (AR5), "Social, Economic and Ethical Concepts and Methods." I took the lead in drafting the section of that chapter dealing with IAMs, damages and the social cost of carbon.

As a member of the National Academy of Sciences, I was invited to present that chapter's findings at a joint meeting of the US and Mexican National Academies

[4] W. Schlenker, M. Hanemann and A. Fisher (2005) "Will U.S. Agriculture Really Benefit from Global Warming? Accounting for Irrigation in the Hedonic Approach," *American Economic Review*, 395-406; W. Schlenker, M. Hanemann and A. Fisher (2006) "The Impact of Global Warming on U.S. Agriculture: An Econometric Analysis of Optimal Growing Conditions," *Review of Economics and Statistics* 88(1) 113-125; A. Fisher, M. Hanemann, M. Roberts and W. Schlenker (2012) "The Economic Impacts of Climate Change: Evidence from Agricultural Output and Random Fluctuations in Weather: Comment," *American Economic Review* 102(7) 3749-3760.

of Sciences on climate change in Mexico City in April, 2014. For the National Academy's Board on Environmental Change and Society, I co-organized a Workshop on Integrating Socio-Economic Factors with Abrupt Change and Extreme Events in Climate Models in Washington in June 2014.

In an Information Request from Peabody dated July 15, 2015, I was asked what literature on these topics I had reviewed (Question 18). In my response, dated July 27, 2015, I provided a listing of over 1,200 items on IAMs, IAM damage functions, and the social cost of carbon, which I have reviewed over the past decade or so. This is a subset of a larger literature on specific topics (e.g., the impact of climate change on agriculture, the impact of climate change on energy demand and supply, the impact of climate change on water, etc.) which I have reviewed over this period. I believe that, contrary to Professor Mendelsohn's characterization of my expertise, I am more than qualified to testify to this information.

B. GLOBAL EMISSIONS IN IWG SCC

Q. Professor Mendelsohn stated: "Professor Hanemann and Professor Polasky appear to be unaware that the IWG is measuring the SCC assuming that the rest of the world will never do any mitigation."[5] Do you agree with this statement?

A. I disagree with the statement for two reasons: 1) I was well aware of the IWG's assumption with regard to mitigation, and 2) it misrepresents what the IWG actually did assume.

The IWG's emission scenarios all assume that greenhouse gas (GHG) emissions are reduced eventually (see Figure 1B on page 19 of my Rebuttal

[5] Peabody Ex.___ at RM-1, page 3 (Mendelsohn Rebuttal).

Testimony), and one of the five emissions scenarios assumes that emissions are reduced sufficiently to ensure that the atmospheric concentration of CO_2 is stabilized at 550 ppm by the end of this century. Thus, it is incorrect to assert that the IWG assumes the rest of the world will never do any mitigation (*i.e.*, never reduce GHG emissions).

Q. Professor Mendelsohn continued as follows: "They [Hanemann and Polasky] appear not to realize the IWG values assume that not only is Minnesota the first place to undergo mitigation, but it is the only place to ever do mitigation. They are not troubled that the cost of global mitigation is borne by Minnesota alone in this analysis."[6] Do you agree with this statement?

A. No. This statement misrepresents what the IWG assumed.

The IWG's value of the SCC assumes neither that Minnesota is the first place to undergo mitigation nor that it is the only place ever to do mitigation. The IWG's estimate was developed, after all, to value mitigation by federal agencies and mitigation resulting from federal regulations.

The IWG's SCC estimate measures the value of the damage from an incremental unit of CO_2 emissions added to the emission profiles shown in Figure 1B of my Rebuttal Testimony, or the benefit from a unit of emissions subtracted from those emission profiles, regardless of where in the world the addition (or subtraction) of emissions occurs.

[6] Peabody Ex.___ at RM-1, page 3 (Mendelsohn Rebuttal).

C. STATEMENT REGARDING BENEFITS TO MINNESOTA

Q. Professor Mendelsohn stated:

> **There is every reason to believe that Minnesota will be a beneficiary of warming over the next century from the increased productivity of their ecosystems, from the increase in crop production, and from reductions in heating costs in the winter. These will far outweigh any likely damage in the state during this period.[7]**

Do you agree with that statement?

A. I am not in a position to offer my own personal assessment of the likely impacts of climate change in Minnesota because I have not studied the impacts of climate in Minnesota in the same way as I have studied the impacts in California.

However, I find nothing in the testimony to show that Professor Mendelsohn has personally studied the impacts of climate change in Minnesota either. He does not appear, therefore, to be in a position to offer the assessment which he gave. Professor Mendelsohn's statement is speculative at best.

Q. Are there any published scientifically based opinions on likely impacts of climate change to Minnesota?

A. Not to Minnesota specifically, but the recent US National Climate Assessment characterized the likely impacts of climate in the Midwest region, including Minnesota, as follows:[8]

1. In the next few decades, longer growing seasons and rising carbon dioxide levels will increase yields of some crops, though those benefits will be progressively offset by extreme weather events. Though

[7] Peabody Ex.___ at RM-1, page 3 (Mendelsohn Rebuttal).
[8] Melillo, Jerry M., Terese Richmond and Gary W. Yohe (eds.) 2014. *Climate Change Impacts in the United States: The Third National Climate Assessment*, page 419.

adaptation options can reduce some of the detrimental effects, in the long term, the combined stresses associated with climate change are expected to decrease agricultural productivity.

2. The composition of the region's forests is expected to change as rising temperatures drive habitats for many tree species northward. The role of the region's forests as a net absorber of carbon is at risk from disruptions to forest ecosystems, in part due to climate change.
3. Increased heat wave intensity and frequency, increased humidity, degraded air quality, and reduced water quality will increase public health risks.
4. The Midwest has a highly energy-intensive economy with per capita emissions of greenhouse gases more than 20% higher than the national average.
5. Extreme rainfall events and flooding have increased during the last century, and these trends are expected to continue, causing erosion, declining water quality, and negative impacts on transportation, agriculture, human health, and infrastructure.
6. Climate change will exacerbate a range of risks to the Great Lakes, including changes in the range and distribution of certain fish species, increased invasive species and harmful blooms of algae, and declining beach health. Ice cover declines will lengthen the commercial navigation season.

To summarize, I do not understand the basis for Professor Mendelsohn's opinion. And I do not see how the US National Climate Assessment's

characterization of impacts leads to the conclusion that benefits in Minnesota "will far outweigh any likely damage," as Professor Mendelsohn asserted.

D. MODIFICATION OF THE IAMS BY THE IWG

Q. Professor Mendelsohn stated:

> **The IWG did not cite any results that actually come from the DICE, FUND, or PAGE models. All the results mentioned in the IWG are from a hybrid model that uses different assumptions from different places.[9]**

Do you agree with this statement?

A. Yes, I agree with the statement but, I think it is irrelevant.

Professor Mendelsohn's statement alluded to the fact that, when running the three IAMs side-by-side, the IWG standardized the model inputs – the projections of future population, income and emissions – so that they had a common set of input data. As I explained in my Direct Testimony, this was done in order to put the three models on a common footing. As stated in DOC Ex.___ at 15-17 (Hanemann Direct), "This made them more comparable. Standardizing the external model inputs is the conventional practice in model inter-comparison exercises." In my opinion, it would have been unreasonable if the IWG had *not* done this.

Q. Continuing with the same theme, Professor Mendelsohn stated: "They [Hanemann and Polasky] appear to be unaware that one of the primary values of IAM models is that they carefully integrate economic assumptions across the economy. At least the DICE and FUND model are internally consistent. The IWG exercise violates the

[9] Peabody Ex.___ at RM-1, page 4 (Mendelsohn Rebuttal).

carefully constructed assumptions of these IAM models with IWG assumptions."[10] Do you agree with this statement?

A. No. I am very well aware that each model, individually, consistently integrates a set of assumptions across the economy. Professor Mendelsohn in fact appears to be unaware that what the IWG was doing was in the nature of a *model inter-comparison exercise*, whereby several models are run side-by-side with common inputs and their results are compared. As just noted, standardizing the inputs is the conventional practice in such an exercise.

Q. Professor Mendelsohn stated: "One cannot make different assumptions about income and population without changing the interest rate in DICE."[11] Do you agree with this statement?

A. No. As explained in depth in DOC Ex.___ at 47 (Hanemann Direct), the IWG ran DICE in a simulation mode, not an optimization mode. When a model like DICE is run in a simulation mode, one certainly can make different assumptions about income and population without changing the interest rate. Please refer back to my previously cited Direct Testimony on this matter for additional information that I have already provided.

[10] Peabody Ex.___ at RM-1, page 4 (Mendelsohn Rebuttal).
[11] Peabody Ex.___ at RM-1, page 5 (Mendelsohn Rebuttal).

Q. Professor Mendelsohn stated: "DICE is very carefully calibrated to predict emissions depending on GDP and an observed decay rate in emission per unit of GDP. These assumptions are overridden in the IWG analysis."[12] He intends this as a criticism of what was done by the IWG. Do you agree with that criticism?

A. No. Professor Mendelsohn appeared to be arguing that it would have been better for the IWG to adopt the prediction of future GHG emissions generated by the optimization version of DICE than the method the IWG chose to pursue – use emission projections from EMF-22 extended out to 2300. I have seen no evidence – and Professor Mendelsohn offered no evidence – that DICE has a good track record at projecting GHG emissions. I have seen no evidence, for example, that the DICE 1999's projection of GHG emissions in 2010 or 2020 are particularly on target, or are superior to projections developed by the EMF-22 exercise. Thus, I do not find Professor Mendelsohn's criticism to be particularly salient.

Q. Professor Mendelsohn stated: "The IWG results are based on long term assumptions that have not been evaluated. It is simply not correct to argue that the IWG results depend on three well reviewed models."[13] Do you agree with this statement?

A. No. In my Direct Testimony, I stated clearly that, when using DICE, PAGE and FUND, the IWG changed some of the model inputs in order to place them on a common footing – see DOC Ex.___ at 48-49 (Hanemann Direct). The IWG results depend on both the IAMs and the inputs fed to those models.

[12] Peabody Ex.___ at RM-1, page 5 (Mendelsohn Rebuttal).
[13] Peabody Ex.___ at RM-1, page 5 (Mendelsohn Rebuttal).

Q. Professor Mendelsohn stated: "Professor Hanemann and Polasky do not appear to be aware that the social cost of carbon estimates that would come from the DICE and FUND models are not consistent with the estimates of the IWG."[14] Is that a fair statement?

A. No. As already noted, I am fully aware that the IWG made changes to DICE and FUND, and that those changes affect the model outputs. Once those changes are taken into account, however, the IWG estimates of the SCC are consistent with the DICE and FUND models.

E. AVERAGING OF THE IAMS BY THE IWG

Q. Professor Mendelsohn stated:

> **Both Professor Hanemann and Professor Polasky believe it is appropriate that the IWG averaged the results across the DICE, FUND and PAGE models. That implicitly means they feel all three models are equally valid. They treat the single equation damage function of DICE, the uncalibrated probabilistic damage function of PAGE, and the carefully calibrated sector-specific regional damages of FUND as equally valid. No justification is given for this treatment.[15]**

This appears to be intended as a criticism of your Direct Testimony. Do you accept this criticism?

A. No. This was addressed in DOC Ex.___ at 73 (Hanemann Direct):

> Averaging implies weighting those models equally. This was a policy decision made by the IWG. I am not aware of any suggestions in the existing economic literature on this topic that would provide a basis for doing something different.

[14] Peabody Ex.___ at RM-1, page 5 (Mendelsohn Rebuttal).
[15] Peabody Ex.___ at RM-1, page 7 (Mendelsohn Rebuttal).

Q. Citing your Direct Testimony, Professor Mendelsohn stated:

> **Professor Hanemann has estimated a damage function for farmland in the United States. It is a quadratic function based on temperature over the growing season in the United States. It is not consistent with the damage function in the IAM models based on the change in global temperature since preindustrial times.[16]**

Do you agree with this statement?

A. There does not appear to be a conclusion or point made by the statement to which I can agree or disagree.

I have estimated functions that measure the value of farmland in the United States as a function of temperature, using a quadratic formulation, but I did not cite any of those papers in my Direct Testimony. I am not sure, therefore, why Professor Mendelsohn mentioned them in connection with my Direct Testimony. The algebraic formulas used in my papers are different from the IAM damage function formulas, which are measuring something different from the functions that I used in those past papers. I was measuring the level of farmland value in a US county as a function of the level of temperature during the growing season in the county where the farmland is located. The IAM functions used by the IWG measure *the change* in overall GDP in an entire region (in the entire world, in the case of DICE) as a function of *the change* in regional (or global) average annual temperature compared to the pre-industrial period. I have no idea why Professor Mendelsohn might expect the two functions to be similar, when they measure different things.

[16] Peabody Ex.___ at RM-1, page 7 (Mendelsohn Rebuttal).

Q. Given that they are measuring different things, does your farmland value estimation carry any lessons for the IAM damage functions?

A. Yes. My farmland value analysis serves to illustrate a general point that applies to the IAM damage functions. It demonstrates that the coarse temporal aggregation in the IAMs – which focus on changes in *annual* temperature – leads them to overlook harmful temperature events that play out on finer time scales, such as a period of days, thereby understating the damages from warming.

In Schlenker *et al*., (2006) we conducted an econometric analysis of the impact of climate on farmland value in the US east of the 100th meridian. We used three climate variables: precipitation during the growing season; temperature within the growing season measured as degree days between 8°C and 32°C (the agronomically recommended range for growing crops); and extreme temperature measured as degree days over 34°C during the growing season. Extreme temperatures occur for only a small fraction of the time – often, only a few days – but they proved to have a powerful effect. When we predicted how farmland value would be affected under climate change projections, the vast majority of the impact was accounted for by the increase in the occurrence of extreme temperatures. These events hardly show up in annual average temperature, the variable that drives the IAM damage functions. Thus, focusing on annual temperature could understate damages in sectors vulnerable to such extreme but short-lived events by as much as an order of magnitude.[17]

[17] I had alluded to this possibility in DOC Ex.___ at 53 (Hanemann Rebuttal).

F. MEASUREMENT OF DAMAGES IN THE IAMS

Q. Professor Mendelsohn stated that, while Professor Polasky has argued that the IAM damage functions underestimate damages, he failed to support that claim.[18] Have you offered evidence in support of the argument made by Professor Polasky?

A. Yes. As part of his Rebuttal Testimony, Professor Mendelsohn failed to cite research that the aggregate damage estimates on the IAMs are too low. I hold the same opinion on this matter as Professor Polasky, and I provided the reasons for my opinion in DOC Ex.___ at 47-63 (Hanemann Rebuttal). Those reasons are summarized in Table 1 below.

[18] Peabody Ex.__ at RM-1, page 6 (Mendelsohn Rebuttal).

Table 1: Factors Leading IAMS to Understate Damages

- The IAMs draw on an out-of-date literature on the impacts of climate change. The model developers have largely failed to keep up with the newer literature, which generally indicates more severe damages than the earlier literature.
    - The newer literature is more granular in spatial scale and, sometimes, also in temporal scale.
- Given the convexity of climate damages, the high levels of spatial and temporal aggregation in the IAMs leads to lower damage estimates than would be obtained with a more disaggregated analysis.
- The IAMs do not account for changes in precipitation that could have impacts on wellbeing.
- The existing damage functions do not well account for certain non-market impacts of climate change, including ecological and social impacts.
- The mathematical formulation used in the IAMs represents the degree of warming in a given time period as affecting GDP in that period, but not GDP in future periods.
    - There is empirical evidence of impacts on the rate of growth of GDP.
    - This impact could come about through damage to stocks of capital – physical capital, human capital, and natural capital.
    - Destruction of capital due to climate events would have a lingering impact on future economic and social wellbeing.
- The mathematical formulation used in the IAMs assumes that more GDP is a perfect substitute for what is damaged by climate change. If GDP is a less than perfect substitute – having more market goods does not fully offset the loss of the amenities of nature -- that has been shown to raise the value of the damage.[19]
- The IAMs overlook tipping points and climate risks.
- The IAMs do not account properly for risk aversion on the part of people exposed to the various climate impacts.

[19] Thomas Sterner and U. Martin Persson (2008), "An Even Sterner Review: Introducing Relative Prices into the Discounting Debate," *Review of Environmental Economics and Policy* 2(1), 61-76.

G. THE IWG'S 2013 UPDATE

Q. Professor Mendelsohn stated:

> **Professor Hanemann and Professor Polasky acknowledge that the IWG estimates of the SCC changed dramatically between 2010 and 2013. However, neither seemed particularly concerned about the magnitude of the change. They simply accepted the fact that it was updated.[20]**

Do you accept this as a valid criticism?

A. No.

Whether or not one should be concerned about the change depends on the reason for the change and the nature of the change. If the increase in the SCC value was caused by a change in the procedure adopted by the IWG, then that might be grounds for concern. In fact, as I noted in DOC Ex.___ at 59 (Hanemann Direct), there was *no* change in the procedure adopted by the IWG. The difference between the 2010 and 2013 SCC values was due to updates by the IAM modelers. The model changes were exhaustively detailed by the Electric Power Research Institute in its 2014 report and summarized in DOC Ex.___ at 57-58 (Hanemann Direct). Almost all of the model changes were revisions to the climate modeling, and those were motivated by new information presented in the IPCC's Fourth Assessment Report.

Given that better scientific information was available and was used for the 2013 IWG exercise, I do not see why one would *not* take it into account, let alone take issue with the change as a reason for concern.

[20] Peabody Ex.___ at RM-1, page 8 (Mendelsohn Rebuttal).

Q. Professor Mendelsohn stated:

> **The justification for this large shift [in SCC value] would have to be a major scientific advance. However, what we learn from the IWG is the justification for the change is that the authors of the DICE, FUND, and PAGE models made some minor adjustments in their models.[21]**

Do you agree with this assessment of what happened?

A. No.

There *was* a scientific advance, namely the Fourth IPCC Assessment Report (AR4) which offered a significantly more gloomy assessment of climate change than its predecessor Third Assessment Report. The Fourth Assessment stated, for the first time, "warming of the climate system is unequivocal." An illustration of the difference in tone is the degree of warming reported. In 2001, the Third Assessment reported a warming of about 0.6°C [0.4 to 0.8] over the 20th century. In 2007, the Fourth Assessment reported a warming of 0.74°C [0.56 to 0.92] over 1906-2005.[22]

Furthermore, the characterization of the changes in DICE, FUND and PAGE as "minor" is Professor Mendelsohn's own invention. Neither the IWG 2013 report nor the 2014 EPRI report on what the IWG did in 2013 versus 2010 characterizes the IAM model changes as "minor."

H. DISCOUNTING IN THE IWG SCC

Q: Professor Mendelsohn stated: "The Professors' support for low interest rates appears to be more advocacy than expert advice."[23] Do you agree with this statement?

A. No. To the extent that he is accusing me of offering advocacy rather than expert advice, I reject that accusation. In DOC Ex.___ at 68-69 (Hanemann Direct) I stated:

[21] Peabody Ex.___ at RM-1, page 8 (Mendelsohn Rebuttal).
[22] In 2013, the Fifth Assessment Report reported a warming of 0.85°C [0.65 to 1.06] over 1880-2012.
[23] Peabody Ex.___ at RM-1, page 6 (Mendelsohn Rebuttal).

> The three values chosen by the IWG and the 3% value chosen by the IWG for the central estimate are policy judgments by the IWG. In my opinion, it was appropriate for the IWG to use these numerical values. They are consistent with the values used in the existing literature on the economics of climate change and of GHG mitigation. Another major study, the Stern (2006) Review, conducted for the UK Government, used a discount rate of 1.4%. For DICE, Nordhaus uses 5.5%.

This is expert testimony, not advocacy. In DOC Ex.___ at 71 (Hanemann Rebuttal), I stated that there is a well-developed economic theory of the discount rate, and I went on to explain that theory in some detail as the basis for my own reasoning in DOC Ex.___ at 72-85 (Hanemann Rebuttal).

Q. Professor Mendelsohn stated:

> **Arguments have been made by economists why discount rates may fall in the far future. They are based on a slowing of the growth of income. For example, the DICE model assumes that interest rates will fall as per capita income falls. Although interest rates are 5% today in DICE, they fall to closer to 3.5% by 2100. Professor Hanemann and Professor Polasky appear not to be aware that DICE itself has a falling interest rate tied to a slowing of economic growth over time. This justifies a discount rate that falls over time but it does not justify a low fixed rate.[24]**

Are you, in fact, unaware of the connection between the rate of growth of income and the discount rate?

A. No. I am well aware of this, and I discussed it in my Rebuttal Testimony.

What is being considered here is known as the *consumption rate of discount*: this is the discount factor used to calculate the discounted present value of future damages resulting from a unit increase in current GHG emissions. I will denote this

[24] Peabody Ex.___ at RM-1, page 6 (Mendelsohn Rebuttal).

by ρ. As I explained in DOC Ex.___ at 72-75 (Hanemann Rebuttal), the consumption rate of discount is the sum of two factors:

(a) The utility *rate of discount*, also known as the pure rate of time preference, which I denote by δ. This is the rate at which a decision maker is willing to trade off wellbeing in the future for wellbeing now. In the climate context, what is being traded off is the wellbeing of future generations for the wellbeing of the present generation.

If δ = 0, a unit of wellbeing for a future generation is viewed as being equally important as a unit of wellbeing for the present generation. If δ > 0, a unit of wellbeing for a future generation is viewed as less valuable than a unit of wellbeing for the present generation. The larger the value of δ, the less valuable the wellbeing of a future generation relative to that of the present generation. If δ < 0, a unit of wellbeing for a future generation is viewed as more valuable than a unit of wellbeing for the present generation.

Whatever value is chosen for δ, this is inherently an ethical judgment.

(b) The *marginal utility factor* is a correction factor that accounts for the (presumed) fact that the marginal utility of income declines when one is richer: an extra dollar is worth less to a rich person than a poor person.

The discount rate applied to future damages is the sum of these two factors:

$$\rho = \delta + \text{marginal utility factor} \qquad (1)$$

In the standard Ramsey model, the marginal utility factor is the product of two terms: how much richer the future generation will be (represented by g) and how

rapidly the marginal utility of income declines when one is richer (represented by η). Thus,

$$\text{Marginal utility factor} = g \cdot \eta \qquad (2)$$

The larger the value of η, the faster marginal utility decreases as income rises.

The statement made by Professor Mendelsohn refers to this marginal utility factor. He appeared to be saying that I am unaware that the growth in income (represented by *g*) affects the discount rate, ρ. Suffice it to say that I am well aware of this.

Q. Professor Mendelsohn stated: "Professor Hanemann and Professor Polasky suggest there is an ethical reason to adopt low interest rates."[25] Do you agree with this statement?

A. No. It conflates two distinct ideas:

(I) As indicated in equation (1), the interest rate (ρ) depends partly on δ, how future generations' wellbeing is valued relative to that of the present generation, which is inherently an ethical judgment.

(II) The interest rate also depends on the marginal utility factor, and there are sound economic reasons why the formula in equation (2) could be modified downwards.

Professor Mendelsohn argued for a high value of δ, but he simultaneously asserted that it would be "advocacy" and not "expert advice" to argue for a low value of δ. He also overstated the importance of the marginal utility factor.

[25] Peabody Ex.___ at RM-1, page 7 (Mendelsohn Rebuttal).

As it happens, Frank Ramsey, who invented Ramsey discounting, held that one should set the value of δ at zero. To set a positive value of δ, he argued, "is ethically indefensible" (Ramsey, 1928, p. 543). Pigou, who invented the concept of externality, also held that one should set the value of δ at zero.[26]

I am curious as to whether Professor Mendelsohn would consider Pigou and Ramsey to have been offering advocacy rather than expert advice.

With regard to the marginal utility factor, given by the formula in equation (2), in DOC Ex.___ at 76-79, I noted that this mathematical formula relies on two specific assumptions that I regard as implausible and that have been questioned in the economics literature. If either of those assumptions is rejected, the formula in equation (2) must be modified downwards.

The first assumption is that the society's preferences and expectations regarding what one should get out of life remain unchanged over the span of time being considered – in this case, 300 years.[27] If that assumption is incorrect, it *reduce*s the marginal utility factor. If what a person expects out of life changes over time, this undercuts the notion that money necessarily means less to him when he is richer. This was first corrected by Ryder and Heal in a 1973 journal article cited in my Rebuttal Testimony (page 77).

The second assumption is that society cares about just one thing – GDP – and all the impacts of climate change can be reduced to the equivalent of a reduction in GDP. This is a very strong assumption. It implies that increased production of market goods is a perfect substitute that can offset any harm from climate change, including harm to human life and health and damage to natural ecosystem.

[26] A. C. Pigou (1920) *The Economics of Welfare*, London, Macmillan, p. 25.
[27] In this application of Ramsey discounting, the decision maker is taken as a surrogate for society as a whole.

Q. Has this second assumption been addressed by the IWG?

A. Yes. The 2010 IWG Report called this assumption into question:

> In the context of climate change, however, it is possible that the damages to natural systems could become so great that no increase in consumption of non-climate goods would provide complete compensation. For instance, as water supplies become scarcer or ecosystems become more fragile and less bio-diverse, the services they provide may become increasingly more costly to replace. (IWG, 2010, p. 33)

If people care separately both for items that money can buy and also for other, non-market items, and if they do not see those two types of items as perfect substitutes for one another, this changes the formula in equation (2) for the marginal utility factor. This implication was first noted, in a different context, by Malinvaud (1953).[28] In an environmental context it was first noted by Fisher and Krutilla (1975) and was developed further by Gerlach and van der Zwann (2002).[29] Specifically, if people care for unimpaired natural environment, but this is increasingly diminished with economic growth and with climate change, the mathematical implication is to *reduce* the value of the marginal utility factor and to lower the discount rate.

There are additional economic reasons why the standard Ramsey formula might not apply. These result from changes to the assumptions in Ramsey's original 1928 model.

One recent development involves hyperbolic discounting, which is discussed in DOC Ex.___ at 79-83 (Hanemann Rebuttal). Hyperbolic discounting occurs when the rate used to discount from one period to the next declines as the two periods

[28] E. Malinvaud (1953) "Capital Accumulation and the Efficient Allocation of Resources," *Econometrica* 21(2), 233-268.

[29] A. Fisher and J. Krutilla (1975), "Resource Conservation, Environmental Preservation and the Rate of Discount," *Quarterly Journal of Economics* 89(3), 358-370. R. Gerlagh and R. van der Zwaan (2002), "Long-term Substitutability Between the Environment and Manufactured Goods." *Journal of Environmental Economics and Management* 44, 329-345.

being considered lie farther out in the future. There is empirical evidence that people often see things this way when making real decisions. Given the long span of time involved in the computation of the SCC, any form of hyperbolic discounting would significantly reduce the value of the discount rate.

Another consideration is uncertainty and risk aversion. Many uncertainties can arise in the context of long-run decision making on climate mitigation and damage reduction. There is some uncertainty about long-run growth – one doesn't know just how rich future generations will be. There is some uncertainty about how fast the planet will warm, and how damaging this will be. Such uncertainties can be a cause for risk aversion -- a concept I explained in DOC Ex.___ at 60-62 (Hanemann Rebuttal). If an allowance is made for risk aversion, it has been shown that the effect is to change the formula in equation (2) and lower the effective discount rate.[30]

In short, besides ethical considerations regarding how one weighs the wellbeing of future generations impacted by climate change (i.e., δ), there is an economic rationale for a lower value of the marginal utility factor in (1) that would have the effect of lowering ρ. This argument is summarized in Table 2.

[30] What is involved here is a re-formulation of the Ramsey model based on the work of L. G. Epstein and S.E. Zin (1989) "Substitution, Risk Aversion and the Temporal Behavior of Consumption and Asset Returns," *Econometrica*, 57(4), 937-969. For the application to the DICE model, see B. Crost and C.P. Traeger (2014) "Optimal CO_2 Mitigation under Damage Risk Valuation," *Nature Climate Change,* Vol. 4, 631-636, and S. Jensen and C. P. Traeger, (2014) "Optimal Climate Change Mitigation under Long-Term Growth Uncertainty: Stochastic Integrated assessment and Analytic Findings," *European Economic Review,* Vol. 69, 104-125.

Table 2: Factors Leading to a Lower Interest Rate for Discounting Future Climate Damages

1. Placing more weight on the well-being of future generations (lower δ)
2. What people expect out of life changes over time. As they get richer, money means somewhat less to them – but not as much less as if their expectations had remained unchanged.
3. Society does not just care about GDP. All the impacts of climate change cannot be reduced to the equivalent of a reduction in the production of market goods. More GDP is not a perfect substitute for the damage caused by climate change.
4. A lower rate is employed to discount outcomes occurring in the distant future than near term.
5. Given the uncertainties involved with future climate impacts over a long span of time, some degree of risk aversion is appropriate when assessing those impacts.

Q. Professor Mendelsohn stated:

> **By lowering the discount rate, the professors are shifting the burden of paying for climate change away from these future wealthier generations and putting the cost instead on the present generation. It is not clear why the present relatively poor generation should have to bear more than their fair share of the cost of this intergenerational policy. It is not at all clear why a low discount rate is 'ethical'.[31]**

Do you agree with this argument?

A. Not at all. Unless I am mistaken, Professor Mendelsohn seemed to be offering his own ethical argument here.

The 2015 value of the SCC measures the discounted present value of the future damages generated by a unit increase in emissions in 2015. Similarly, the 2030 value of the SCC measures the discounted present value of the future

[31] –Peabody Ex.___ at RM-1, page 8 (Mendelsohn Rebuttal).

damages generated by a unit increase in emissions in 2030, and the 2050 value of the SCC measures the discounted present value of the future damages generated by a unit increase in emissions in 2050. Conceptually, when using an SCC value one is applying the "polluter pays" principle to each generation of polluters. In my view, that is fair.

III. RESPONSE TO PROFESSOR TOL

A. *MY EXPERTISE*

Q. Professor Tol stated: "It appears to me as though the parties retaining Dr. W. Michael Hanemann have requested him to provide testimony outside his area of prior experience and expertise."[32] Do you agree?

A. No. See my Surrebuttal of Mendelsohn at page 3.

Q. Professor Tol stated:

> **Dr. Hanemann claims that the first estimate of the impact of climate change was published in 1992. (Hanemann Direct at 30: 15-16.) But it was in 1979 by Dr. Ralph C. D'Arge, while the first estimate of the social cost of carbon was published in 1982 by Dr. William D. Nordhaus.[33]**

Do you agree with these statements?

A. No. What Professor D'Arge published was a damage function for changes in global temperature, not an estimate of the discounted present value of damages from an additional unit of CO_2 emissions. What Professor Nordhaus presented in his 1980 working paper and his 1982 journal article was an estimate of the social cost of

[32] Peabody Ex.__ at RM-1, page 3 (Tol Rebuttal).
[33] Peabody Ex.__ at RT-2, page 3 (Tol Rebuttal).

carbon, but it was not based on the optimal solution to the economic growth model that he formulated in those papers: it was based on an analysis of some alternative non-optimal solutions. In a 1990 working paper and a 1991 journal article, he published an analysis based on the optimal *steady state* solution. His first paper presenting an estimate of the social cost of carbon based on a full optimization of his growth model, with an optimal *transition path* appeared in *Science* in 1992.

Q. Professor Tol stated: "Dr. Hanemann's Figure 1 (Hanemann Direct at 25:1-2) is accurate for PAGE but not for DICE and FUND. In DICE, the impacts of climate change (7) affect economic growth (1)."[34] Do you accept that criticism?

A. No. The diagram was intended to provide a simplified and generic explanation of an Integrated Assessment Model for a lay reader. It was not intended to be the detailed "wiring diagram" of any specific IAM.

Q. Professor Tol stated: "Dr. Hanemann's [sic] further confuses 'equilibrium warming' (shown in his Equation (2), Hanemann Direct at 28: 13) and 'transient warming' (used in DICE, PAGE and FUND). This is a basic error. 'Equilibrium warming' refers to equilibrated warming . . ."[35] Do you agree with this statement?

A. No. I am perfectly aware of the distinction between equilibrium and transient warming. However, I deliberately omitted that distinction because this text was intended as a simplified explanation for a lay reader.

[34] Peabody Ex.__ at RT-2, page 3: lines 38-40 (Tol Rebuttal).
[35] Peabody Ex.__ at RT-2, page 3: lines 42-45 (Tol Rebuttal).

B. THE AVAILABILITY OF FUND

Q. Professor Tol stated: "Dr. Hanemann claims that FUND is not 'readily available.' (Hanemann Direct at 65: 1-8.) This is false. FUND has been in the public domain since 1999; . . .."[36] Do you agree with this statement?

A. No. I did not deny that FUND was in the public domain. I stated that it is not *readily* available, an important distinction. By that I meant it was not easily accessible and available for an outsider to run, let alone modify FUND in the same way as many researchers have been able to access, modify, and run DICE.

Q. Can you provide further clarification to your statement regarding the accessibility of FUND?

A. Yes. The following warning is posted on the FUND web site:[37]

> It is the developer's firm belief that most researchers should be locked away in an ivory tower. Models are often quite useless in unexperienced hands, and sometimes misleading. No one is smart enough to master in a short period what took someone else years to develop. Not-understood models are irrelevant, half-understood models, treacherous, and misunderstood models dangerous.
>
> Therefore, FUND does not have a pretty interface, and you will have to make to (sic) real effort to let it do something, let alone to let it do something new. If you want to give it a try, you can download selected versions of the model. You will need TurboPascal 7.0 for DOS to operate version 2.9 and below. This programme is no longer on the market, but you can purchase Delphi by Borland instead. For version 3.0-3.4, you need MS Visual Studio 2008 and Oxygene/Chrome/ Prism.
>
> For version 3.5 and higher you can run FUND with the free Visual C+ Express Edition.

[36] Peabody Ex.__ at RT-2, page 3: lines 49-50 (Tol Rebuttal).
[37] http://www.fund-model.org/ accessed on September 5, 2015.

Likely due to these challenges of accessing and running FUND, very few people have opted to "give it a try." In fact, prior to the IWG studies, I am aware of only one occasion where a journal article using FUND was published that did not involve the participation of the model developers (Professor Tol or Professor Anthoff). That particular instance led to a public dispute as to whether or not there was a programming error in the FUND code.

C. THE IWG'S USE OF THE FUND MODEL

Q. Professor Tol stated:

Even FUND's own numbers were above any offered by Peabody.

> **In 2011, FUND estimated a social cost of carbon of $8.0/tC; in 2014, was $6.6/tC. In other words, FUND as used by the FUND team shows a *lower* social cost of carbon, whereas FUND as used by US Federal Government shows a *higher* social cost of carbon.[38]**

Do you accept this as a criticism of the IWG study?

A. No. When the IWG ran FUND, it used different drivers of emissions than those native to FUND. As I explained in DOC Ex.___ at 66 (Hanemann Direct), this was done in order to put the three models on a common footing and to make them more comparable. Standardizing the external model inputs is the conventional practice in model inter-comparison exercises. In my opinion, it would have been unreasonable if the IWG had *not* done this.

[38] Peabody Ex.__ at RT-2, page 6: lines 120-123 (Tol Rebuttal).

D. DISCOUNTING IN THE IWG SCC

Q. Professor Tol made a number of statements regarding the Ramsey rule for discounting, as follows:

> **Dr. Hanemann defends the discount rates used by the IWG. I disagree. The Ramsey rule is a more appropriate choice.[39]**
>
> **According to the Ramsey rule, the discount rate should vary with economic growth. The Ramsey rule makes sense because it relates the money discount rate to parameters underlying the "time value" of money.[40]**
>
> **Furthermore, because we expect our income to grow, a dollar gain today is worth more than a dollar gain in a year from now, because the relative gain in income is greater now than later.[41]**
>
> **The IWG used real discount rates of 2.5%, 3.0% and 5.0% and did not use the Ramsey rule, which had an effect on its analysis.[42]**
>
> **The Ramsey rule implies that future impacts are more heavily discounted in more rapidly growing economies.[43]**

Do you agree with those statements?

A. No.

Professor Tol correctly characterized the Ramsey rule. Where I disagree with him concerns the validity of the standard formulation of Ramsey rule in the present context. As I summarized above in Table 2 in my response to Mendelsohn's Rebuttal Testimony, the Ramsey rule formulation which Professor Tol invoked makes certain assumptions that I find implausible, namely:

- What people expect out of life does not change over centuries.

[39] Peabody Ex.__ at RT-2, page 3, lines 52-53 (Tol Rebuttal).
[40] Peabody Ex.__ at RT-2, page 4, lines 56-58 (Tol Rebuttal).
[41] Peabody Ex.__ at RT-2, page 4, lines 65-67 (Tol Rebuttal).
[42] Peabody Ex.__ at RT-2, page 4, lines 76-77 (Tol Rebuttal).
[43] Peabody Ex.__ at RT-2, page 6, lines 103-104 (Tol Rebuttal).

- Society cares only about GDP; all the impacts of climate change can be reduced to the equivalent of a reduction in the production of manufactured goods.
- Outcomes are discounted at the same rate regardless of the span of time involved.
- There should be no allowance for risk aversion separate from intertemporal preferences.

As I explained in my response to Mendelsohn's Rebuttal Testimony in connection with Table 2, if one or more of these assumptions are set aside, it changes the formulation of the Ramsey rule and leads to a lower discount rate than what Professor Tol advocated.

IV. RESPONSE TO MR. MARTIN

A. USE OF THE IWG SCC FOR RESOURCE PLANNING

Q. Mr. Martin stated in his Rebuttal Testimony: "Sixth, several Parties mention that the Federal SCC *was designed for the specific purpose* of regulatory impact analysis and not for use in integrated resource planning."[44] Further on, he also makes a similar statement:

> **I noted in my Direct Testimony that the SCC is designed for a specific, limited purpose: federal regulatory impact analysis under Executive Order 12866. It is intended to help evaluate whether the benefits of a proposed federal regulation outweigh its costs.[45]**

Do you agree with these statements?

A. No. As I explained in DOC Ex.___ at 17 (Hanemann Rebuttal):

[44] Xcel Ex. __ at 10 (Martin Rebuttal).
[45] Xcel Ex. __ at 20 (Martin Rebuttal).

> Resource planning is a form of cost-effectiveness analysis. A cost-effectiveness analysis seeks to identify the least cost means of achieving a given target or goal. In turn, a cost-effectiveness analysis is a particular type of cost-benefit analysis where the alternatives all have the same benefit. In that case, maximizing the net benefit (the object of a cost-benefit analysis) is equivalent to minimizing the cost (the object of cost-effectiveness analysis).

B. CREATION OF A NEW IAM

Q. Mr. Martin stated:

> **Dr. Hanemann does not address the possibility that the IWG could have used the temperature change results from the EMF-22 IAMs as inputs to DICE, FUND and PAGE, and only used the latter to calculate economic damages.[46]**

Do you agree with that statement?

A. Only in part. It is correct that I did not address that possibility. But, the reason I did not is that I do not think it would have been easy or desirable for the IWG to have done what Mr. Martin suggests. To do this would be tantamount to creating a new (hybrid) IAM.

Q. Why would you not recommend creating a new hybrid IAM?

A. For the IWG to rely on a new model, not previously published in peer-reviewed literature, would be questionable. Moreover, combining the damage functions from DICE, PAGE and FUND with the carbon cycles of IMAGE, MERGE, MESSAGE and MiniCAM would have been computationally challenging. And, had the IWG done this, I am not sure that it could readily have standardized the stochastic treatment of the climate sensitivity parameter across the hybrid models.

[46] Xcel Ex. __ at 18 (Martin Rebuttal).

C. ADAPTATION AND TECHNOLOGICAL CHANGE

Q. With regard to Professor Polasky, Mr. Martin stated:

> **Professor Polasky discusses why the IAMs' omission of some damages and incomplete modeling of possible catastrophic damages could lead them to underestimate the value of the SCC, but he does not mention a significant counterbalancing omission that the IAMs incompletely model adaptation to climate change and do not incorporate any endogenous technological change at all.[47]**

Mr. Martin made a similar point with regard to your Rebuttal Testimony, referring to the 95-percentile value of the SCC "which captures some uncertainty regarding 'tipping point' damages, but not the counterbalancing uncertainty regarding adaptation and technological change."[48] Do you agree with these statements?

A. To a limited degree. I agree that some degree of adaptation and endogenous technological change will occur in the future. The degree to which they will occur is unknown. Since it is unknown, I am not sure how it could be incorporated in IAMs. While some adaptation and technological change will occur, it would be wrong to assume that they will occur instantaneously and will be costless and 100% effective. In other words, even with adaptation and technological change, costs will be still be incurred due to the impacts of climate change. Moreover, while the uncertainty regarding adaptation and technological change offsets to some degree the uncertainty regarding catastrophic damages from climate change, I strongly doubt that it fully "counterbalances" the latter uncertainty.

[47] Xcel Ex. __ at 24 (Martin Rebuttal).
[48] Xcel Ex. __ at 22 (Martin Rebuttal).

Q. Similarly, Mr. Martin stated:

> **It is counterintuitive, and contrary to current evidence, to assume that future societies will take no action to scale up GHG mitigation and adaptation, despite experiencing severe climate damages. There is tremendous technical innovation occurring today to reduce the CO_2 intensity of energy, as well as governmental efforts at the state, federal and global scale to mitigate CO_2 emissions and adapt to climate change.[49]**

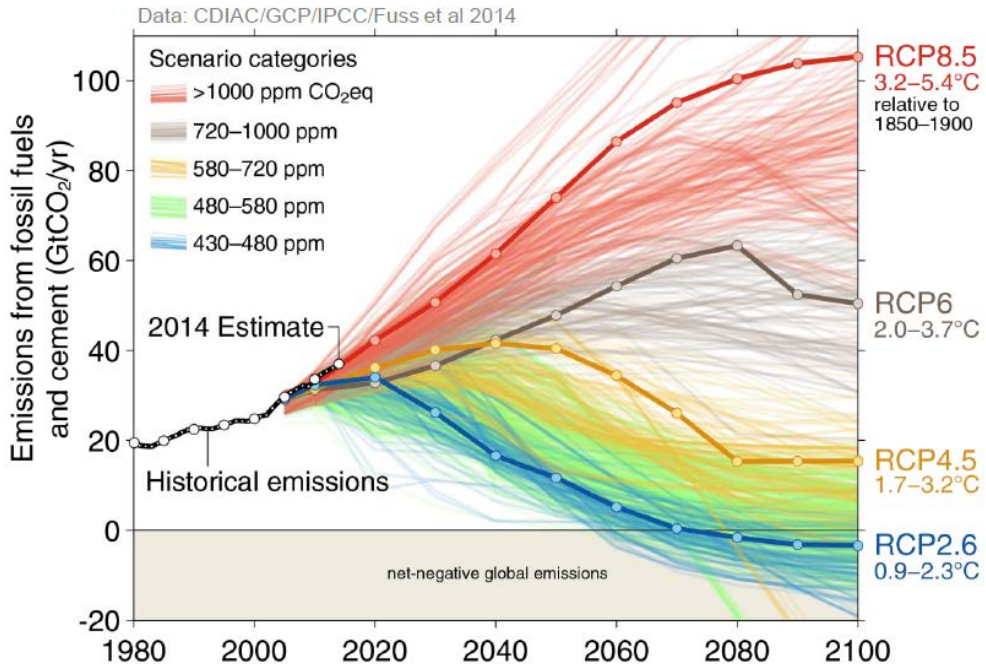
Do you agree with these statements?

A. To some degree. I agree that there is considerable technical innovation underway to reduce the CO_2 intensity of energy, and there are also significant governmental efforts to promote mitigation, such as the Clean Power Plan in the US. However, at least in the US, there is also significant political opposition to these governmental efforts, and the outcome is as yet unclear.

The thrust of Mr. Martin's remarks is the IWG's estimate of the SCC may be too high because it has not adequately accounted for future actions to reduce CO_2 emissions. In my view, it is premature to assert that conclusion. The fact is that global emissions have risen significantly over the past fifteen years. As shown in Figure 1 below, global emissions are currently on track to follow the highest of the four GHG concentration scenarios adopted by the IPCC for its Fifth Assessment Report. This does not mean that we might not move to lower emission and concentration scenarios later in this century. It is just that Mr. Martin's confidence in that outcome is, as yet, premature.

[49] Xcel Ex. __ at Page 48, lines 17-22 (Martin Rebuttal).

Figure 1- Observed Emissions and Emissions Scenarios



Q. What does the black line show in Figure 1?

A. The black line shows historical emissions from 1980, when the diagram commences, through 2014.

Q. What do the colored lines show?

A. The faint lines in the diagram are projections of emissions under various scenarios coming from 31 IAMs. Each faint line is a particular scenario. The IAMs generating the scenarios were what I called in my Direct Testimony IAMs of the second type:[50] they trace the link from economic activity to changes in climate but not the link from changes in climate to impacts and external costs. The emissions scenarios were assembled by Working Group III for the Fifth Assessment Report. The database contains over 1000 scenarios that met the criteria set for acceptability. The emissions scenarios start in 2010. The majority of the scenarios (about 95%) were

[50] DOC Ex.__ at 29 (Hanemann Direct).

generated as part of nine model inter-comparison exercises, of which one was the EMF-22 exercise.

The scenarios fall into two groups: those in which emissions were unconstrained ("baseline" scenarios) and scenarios in which emissions were constrained to meet some target level, typically in 2100 – as was the case with the EMF-22 exercise. The scenarios were also classified in a second manner. All the scenarios were run through a single climate model to determine, in a comparable manner, the CO_2-equivalent atmospheric concentration in 2100 associated with the scenario. The scenarios were classified into five groups corresponding to five ranges of CO_2–equivalent values. The five colors, from blue to red, represent this classification. The four heavy colored lines show the four scenarios of anthropogenic forcings ("representative concentration pathways" or RCPs) used by Working Group 1 when running climate models to simulate future climate outcomes. These heavy lines are colored to indicate the range of 2100 CO_2–equivalent concentrations to which it most closely corresponds. The red scenarios generally correspond to baseline emission scenarios.

What the figure shows is that, as of 2014, the actual trajectory of global emissions corresponds most closely to the highest RCP scenario, which is similar to a baseline (unconstrained) trajectory.

As I stated in DOC Ex.___ at 26, "As time passes, we will know more about the likely trend of emissions during the coming decades. That information can – and should – be used to update future estimates of the SCC."

D. PROJECTING FUTURE IMPACTS OF EMISSIONS

Q. Mr. Martin states:

> **[A]ttempting to model climate damages and societal response out to the year 2300 is equivalent to scientists in the early 1700s attempting to model our society today. It is similarly difficult for us to imagine what technologies may be available in the year 2300, and how societies may innovate to reduce CO_2 emissions in response to climate change.[51]**

Is that a good characterization of what is involved in future projections of climate impacts and mitigation costs?

A. Not with regard to climate impacts. In May of this year, the National Oceanic and Atmospheric Administration (NOAA), announced that the monthly global average concentration of CO_2 in the atmosphere exceeded 400 parts per million (ppm). The atmospheric concentration of CO_2 is estimated to have been about 280 ppm prior to 1800, the start of the industrial revolution. It rose to about 290 ppm in 1900. The last time the Earth had this much CO_2 in the atmosphere was several million years ago,[52] before *Homo sapiens* existed on the planet. The likely climate outcomes are unprecedented in human history.

E. MEDIAN VS. MEAN AND DATA TRIMMING IN ESTABLISHING THE SCC VALUES

Q. Mr. Martin stated: "Our approach recognizes the inherent uncertainty and applies well accepted statistical methods to manage that uncertainty by excluding both low and high outlier values that have a low probability of occurring."[53] Can you re-state

[51] Xcel Ex. __ at Page 25, lines 3- 8 (Martin Rebuttal).
[52] National Research Council, *Understanding Earth's Deep Past*, National Academies Press, Washington, D.C., 2011 Figure 2.2.
[53] Xcel Ex. __ at 3 (Martin Rebuttal).

Mr. Martin's initial proposition from his Direct Testimony, and do you agree that these are well-accepted statistical methods?

A. In his Direct Testimony, Mr. Martin argued against the IWG's estimate of the SCC, which used as a point estimate the mean value of the SCC across the probability distribution of SCC values associated with a 3% discount rate. Instead, he proposed a range of SCC values, based on the 25-percentile and 75-percentile values of the IWG's probability distributions of SCC values for the 2.5% and 5% discount rates. For each discount rate, the IWG's probability distribution of SCC values was examined, and the 25-percentile and 75-percentile values were identified. The three 25-percentile values were averaged across the three discount rates to produce his proposed value for the lower end of the range of SCC values, and the three 75-percentile values were averaged across the three discount rates to produce his proposed value for the upper end of the range of SCC values.

As to whether I agree if these are well accepted statistical methods, no I do not.

Using the inter-quartile range (the range of values from the 25-percetile to the 75-percentile) is an example of what is known in statistics as data trimming.

Trimming is applied when the extreme values of the data are regarded as outliers, which is how Mr. Martin characterized them in his Direct Testimony.[54] However, I rejected that characterization in my Rebuttal Testimony:[55]

> In my view, "outlier" is the wrong term for what is going on in Figure 9 in Xcel Ex. ___ at 65 (Martin Direct) … In statistics, an outlier is an observation that is distant from other observations. What we have here, however, is a continuum of observations with increasingly large values. In this case, it is not that there are outlier values

[54] See, for example, Xcel Ex. __ at 27 (Martin Direct).
[55] DOC Ex.__ at 67 (Hanemann Rebuttal).

> of the SCC. It is that the distribution of SCC values is skewed with a long right tail. A non-normal distribution (in this case positively skewed), will intrinsically include data points that are much larger than others in the same population.

When the extreme values are a legitimate part of the distribution rather being external to it (for example, because of measurement error), trimming is *not* appropriate: it excludes data points that really belong in the distribution.

As I noted in my Rebuttal Testimony, catastrophic but low probability outcomes are of the essence in climate mitigation decisions.

> [W]hat is at [sic] involved here is essentially a matter of risk management – regulating GHG emissions so as to avoid the risk of possibly very harmful climatic outcomes in the right tail of the warming and SCC probability distributions.[56]

Excluding those outcomes via trimming is inappropriate and counterproductive in a risk management context.

Q. Mr. Martin stated: "The 95th percentile represents an unreasonably low level of risk tolerance."[57] Do you agree?

A. No. As I noted in my Rebuttal Testimony, in other regulatory contexts involving low risk but potentially catastrophic outcomes it is common to focus attention on events that can occur with as little as 5% probability.[58] I quoted an analogy from a recent report from the Economist Intelligence Unit, as follows:[59] "We wouldn't get on a plane if there was a 5% chance of the plane crashing, but we're treating the climate with that same level of risk in a very offhand, complacent way."

[56] DOC Ex.___ at 70 (Hanemann Rebuttal).
[57] Xcel Ex. __ at 17 (Martin Rebuttal).
[58] DOC Ex.___ at 70 (Hanemann Rebuttal).
[59] DOC Ex.___ at 71 (Hanemann Rebuttal).

Q. Mr. Martin stated:" [T]he average is a poor indicator of central tendency for a non-normal, heavily skewed probability distribution such as the SCC."[60] Do you agree with this statement?

A. No. In DOC Ex.___ at 69-70 (Hanemann Rebuttal), I cited the IWG's justification for using the mean rather the median of the distribution of SCC values.[61] The IWG stated that, in the climate change context, "sound decision-making requires consideration of not only the typical or most likely outcomes, but also less likely outcomes that could have a very large (or small, or even negative) damage (the tails of the distribution)." I concurred with the IWG's conclusion that, in this case, the mean is the appropriate measure of central tendency.

F. AREAS OF AGREEMENT

Q. Did Mr. Martin make some statements in his Rebuttal Testimony with which you do agree?

A. Yes, there are several areas where I agree with him.

I agree with his rejection of the range of SCC values proposed by Dr. Smith in her Direct Testimony. For 2020, that range was from $1.62 to $5.14 per net metric ton. He stated:

> In the event that the Commission retains a focus on global damages, a range this low and narrow would not capture much of the inherent uncertainty, and would not, in my view, reflect an appropriate level of risk tolerance.[62]

[60] Xcel Ex. __ at 15 (Martin Rebuttal). The statement is repeated at Xcel Ex. __ at 16 (Martin Rebuttal).
[61] Interagency Working Group, *Response to Comments: Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866* (July 2015, p. 26).
[62] Xcel Ex. __ at 29 (Martin Rebuttal).

He observed that "Peabody's experts make various and apparently conflicting recommendations."[63] I agree.

With regard to Professor Mendelsohn's claim in his Direct Testimony that the optimal global temperature is 1.5 °C to 2 °C higher than average temperature in 1900, on the basis of which he modified the DICE damage function, Mr. Martin stated that this claim rests "on vague allusions to what 'the research' indicated … but the evidence he presents is anecdotal rather than comprehensive, so difficult to assess."[64] I agree.

Mr. Martin stated:

> I do believe the low, tight SCC ranges recommended by Dr. Mendelsohn – even if they capture the possibility that climate damages could be lower than predicted by the IWG – do not adequately capture the inherent uncertainty in predicting climate damages, and imply an inappropriately high level of risk tolerance.[65]
>
> I do not believe that climate damages are likely to be lower than predicted by the SCC, but I do agree that the range of SCC values recommended by Professor Mendelsohn implies an inappropriately high tolerance of risk.

Mr. Martin disagreed with Dr. Smith's recommendation to discard the 2.5% discount rate.[66] I agree with him on that point.

[63] Xcel Ex. __ at 33 (Martin Rebuttal).
[64] Xcel Ex. __ at 34 (Martin Rebuttal).
[65] Xcel Ex. __ at 35, lines 11-15 (Martin Rebuttal).
[66] Xcel Ex. __ at 42, lines 14-16 (Martin Rebuttal).

V. RESPONSE TO DR. SMITH

A. CLIMATE DAMAGES IN THE IWG SCC

Q. Dr. Smith stated:

> **The integrated assessment models that are being used to calculate SCC estimates contain highly aggregated damage functions that are not fully consistent with the standard notion of the damage cost approach, and it is this aggregated approach that lacks evidentiary basis.[67]**

Do you agree with this statement?

A. No. I addressed this issue in DOC Ex.___ at 39-40 (Hanemann Rebuttal), where I stated my opinion that "it is not reasonable to expect the use of conventional dose-response functions on the spatial and temporal scales required for an IAM damage function."

Q. Dr. Smith stated:

> **[T]he IWG's framing approach does not reasonably address the inherent uncertainties of estimating societal values over 300 years and relies on fixed assumptions about societal values that are highly speculative.[68]**

She argued that the valuation of damages should not be extended out to 2300 and should be terminated sooner because the assessment of those damages is "highly speculative." Do you agree?

A. No. Obviously, there is uncertainty regarding how people will value climate impacts 300 years from now – for example, how people will react if sea levels rise by several meters and threaten to inundate coastal areas, etc. However, that is not a valid reason to truncate the analysis to the time period before such impacts occur.

[67] GRE, MP, OTP, MLIG Ex. ___ at AES-D2 page 4 (Smith Rebuttal).
[68] GRE, MP, OTP, MLIG Ex. ___ at AES-D2 page 5 (Smith Rebuttal).

Moreover, it is not the major area of uncertainty. The larger consideration is that the degree of warming being projected by climate models after 2100 was last experienced by this planet millions of years ago,[69] before *Homo sapiens* existed. Since these climate outcomes are unprecedented in human history, there are no historical data to which the social and economic impacts could be calibrated. The lack of calibration, however, is not a valid reason to ignore such unprecedented climate risks.

VI. RESPONSE TO PROFESSOR POLASKY

Q. Are there specific points in Professor Polasky's Rebuttal Testimony with which you agree?

A. Yes, there are many points with which I agree. Below I highlight the following areas of agreement.

Professor Polasky noted that, in my Direct Testimony, I had appeared to suggest that the IWG's estimate of the SCC is based on damages occurring through 2100 rather than 2300.[70] That was a typographical error, of which there were several in my Direct and Rebuttal Testimonies. I have prepared a listing below, that corrects those errors.

[69] Hansen *et al*., "Climate Sensitivity, Sea Level and Atmospheric Carbon Dioxide," *Philosophical Transactions of the Royal Society A*, 371:20120294, Figure 4.
[70] CEO Ex.___ at 3 (Polasky Rebuttal).

A. Uncertainty in the IWG SCC.

With regard to Dr. Smith's analysis, I agree with Professor Polasky's statement:

> Uncertainly in assessing the social cost of carbon cannot be avoided. It is not valid to say that because uncertainty is large that attempts to deal with it are excessively speculative. It is also not valid to conclude that the proper response to large uncertainty is to just ignore it.[71]

B. Projections of Future Emissions.

I agree with Professor Polasky's statement: "For the time being, I believe that the IWG emission projections best reflect the current understanding of the likely potential trajectories of future emissions.[72]

C. Measurement of Damages.

I agree with Professor Polasky's statement:

> Smith correctly identifies an area of uncertainty, predicting damages from CO_2 emissions for hundreds of years, but the proposed solution, namely to assume there will be no impacts far into the future, has no bearing in reality.[73]

I agree with Professor Polasky's statement: "However, uncertainty is not an excuse for assuming that the damages from warming above 3 degrees Celsius are zero."[74]

I agree with Professor Polasky's statement: "Using Smith's shorted time horizon would ignore damages from current emissions that occur far into the future. There is no justification for this approach."[75]

[71] CEO Ex.___ at 6 (Polasky Rebuttal).
[72] CEO Ex.___ at 12 (Polasky Rebuttal)
[73] CEO Ex.___ at 16 (Polasky Rebuttal).
[74] CEO Ex.___ at 17 (Polasky Rebuttal).
[75] CEO Ex.___ at 20 (Polasky Rebuttal).

D. Discount Rates in the IWG SCC.

I agree with Professor Polasky's statement: "I disagree with the specific suggestion of not including the 2.5 percent discount rate. I also disagree with the suggestion of including rates higher than 5 percent."[76]

I agree with the following testimony by Professor Polasky, where he quotes Gollier and Weitzman's statements that endorse what I referred to as hyperbolic discounting in DOC Ex.___ at 79-83 (Hanemann Rebuttal):

> Christian Gollier and Martin Weitzman in a 2010 paper entitled "How should the distant future be discounted when discount rates are uncertain?" stated that "there exists a rigorous generic argument that the future should be discounted at a declining rate that approaches asymptotically its lowest possible value"(p. 351). Given the considerable uncertainty about future economic growth especially under climate change, a strong argument for discount rates lower than 2.5 percent can be made.[77]

E. Credibility of Dr. Smith's Approach.

I agree with Professor Polasky's statement:

> I note that the five concerns Smith identifies and her recommendation with regard to each have the effect of lowering the value of the SCC. Smith ignores concerns that others have raised that would suggest the possibility that the SCC is too low. Smith's testimony does not try to provide an unbiased account of the potential problems of the SCC.[78]

He continues: "Smith's approach is not credible."[79] I agree.

[76] CEO Ex.___ at 21 (Polasky Rebuttal).
[77] CEO Ex.___ at 21-22 (Polasky Rebuttal).
[78] CEO Ex.___ at 30 (Polasky Rebuttal).
[79] CEO Ex.___ at 31 (Polasky Rebuttal).

Q. Are there other areas where you agree with Professor Polasky's assessment of other witnesses' testimony?

A. Yes. In referring to areas of agreement with Mr. Martin, I agree with Professor Polasky's statement on discount rates as follows:

> I agree with Martin that the approach used by the IWG and the resulting probability distributions for each discount rate, and use of a range of different discount rates, constitutes a reasonable and best available set of information on which the Commission can proceed with its task of establishing an externality cost for CO_2 emissions.[80]

F. IWG SCC Use of Mean Value

At several points in his discussion of Mr. Martin's analysis of the IWG's distributions of SCC values, Professor Polasky stated that he considers the use of the mean more appropriate than the median in the present context.[81] I agree.

G. Projections of Future Emissions.

With regard to Dr. Smith's argument that IWG should have calculated the SCC based on an optimal level of emission reduction, I agree with Professor Polasky's statement:

> Smith argues that the IWG should use emission projections assuming an optimal level of future CO_2 emissions. An optimal emission projection assumes that there is a global climate policy in place that equates the marginal cost of reducing emission with the SCC and determines the quantity of emissions that would result from this policy. As is clear to any observer of climate change policy, we do not have such policy in place now, nor is there any guarantee that we will have such policy in the future.[82]

[80] CEO Ex.___ at 32 (Polasky Rebuttal).
[81] CEO Ex.___ at 34-42 (Polasky Rebuttal)
[82] CEO Ex.___ at 11 (Polasky Rebuttal)

With regard to Professor Mendelsohn's similar claim, I agree with Professor Polasky's statement: "[W]e do not currently have such policy and getting to such a policy does not appear likely any time soon."[83]

H. Inclusion of PAGE, FUND, and DICE IAMs in the IWG SCC.

With regard to Professor Mendelsohn's assertion that PAGE should not have been used by the IWG, I agree with Professor Polasky's statement:

> I disagree. The PAGE model along with the FUND and DICE model are the three most prominent economic climate change IAMs. The inclusion of all three models is appropriate and preferred to selecting only one or two models. There is uncertainty regarding several aspects of climate change. The best method to approach estimating the SCC is incorporating several different methodologies and sets of assumptions.[84]

VII. CORRECTIONS

Q. Are there corrections that you would now like to note regarding your previous Direct and Rebuttal Testimony?

A. Yes. Below is a table of corrections each to DOC Ex.___ (Hanemann Direct) and DOC Ex.___ (Hanemann Rebuttal):

Table 3: Corrections to DOC Ex.___ (Hanemann Direct)

Location	Correction
Page 26, Line 6	~~continuing at least through 2100~~ continuing through 2300
Page 26, Line 12-13	~~typically reported through about 2100~~ reported through 2300
Page 35, Line 3	~~between 2060 and 2100~~ after 2060
Page 36, Figure 2	In the row labeled 'Time Steps' and the column for 'Page': ~~10 year steps (2000 2006)~~ 10 year steps (2000-2060); and ~~20 year steps (2060 2100~~ 20 year steps (2060-2300)
Page 53, Line 7	~~2100~~ 2300

[83] CEO Ex.___ at 46 (Polasky Rebuttal)
[84] CEO Ex.___ at 48 (Polasky Rebuttal)

Table 4: Corrections to DOC Ex.___ (Hanemann Rebuttal)

Location	Correction
Page 3	~~Peabody Ex.__ at 9 (Bezdek Direct)~~ Peabody Ex.__ RHB-2, page 9 (Bezdek Direct)
Page 27	~~VI.~~ VII. WHETHER SCC ESTIMATES SHOULD BE BASED ON THE "FIRST TON" OR THE "LAST TON" OF CO2 EMISSIONS?
Page 29	~~VII.~~ VIII. THE RELEVANCE OF LEAKAGE FOR APPLYING A SCC.
Page 31	~~VIII.~~ IX. THE UNCERTAINTY REGARDING EQUILIBRIUM CLIMATE SENSITIVITY
Page 34	~~IX.~~ X. CRITICISMS OF THE IAM DAMAGE FUNCTIONS
Page 38	~~X.~~ XI. THE VALIDITY OF CRITICISMS OF THE IAM DAMAGE FUNCTIONS
Page 63	~~XI.~~ XII. CATASTROPHIC OUTCOMES
Page 66	~~XII.~~ XIII. USE OF THE MEAN VERSUS THE MEDIAN ESTIMATE OF THE SCC
Page 71	~~XIII.~~ XIV. DISCOUNTING
Page 87	~~XIV.~~ XV. SUGGESTED RANGE OF SCC VALUES

Q. Does this conclude your testimony?

A. Yes.